Recent Research in Psychology

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Editors

Recent Trends in Theoretical Psychology
Volume II

Proceedings of the Third Biennial Conference of the
International Society for Theoretical Psychology
April 17–21, 1989

With 23 Illustrations

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PREFACE

This volume constitutes the proceedings of the third biennial conference of the International Society for Theoretical Psychology, held in Arnhem, the Netherlands, April 17-21, 1989. Fifty-six papers were presented during the four days of the conference, including an invited address by Professor A. D. de Groot, and seven papers composing two plenary sessions, four on the *contribution of history to theory*, and three on *theoretical alternatives for contemporary psychology*. Of these, 46 papers are presented in the proceedings; all of which suffered editorial changes and, with the exception of the invited address, were required to meet a 15 page restriction on length. The editors gratefully acknowledge John Mills, Leendert Mos, and Hank Stam for their invaluable editorial assistance.

The papers included here are presented without discussants' commentaries. (Over 125 psychologists participated at the conference.) While the papers are representative of the scope of topics covered at the conference, the hours of formal and informal discussions must, unfortunately, be left to the reader's imagination. We encourage the reader to attend one of our next conferences, planned biennially, and alternately, in North America (1991) and Europe (1993).

Classification of conference papers serve to highlight close connections among various papers, but inevitably there are more cross references than can be shown even in the best of all possible classifications. The editors decided to structure the proceedings using four headings: (1) philosophy and metatheory of psychology; (2) theoretical psychology; (3) historical analysis of psychological theory; and (4) advancement of substantive theory. *Philosophy and metatheory of psychology* includes foundational discussions about the nature of psychological theorizing, explanation, and methodology. *Theoretical psychology* concerns reflections on extant theories and discussions of fundamental questions within more specialized domains of psychology. *Historical analyses of psychological theory* refers to various and diverse attempts to locate the historical roots of current theories, or else to demonstrate the relevance of historical episodes for current theorizing. *Advancement of substantive theory* includes papers that propose new hypotheses, theories, and new ways of theorizing about psychological phenomena.

Seventeen papers are included under the heading *philosophy and metatheory of psychology*. **Adriaan de Groot,** in his invited address, looks to the preconditions required to improve the discipline's internal consistency. The goal of unifying psychological studies, which he deems a scientific necessity, can be enhanced by means of concept analysis and the formation of consensus groups, both of which are exemplified in his contribution. The next three papers compose the plenary session on *theoretical alternatives for*

contemporary psychology. **Giorgi** raises the question as to the appropriate cultural institutionalization for psychology and sketches his vision from a phenomenological perspective. **Tolman,** examining the assumptions common to our everyday existence, argues for philosophical materialism as the basis for psychology. **Kenneth Gergen,** in contrast to his phenomenological and materialist protagonists on the panel, presents a social constructionist point of view. Preferring to speak of multiple realities, he explores the limits of our conception of the real and our justification for our choice of grounding ontology.

The next thirteen papers are concerned with the nature of explanation or the assumptions underlying methodology in psychology. **Shotter** claims that psychology is not a natural but a moral science. Ideas begin not in the head, but in bodily activity and, hence, psychological explanation must find its starting point in the actions and conversations of everyday life, not in the abstract principles of mind. **Mos and Boodt** aim to explicate the nature of psychological explanation. They argue that explanation is fundamentally theoretical in nature, only bounded by historical-cultural life forms and, therefore, that psychology is inescapably a hermeneutic endeavor. **Vollmer** critically examines the claims of eliminative materialism as the latest and most radical version of scientism to be imposed on the human and social sciences. **Stam,** examining the impact of psychology on culture, concludes that ordinary language explanations are more likely to change people's self-understanding than psychological explanations. Psychological explanations must recognize that mental events are embedded in the discursive practices of a human community that shares linguistic and cultural practices. **Widdershoven** proposes a narrative theory of action, from within which actions are viewed as meaningful expressions of an integrated life story. The case of addiction is chosen as an example. **Schwanenberg** uses the concepts of 'test of probability' and 'test of meaning' to distinguish between positivistic and hermeneutic orientations towards the subject matter of psychology. **Looren de Jong** discusses the notions of naturalism and intentionality, and maintains that the former does not, contrary to some, entail reductionism. He sketches a non-reductionistic conception of an intentional psychology which, nevertheless, views the mind as the product of organism-environment interaction. **Boodt and Mos** attempt to understand the nature of the 'psychological' from the perspective of the 'social'. From a hermeneutical perspective, these authors claim, that social-psychological reality can be interpreted as a dialogical relationship between individual-psychological and social-cultural 'prejudices'. From a feminist, social constructionist perspective, **Mary Gergen** pointing to the restrictions of traditional objectivistic theorizing, submits that as psychologists we are responsible for the theories we create and the values these encourage.

This section concludes with four papers that address some fundamental issues in methodology. **Smaling** presents his 'Münchhausen' conception of

objectivity in an attempt to reconcile post-positivistic approaches with their apparent abandonment of objectivity. **Shames** is critical of the discipline's adherence to an empiricist epistemology in its methodology and our neglect of constructionism, hermeneutics, and critical theory. The consequence of this negelect is an absence of coherent theory. **Wild, Schopflocher, and Kuiken** distinguish between empirical-inductivist and phenomenological-intuitionist modes of protocol analyses in an attempt to adequately identify linguistically expressed experience. In the final paper in this section, **Goudsmit** shows that in both 'inside' and 'outside' research traditions the distinction between object and method of investigation poses problems though for different reasons. He suggests how these problems can be exploited.

Under the second section of *theoretical psychology*, we have included eleven papers. **Van Geert** draws attention to the phenomena of psychological growth which he deems to be essentially unpredictable, and considers how developmental theory can take account of such phenomena. **Biesta, Miedema, and IJzendoorn** reconstruct John Dewey's reflex arc concept within a transactional paradigm in order to critically examine John Bowlby's attachment theory. In a paper concerned with cognitive development, **Boom** distinguished between operational and conceptual structures, and argues that both these have their place in a balanced developmental model. The next four papers have the social construction of the individual as their common subject. **Fischer** presents a social-cognitive view of emotions, rejecting the polarity between rationality and affect, and argues that emotional experiences are inherently cognitive, but not reducible to cognition. **Aebischer** argues that individual information processing is embedded in a social context, conforming to community rules, and yet must allow for individual differences. **Jansz** argues that the social cognition paradigm, the study of cognitive processes in social action, is burdened by an individualist ideology which must be replaced by a concept of the person as a truly social construction. Staying close to this theme, **Apfelbaum** uses new perspectives opened up by feminist research to argue that many so-called universal biological processes are in fact dependent upon sociological and historical beliefs. The next four papers deal with the compu-tational view of cognition and neuroscience. **Smythe** discusses some recent critiques of the computational theory of mind, and argues that mental representations function relative to the interpretative practices of a community. According to **Bem**, computationalists inflate internal representations to symbols in the mind and, instead, maintains that intentionality is not merely dependent upon language but also on our active involvement in the world. **Jorna** examines Stephen Kosslyn's theory of pictorial representation in the context of discussing constraints on the notions of symbol and representation. The final paper in this section by **Braun** takes issue with top-down approaches to cognition. He specifies the necessary and sufficient conditions for a behav-ioral neuroscience, that is, a bottom-up style of

theorizing about organized behavior contra Jerry Fodor and Zenon Pylyshyn's recent criticisms of the bottom-up approaches, such as connectionism.

Under the third heading of *historical analyses of psychological theory*, we have included thirteen papers. The first four papers compose the second plenary session on the *contribution of history to theory*. **Kurt Danziger** points out that the empirical domains about which psychologists theorize are not natural phenomena, but carefully constructed products of psychological practices. Psychologists tend to legitimize their activities in terms of the natural science model and, thereby, deny the historicity of their theories. **Van Strien** is also concerned with the a-historical character of our theories which aim to formulate general propositions. He proposes to recontextualize such theories by means of a reconstruction of the original problem situation. **Van Rappard** critically presents the social-historical and intellectual-historical (*Problemgeschichte*) approaches to the historiography of psychology. If theoretical psychology is viewed as metapsychology, then the intellectual-historical approach to history may be viewed as part of theoretical psychology. **Scheerer** analyzes the rhetorical use of selected concepts in German psychology during the Weimer Republic.

The remaining nine papers all deal with historical approaches to selected psychological theories. **Mills** interprets Clark Hull's theory of value as an example of Jurgen Habermas' conception of purposive-rational action. **Lubek** describes the interactionist social-psychological perspective of the turn-of- the-century thinker Gabriel Tarde, with a view towards understanding the lack of institutional support for interactionist theory in sociology and psychology. **Elbers**, in a complicated historical survey, compares the results of the behaviorist program of infancy research with originating in the ethological and cognitivist orientations. **Fireman and Kose,** in a comparative analysis of the early works of Jean Piaget and Lev Vygotsky, argue that an understanding of consciousness was an essential feature of both their works. **Maiers** critically examines the 'crises' as depicted by Karl Bühler and Vygotsky in an attempt to understand the pluralist and monist epistemic bases for psychology. **Van der Veer** reconstructs the historical background of the 'zone of proximal development', showing that this concept does not fit in well with Vygotsky's later work. **Droste** discusses various internalist and externalist factors which contributed to Sigmund Freud's rejection of the seduction hypothesis. **Panhuysen** demonstrates how heuristics leads theory construction by examining the biomedical origins of Freud's mechanization of the mind and his general theory of the psyche. **Hildebrandt**, completes this section, with an examination of the descriptive role of folk psychology in psychological theories, especially cognitive theories. While folk psychology does not provide a normative standard for the formulation of psychological theories, it does help to understand the historical development of contemporary theories.

Five papers have been included under the final heading of *advancement of substantive theory*. The first paper by **Michael Hyland** presents archeological evidence to argue that psychologically induced physical illness would have been functional during the Paleolithic as a mechanism for ensuring group co-operation. The psychological state — morbidity relationship ceased, however, to be biologically adaptive from Neolithic times up to the present. **Thorngate and Carroll** analyze the problems inherent in the use of adjudicated contests. They propose strategies to prevent fair contests from dissolving into unfair ones as the contestant population increases. **Borg** presents a general model for interindividual comparisons. He describes an empirical test of this model with respect to effort and exertion, concluding that it may have wider applications to most kinds of 'interprocess' comparisons. **Baker** sketches a theoretical perspective within which to understand human communication. He maintains that only by focusing on the mental activities of speakers and hearers, will we be able to develop a substantive theory of meaning and understanding. **Jan Smedslund,** in the final paper, expounds his notion of 'psychologic', namely, the explication and systematization of the implicit common sense psychology embedded in our everyday language. Psychologic permits us to clearly distinguish between conceptual and empirical relations, thus enabling researchers to avoid pseudo-empirical research.

In preparation of this volume, we gratefully acknowledge the assistance of Mrs. Valerie Welch for entering the text, and the Center for Advanced Study in Theoretical Psychology at the University of Alberta for providing the computer facilities for text preparation and printing of camera-ready copy.

The Editors
January, 1990

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UNIFYING PSYCHOLOGY:
ITS PRECONDITIONS

Adriaan D. de Groot

SUMMARY: It is argued that improving psychology's internal consistency is a scientific necessity. One precondition is that proponents of various 'scientific' and 'humanist' approaches come to agreement on mission and methodological standards of psychology as a *scientia* (= wetenschap — science). The resulting demarcation problem is solved, in principle, by *(Forum)* theoretical definitions of the 'scientific truth' and 'scientific import' of statements. On this basis, *(signific)* concept analysis can provide agreeable definitions of basic concepts, to be proposed for (modification and) actual agreement in *consensus* groups. It is concluded that these laborious 'scientia'-activities are badly needed and instrumental in serving the goal of unifying psychology.

Introduction

During the eighties a number of publications have been devoted to 'psychology's fragmentation' and to the question of whether something is to be done about it; and if so, how.[1] Several initiatives have been taken by proponents of an active strategy against fragmentation and for some sort of unification, such as the founding of the Society for Studying Unity Issues in Psychology (SUNI) in 1986.[2] Although I have not followed these activities in detail my educated guess is that there are two main problems this movement for unifying psychology is confronted with, one external and one internal. First: how to raise a general interest in unity problems? Second: how to unify the unifiers? Both problems clearly refer to preconditions of success. Without a reasonably general interest within a field a movement for unifying that field cannot succeed. But such general interest can be expected only if the unifiers know, and agree about, what they are after. Obviously, this second problem deserves priority. Proponents of unifying psychology had better agree, first, on the fragmentation *diagnosis*, and second, on the *therapy* they envisage — including the arguments for necessity and feasibility. To begin with, however, a few introductory remarks will be made on the first problem.

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[1] See, for example, Sarason (1981), Staats (1983), Fiske & Schweder (1986), Altman (1987), Spence (1987), Krantz (1987), Gergen (1988), and discussions in *New Ideas in Psychology* (1987) by Royce, Bakan, Toulmin, and Baer.

[2] See the volumes of the *International Newsletter of Uninomic Psychology*. Volume 6 (1988) contains 65 names of SUNI-members, including those of five persons with a non-English first language.

Raising a general interest in a vague and complex, theoretical problem area such as that of unity issues is, of course, difficult; in particular in psychology where strict empiricism on the one hand and specific theorizing on the other are traditionally proclaimed as the *summum bonum*. A particular explanation of a low general interest is hardly needed. It is worthwhile, however, to sketch a few different positions: motives and reasons, including rationalizations.

First, younger specialists involved in making a career for themselves, in particular if working in units that are part of some prospering mainstream, are not likely personally to feel the need (Krantz, 1987). Second, some of these same persons, and many others as well, appear to not even see the problem that unification is supposed to solve. I am referring to colleagues whose view of psychology as a science remains limited to the work of groups and persons with whom they, explicitly or implicitly, share certain restrictive postulates. These colleagues do not see serious schisms within 'scientific' psychology but solely one between what they consider as 'scientific work' and as 'nonscientific' and therefore, negligible work.

Third, among those who do see psychology as one, regrettably divided, if not 'chaotic', scientific field (Staats, 1984) many have soothed or rationalized their worries in various ways. One argument says that the present divergence still is an effect of psychology's youth as a science, implying that the problem will solve itself in due time, naturally; that is, by continued research and discussion, without particular unifying efforts.[3] This consolation is sometimes supported by the view that psychology's compartmentalization would be due mainly to a natural process of specialization in terms of subject matter — as it is observed in other fields as well. According to another argument psychology cannot be but a 'multifaceted' science because of the particular nature of its object; that is, we must just learn to live with not only its fragmented content but also its diversity of views, theories, schools of thought; and with a corresponding multitude of incommensurable languages. A radical version of this somewhat apathetic view amounts to praising and sanctioning the proliferation of methodological and terminological options by maintaining that pluralism is beautiful. This view, reminiscent of Feyerabend's "anything goes" (1975, 1978) or, for that matter, of Chairman Mao's "let a hundred flowers blossom and a hundred schools of thought contend" (1966, pp. 302-303), is indeed radical in the sense that it obliterates traditional principles of scientific methodology — in particular the economy principle. It is, however, highly conservative in its consequences. It says that nothing need be done, except muddling through in discord, as we are doing.

Finally, one may, of course, be uninterested in efforts at unifying psychology because of a prohibitive doubt as to its feasibility. Most often heard is the

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[3] See, for example, regarding intelligence, H. J. Eysenck (1988).

argument that the extant differences in view cannot be bridged because they are too deeply rooted in different philosophies of life, images of man, or, religious or political ideologies. Supporters of this position do not deny that the problem of bridging views is an empirical one but they deem it too unpromising to join the movement. A kindred argument is that the enterprise is hopeless because, and as long as, the proponents of unification appear grossly to underestimate its difficulty.

What is my own position? Being not only old but also a lifelong generalist, I do, of course, see the problem, and I do feel the need. I reject emphatically the soothing rationalizations by which the importance of the chaos problem is minimized or soft-pedaled, or even its existence denied. I think it is a shame that psychologists do not tend to stand on each "others' shoulders but rather to step in each others' faces" (David Zeaman, quoted in Bolles, 1975, p. 4), or, somewhat less harshly phrased, on one another's toes. However, my position is rather near to the last one mentioned. I agree with the pessimists to this extent that the feasibility of a successful unification operation is highly problematic, and that the obstacles to be overcome are generally underestimated. Even so, I am convinced that it must be attempted, because, being scientists, we cannot afford to linger forever in the limbo stage of what Bruno Latour aptly calls that of "science in the making".[4] The responsibility to work for the unification of psychology, or, phrased more modestly, *for the development of strategies by which the unnecessary divergence of school conceptions and terminologies can be curbed and the internal consistency of psychology improved*, rests heavily on our shoulders.

In this paper I shall not embark on a discussion of the contributions of other authors nor on historical causes of our dividedness.[5] I shall concentrate on the 'logic' of the problem, including the use of a few metatheoretical, epistemological and, in particular, linguistic-semantic considerations. In spite of these difficult words the argument will be very down-to-earth. It will focus on the conceptual analysis of a few basic concept terms; and it will lead to a recommended initial approach to the unification problem by which the preconditions can be met and the first steps taken.

How and What to Unify

In developing an overall strategy for improving the internal consistency of psychology the first task at hand is obvious. The objectives must be pinned down; and this must be done in such a way that we, serious proponents of unifying psychology, are likely to agree that this is an important part of what we are really after. Since even this primordial agreement cannot be assumed

[4] Contrasted to "ready made-science: (...) Janus Bifrons", Latour, 1987, p. 4.

[5] For a few remarks on the historical perspective see de Groot (1989).

to exist nor be expected to spring naturally from our traditional arena-type scientific discussions, this first task had better be phrased somewhat more precisely. Wanted is: a formulation of the objectives of the unifying enterprise on which, after *a process of explanation, discussion, deliberation, and negotiation*, general agreement is likely to be possible. Since the resulting logical product of opinions on the objectives must be as substantial as possible — not futile, let alone zero — a formulation is to be produced that seeks *the upper limits of the agreeable*; limits to be tested in the process of deliberation.

Although the objectives problem is far from the most difficult one to solve, I have specified the nature of the task in some detail because in this paper this type of task, and problem, will recur many times. It is typical for what the Signific Concept Analaysis approach (SCA) has been developed (de Groot & Medendorp, 1986 & 1988). One of the basic principles of SCA says: "Start from language"; that is, begin by taking words, concepts, and meanings seriously. The principle is self-evident. Valid agreements cannot but be laid down in language, in sentences in which the meaning of crucial words is reasonably agreed upon.

Therefore, what do we *mean* when we *say* that we want to work for 'unifying psychology', for studying 'unity issues', or for the ideal of a 'uninomic psychology'? This must be specified in a formulation, that is, in a few 'defining clauses' (1988, *op. cit.* section 3.4) that pin down *adequately* what we want, and expect to be able, to agree upon. Such adequacy implies, among other things, that these terms themselves should not give rise to misunderstandings and/or averse reactions — not solely so within the inner group but also among the broader audiences that are to be convinced. On this criterion some phrasings and particular words cannot be accepted. 'Uninomic psychology' is a case in point. It is a misnomer for three reasons. First, the word 'uninomic' is a so-called monstrum — half Latin, half Greek — that is likely to abhor sensitive erudites. Second, 'uninomic' means literally: 'one-lawish', suggesting an ideal of one monolithic deductive system for psychology as a whole. This is patently out of reach, not only for psychology but for any empirical science. Third, this being so, the label 'uninomic' is suggestive of a dogmatic new sect rather than of a meeting ground where different views are to be discussed freely, and negotiated for consensus.

It is true that in this respect the term 'unifying' is a little suspect, too. If associated with, for instance, the older ideal of 'unified science',[6] or, with that of unified theories in physics and, more recently, in cognitive science (Newell, 1987), it sounds overly pretentious. Moreover, and apart from the unrealistic end it suggests, namely of a unified psychology as-a-whole, it has the disadvantage of not excluding meanings such as those of the unification of the

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[6] In the 'Vienna Circle' program, and in particular Otto Neurath's project of the *International Encyclopedia of Unified Science*; cf. Joergensen's (1951) overview.

Roman Empire, or of mergers of firms, or coalitions of political parties. That is, it does not exclude unifying by overpowering, or by overruling. Let us be precise here. In the history of human *warfare* it has happened now and then that a victor succeeded in implanting his dogmas into the minds of the defeated population. In *commerce* victories of one 'concept' over another one, up to eliminating the latter's enterprise, occur quite often. In democratic *politics* the majority is supposed to have highly consequential rights; and a stronger, qualified majority is almost supposed to *be* right. But in the *scientific competition of ideas*, in spite of its increasingly warlike, commercialized, and democratized forms of our day, all these overruling procedures are out of place. They contradict the basic idea of a scientific intercourse in which only rational arguments count and the rules of the game — conventions — must be accepted generally.

In spite of these risks of misinterpretation, however, I have maintained the term 'unifying' — rather than, for instance, the vaguer concept of 'integrating'. The reason is that the risks of misunderstanding can be reduced substantially by appending what is called a *contextual specification* (1988, *op. cit.*, section 3.1) of the verb 'unify'. The first and most important steps consist of:

1) not speaking of 'unification' but of 'unifying' in its transitive meaning,

2) specifying its direct object as intended in our case, and

3) specifying the rationale of this intention.

As regards the question what we are intending to unify, (2), I assume that we are not, at least not primarily, attempting to unify a guild of professionals, called psychologists, nor the variegated collection of interesting study subjects, called psychological ones. Neither is the direct object the whole system of theories and facts, called scientific psychology. It is certain basics of psychology that we want to unify. 'Unifying psychology' is to mean: seeking, strengthening, and enlarging a common basis. According to my proposal-for-agreement this *common basis* is to consist of generally accepted definitions, postulates, and certain methodological principles. The rationale of this implementation, (3), is that it is precisely what is needed in order to interconnect the unnecessarily incommensurable languages and conceptualizations that are current within our many provinces and compartments.

Reaching Consensus: Is Psychology a 'Science'?

Taking into account that the hoped-for increased unity in terms of basics must be achieved by means of rational argumentation, deliberation and negotiation, without any type of overruling, a few characteristics of the *consensus process* can be foreseen and corresponding preconditions formulated.

First, there is no hope for any dogmatisms, whether behaviorist, psychoanalytic, humanist, or cognitivist, and so forth, that unifying efforts will make them win over their opponents to embrace their postulates. This means, second, that an operation of *dedogmatizing* is called for. This should lead to a situation where different schools of thought tolerate each other's methodological and substantive ideas as possible working hypotheses, and thereby as alternative options in defining 'scientific work'. Tolerance alone, however, is not enough. It would lead to the spurious solution previously expressed as: 'pluralism is beautiful'. Consequently, third, there must be a common ground, consisting of a few basic *agreements* between schools regarding the *demarcation problem*: what is to be considered a 'scientific' approach, or, a 'scientifically valid proposition'? Without some agreement on demarcation the whole unifying operation would remain pointless. It must be possible to reject — in principle, to reject unanimously — pseudo-scientific ways of reasoning and operating as well as scientifically invalid propositions, if pronounced in the name of science.

At this early stage of the present argument the objection that reaching this wonderful situation will be utterly impossible had better be responded to immediately. Apart from the risk that this objection, if supported collectively, continues to work as a self-fulfilling prophecy it is just an apriorist opinion on an empirical problem. The critic who holds it — amounting to: 'psychology will never grow up' — must be dedogmatized. Of course, it must be conceded that reaching a reasonable consensus on matters as fundamental as that of demarcation (not only interspecialist and interscholar but also international and, therefore, multi-lingual consensus) will require a substantial amount of labor. But it must be done; and there are examples in other fields.

In medicine meetings of consensus groups have become a regular phenomenon and are considered helpful, to say the least. But also in physics, the science many of us tend to be jealous of because of its supposedly comfortable objectivity, much work had to be done and is being done in order to come to agreement on conventions regarding definitions and units of measurement. The latest ISO Standards Handbook (1982) is the result of no less than 25 years of international discussion and negotiation. By contrast, psychologists thus far have done very little.

Getting back to the demarcation problem, can we at least agree that what we want to unify is the basics of psychology as a 'science'? Or, is it possible to reach agreement on the assertion that psychology is a science — or rather: is to be a science? I am afraid that the English word 'science', albeit for different reasons, is as unsuited to be used as a label for what we strive after as is the expression 'uninomic psychology'.

The ambiguity of the term 'science' has not gone unnoticed, of course (e.g., Creel 1983, p. 303). The amount of international confusion that has resulted from its various meanings, however, appears to be grossly and

generally underestimated. The main obstacle to an internationally acceptable solution of the demarcation problem is the fact that the dominant meaning of 'science' does not encompass academic fields such as Law, History, Language and Literature studies, and other so-called Humanities. This extensional exclusion has as its intensional consequence that to many English speaking psychologists, activities deemed to be characteristic of the humanities, are viewed as 'non-scientific' or 'pre-scientific' in nature. These activities include systematic description, hermeneutic interpretation, qualitative studies in general, self-report methods in particular (introspection, thinking aloud); and they also include language and concept analysis. I admit that times have changed since the reign of behaviorism in America — and in part overseas. Even so, anchored as it is in English linguistic usage, the prejudice that psychology would have to be a natural science — and, what is more, as much as possible, not like biology, but like physics — still remains with us. As a result of the fact that English has become the main language of international communication — and, of course, as a result of the direct influence of the U.S.A. as the foremost producer and selector of psychological publications — this prejudice has spread over many quarters in Europe and elsewhere. To these psychologists, too, sober empiricism along with powerful measurement — or computerization — and mathematical models have become the stamps of scientific value; whereas so-called 'verbiage', including essays and narratives, so-called 'anecdotal evidence', and common sense reasoning are abhorred, and rejected as pre-scientific at best.

But let me stick to language, and point to a translation problem. The common, but superficial translation of 'science' into Dutch is *'wetenschap'* (equivalent to the German *'Wissenschaft'*). The concept 'wetenschap', however, does *not* exclude the scholarly, academic study of the humanities. This has led to many misunderstandings on both sides of the Atlantic, and to some extent also of the North Sea. The consequence is that the assertion 'Psychologie is een *wetenschap*', is acceptable as a postulate for unifiers; whereas: 'Psychology is a science' is not.

'Scientia'

What can be done about this claimed inadequacy of the concept 'science' for the purpose of unifying psychology? First, the claim that a broader definition than the dominant one of 'science' is needed must be supported by a few additional arguments. Second, a solution to the formal terminological problem must be presented. Third, and not least, a solution to the demarcation problem itself will have to be proposed — to the extent that this is needed as a precondition for the unifying task.

As regards the need of a broader definition I shall take off from the attempt by Mario Bunge in his book (with Ruben Ardila) *Philosophy of Psychology* (1987) to formulate general "philosophic principles involved,

usually in a tacit manner, in scientific research" (p. 20-21). To begin with, ascribing 'tacit' beliefs to researchers on the basis of their working habits and methodological preferences — on what else? — is a rather fashionable but suspect and unconvincing way of reasoning. In particular, Bunge's 20 *ontological principles* or postulates — presented side by side with 20 epistemological and 20 moral ones — had better be read as his own beliefs. However interesting they are as such, at least two of these principles are highly critical. After the realist principle *O1*: "The world exists on its own (i.e., whether or not there are inquirers)" comes the materialist *O2*: "The world is composed exclusively of things (concrete objects)". It follows that ideas of any kind, human experiencing and the human mind, are denied the honor of 'existing'. Therefore, no 'Geisteswissenschaft'. But Bunge's book deals with psychology — conceived obviously as a science in the dominantly narrow sense. His materialist definition of 'existing' — since this is what his principle *O2* amounts to — cannot be said to be untenable, nor to logically prohibit the development of a psychological science. But this option is a highly dubious one, that leads to bizarre complications. This can be shown by considering Bunge's ontological principle *O10*: "There are several levels of organization: physical, chemical, biological, social, and technological". Obviously, on purpose, and consistent with principle *O2*, the mental or psychical organization level has been left out; 'there are' cannot be said of phenomena that do not exist. This implies, however, that in order to become 'scientific' everything we know about mental organization would have to be reinterpreted in terms of, and reduced to, one or more of the levels that are supposed to 'exist'. The claim that such a reduction would be necessary is absurd. Most important, it derives from a materialist philosophical option that evidently is not shared by all psychologists. It is an arbitrarily restrictive prejudice, regardless how large a majority of psychologists might opt for it be.

By contrast, let us take a naive look at the phenomena of this world, and in particular at its great *miracles*. Three categories of these have been recognized of old. First, the miracle of matter, the cosmos, studied by physics — including astronomy — and chemistry. Second, the miracle of life, studied by the biological sciences. Third, the miracle of the human psyche, of mental life, of our experiences and ideas, that constitute the primarily available *subjective world* of each of us. Since psychology has the privilege of studying this awe-inspiring miracle, denying its existence is absurd. Nor is the fact that it is difficult to study this world objectively a valid argument. It has been shown that within it there is plenty of lawfulness; for instance — to begin with — in the field of perception. It follows that any attempt at unifying psychology as a 'science' in the dominantly narrow sense is a forlorn enterprise.

Mutatis mutandis, the present argument against arbitrary postulates can be generalized, and used for the purpose of *eliminating* other restrictive principles — provided that these principles do not solely function as working hypotheses by which a group of researchers wilfully delimit their own program

but are also used as *dogmas* by which other approaches are disregarded or rejected, 'on principle', and characterized as non-scientific. Empirically speaking the strategy boils down to not accepting any such rejection of alternative approaches whenever the proponents of the latter are researchers whose work must be taken seriously.

With this condition, 'must be taken seriously', we are back at the demarcation problem. But let us first propose a solution to the *labeling problem*. Urging the whole English speaking world henceforth to use the word 'science' in its broader sense will not work. Nor is it likely that the introduction of a loan word from another modern language — 'wetenschap' or 'Wissenschaft' — will be successful. The simplest proposal appears to be this: whenever unifying problems are discussed, replace the English word by its Latin ancestor, *'scientia'*. Consequently, *we are attempting to unify the basics of psychology as a scientia*.

Regardless of the success or failure of this proposal, in the text to follow *scientia* will be used. 'Scientific' and 'scientist' will be maintained as English words but, refer henceforth respectively to: 'in the way of scientia', and a 'scientia-professional'.

Demarcating 'Scientia': What is to be Produced

How to demarcate 'scientia'? How is this concept to be *defined* in such a way that reasonable agreement can be reached, in particular on its lower boundaries? At first sight this task looks formidable. With a few preparatory considerations and pragmatic problem transformations, however, a solution consisting of a *judgmental procedure* will be shown to be feasible.

First, in order to avoid the danger of getting lost in philosophical vaguenesses, a transformation analogous to the one applied to 'unification' is needed. The risk of reification that typically looms large in attempts at defining abstract nouns must, and can, be eliminated. In this case a switch to a transitive verb is hardly feasible, but the *adjective* 'scientific' presents itself as an adequate entry to contextual specifications. The corresponding subsequent question then is: what types of subjects to this predicate, being or not being 'scientific' — or, being more or less so — are of interest to our purpose? The two primary candidates are obvious; namely, 'scientific activities' and 'scientific results', or 'scientific knowledge' — representing, respectively, the context of discovery (and development), and the context of justification. To begin with, one of these must be defined. Which one deserves priority?

At this point another question arises that must be given a pragmatic answer. In order to be called 'scientific', activities — including methods employed — must make sense in a given situation. This implies that a necessary condition is that they are *adequate* as attempts to attain a *goal* that itself can be called 'scientific'. The same applies in a somewhat different way

to 'results'. Along with their corroboration these will have to meet certain criteria that, at least in part, derive from the goal, from the objectives of scientific inquiry in general. In any case, the question to what general purposes scientists are doing their scientia-work must be attended to and given an answer — a minimally necessary answer — before we can continue our argument.

This new question, again, looks rather intractable. But here another pragmatic rule applies, namely: 'whenever questions regarding goals or objectives of an activity are at issue, begin by questioning *the type of effects* and in particular, *products* the activity is meant to yield". Or simply: what do we want to produce?

The crucial importance of this question can be shown by pointing to a rather wide-spread misunderstanding. Many psychologists appear to cherish the idea that the production of theories and models is not solely a means of promoting the production of scientific knowledge but also its ultimate goal. Scientific knowledge should *consist of theories*, provided that these are tested and the results published. That would suffice. Regrettably, this misunderstanding has been promoted by philosophers of science — as appears from their almost unanimous abhorrence of any type of convention by which the absolute freedom of designing new theories (including new concepts and terminologies) might be curbed. In the case of philosophers of science this overestimation of theories, conceived as ends rather than as means, appears to spring from two reasons. First, their high esteem of theories appears to be a natural effect of the paradigm function theoretical physics fulfills in their thinking. Second, their option for theories as the ultimate product is a self-serving one because in philosophy itself not much more than 'interesting' theories can be produced that do not provide definitive solutions. Since philosophers work and live for, and from, the discussion of perennial problems, many of them do not even want to have it otherwise. Let me quote Karl Popper — who claimed, rightly in my opinion, to have solved a few philosophical problems. In his autobiography he has this to say about his colleagues: "nothing seems less wanted than a simple solution to an age-old philosophical problem" (1974, p. 99: quoted from 1963, p. 5).

However, more important than this unwanted general influence of philosophical thinking on the empirical scientia "psychology" is the fact that the reference to physics with its awe-inspiring theories does not provide a valid argument. Theoretical physicists do not strive after developing theories, period. They strive after developing theories that are *true* — to the best of our present knowledge — and thereby generally *agreed* upon by all physicists. Where there are different 'schools of thought' in physics, or for that matter in chemistry or astronomy, these are viewed as 'science in the making'. To physicists, alternative theories obviously serve the purpose of producing generally accepted, and usable or useful, real 'ready-made science' (Latour,

1987). Moreover, these temporary schools of thought are never a priori incomparable by the use of idiosyncratic, provincial, and wholly incommensurable terminologies — as observed so often in philosophy, and in psychology.

What can be concluded from these considerations? First, that the unifying problem requires an anti-philosopher's rather than a philosopher's way of thinking. Second, that the problem of the ultimate, general goal of *scientia* activities is a crucial preliminary problem that must be solved. Third, that this problem is approached best by reconsidering primarily the intended results of scientific activities rather than these activities themselves. Consequently, the criteria which 'knowledge' or results are to meet in order to be called 'scientific' must be pinned down.

Valid Knowledge Must be 'True'

According to my own old (1961) textbook, in the English version of which (de Groot, 1969) the word 'science' is used in the sense of 'scientia', or '*wetenschap*', the purposes of scientific pursuits are the following (18-19). A scientist "characteristically processes his experiences of the phenomena encountered in his specific sector in one or more of the following ways: he endeavors to *describe*, to *order*, to *record* (measure) them; to *understand* and to *explain* them; in these activities he is motivated primarily by a desire to be able to *predict* new phenomena, so that their predictability shall enable him" — and others — "to *control*" (the corresponding area in) "his sector by *influencing* the phenomena".

All this may be scientific activity. But then (p. 19): the resulting knowledge must be "set out in *statements* so that it can be communicated and used: scientific knowledge is, in principle, public knowledge" (cf. Ziman, 1968).

From this liberal, and as such rather standard, and hardly challengeable account the obvious conclusion can be drawn that the *natural unit of scientific knowledge is a statement,* regarding certain phenomena in the world. In order to be called 'scientific' the statement must make sense and be phrased sufficiently clearly to be workable; but let us take this for granted. However, having switched from 'knowledge' and 'results' to 'statements' as the subject of being scientific or not, another and more general question confronts us. *What types of statements*, regarding *form and content*, are acceptable; that is, can be judged adequately as *scientia*? It appears that the variety is much larger than that of 'results' in the traditional, narrow sense. One 'statement' may consist of a single sentence, or of a passage in a text, or of a more or less complex argument. It may express declarative or procedural knowledge, it may relate a fact, explain a theory, posit a law, but it may also lay down a convention. It may assert something about the comparative value of ideas, the

probability of an event, the plausibility of an interpretation, and so forth. If possible, for all these types of statements we shall have to establish the *criteria* by which their 'scientific validity' can be judged.

It may seem here that the switch to statements has complicated our problem. But, actually, it has been simplified. The most important criteria are generally accepted. Since we are wont to think of 'new knowledge' as the intended product of scientific effort the content of a scientifically valid statement regarding the world must be *new*. Furthermore, it must be of a certain minimal scientific *import*; and, as a matter of course it must be *true*.

For our purpose, however, newness can be discarded. Its elimination is desirable because it is a dangerous shibboleth of our day; it can be faked easily, and it is often — by offering old wine in new bottles; new formulations of well-known relationships. More important, its elimination is necessary because persistent old schools of thought, such as psychoanalysis and behaviorism, and, for instance, various older conceptions of intelligence, may be as much in need of being interconnected as are more recently developed ones. Finally, the remaining relevance of the newness criterion can be incorporated in the importance criterion; whatever is really 'old hat' is not important.

The second criterion, 'scientific import' — including applicability in advanced scientific work, in technology, or practice — will be discussed later. Obviously the primary, most intriguing, and problematic criterion is that of 'truth'. It deserves to be discussed first. Therefore, our next task is that of completing the sentence: 'A statement is *scientifically true*, if and only if....'

Again, this may seem a formidable job, in particular since we have been so lenient and vague in defining the set of acceptable statements. But it is precisely here this broad conception will be helpful, and bear some unexpected fruits. By continuing the way of reasoning introduced in the preceding sections a pragmatic solution to the truth problem will emerge. At this point, however, a few additional comments on this way of reasoning are called for.

One of the disadvantages of the tendency in academic psychology to conceive of our scientia as a natural science, and, in particular, to model it after physics — a tendency I like to label 'physicalism' — is that it involves an overestimation of *strictness*. The strong *emphases* on mathematical and computer models, on formalized languages to which strict logic applies, on powerful scales of measurement, on experimental evidence based on precise operational definitions, along with the corresponding rejections if not aversions mentioned earlier, all this has led to a particularly harmful *neglect*. The area neglected is, in brief, the one that lies between what can be handled strictly and what cannot be scientifically handled at all. To mention a few catch words, it is the area of *experiential* knowledge and wisdom, of *common sense, reasonable* argument, and of *understandable* relationships. It is also the area in which, after *deliberation* and *negotiation, agreements* can be reached on *definitions* and other *conventions* . These cannot but be phrased in *non-precise*

natural language that nonetheless makes sense, and is instrumental in acquiring scientific 'unified' knowledge.

It is in this neglected area that, for our purpose of improving the interconnections between schools of thought within psychology, the hard work will have to be done. It obviously requires mutual understanding; and, therefore, a language that, apart from a few previously agreed technical terms, must be free from jargon that involves theoretical school preconceptions. Interestingly, this requirement also involves understandability to an interested *layman* who is able and prepared to invest some effort in grasping the background of the pertinent statement. We are reminded here of the assertion quoted in the previous section: 'scientific knowledge is, in principle, public knowledge'.

'Scientifically True' Statements: The Layman Paradigm

Getting back now to the problem of how to define 'a scientifically true statement' I take off from that same interested layman. Assuming that he is skeptical enough to not accept any authoritative assurance that a certain statement is just true — possibly even with 'absolute certainty' — what then are the means this layman has available to find out whether a statement can be trusted to be scientifically true? Of course, the problem is that he may be unable, or, unprepared to go deeply into the pertinent evidence and be convinced personally by the corresponding argument. In this case, again, what can the layman do to exert his right of information, that is, of getting certainty about a particular piece of, in principle, public knowledge? The answer is obvious. He will consult other experts, preferably experts of different orientations. If all the experts he has been able to consult unanimously endorse the statement, he will conclude that it is to be trusted, that is: in all likelihood it is 'true'. But if this is not so, does he conclude that the statement is false? Not necessarily or, rather, according to the definition it *is* not-true — and that can be called 'false'. But then there are two subcategories. The issue may be pending; or, the statement may be 'scientifically false', meaning that experts agree unanimously that it is false. In judging the scientific validity, or, the scientific truth of a statement the layman often has to use, and uses, a three-valued logic.

So does the scientist. Thus, pending issues are statements — of fact, theory, convention, plausibility, and so forth, the variety by form and content just described — that are undecided by lack of consensus. These are characteristic of scientia in the making. Scientific knowledge, however, the substance of ready-made scientia, and the product scientists are supposed to strive after, consists of statements that are *scientifically true*.

The definition of 'a scientifically true statement' that constitutes the core of the *Forum Theory* (de Groot, 1984, 1985) has been based on these considerations — the 'layman paradigm' if it must be named. A statement, on

some *issue*, is defined as being 'scientifically true' in the strict sense if and only if *all* living issue experts unanimously endorse its scientific validity, that is, its *'truth to the best of our present knowledge'*. Since actual consultation of all virtual members of such an 'issue Forum' — with a capital F — is hardly ever feasible a less precise but practicable, and operationalizable definition is also given:

> A statement is 'scientifically true', to the best of our present knowledge, if and only if it is unanimously considered as scientifically true in the strict sense by an issue forum — with a lower case f — that has been selected and has operated according to the rules of the Forum Theory.

I shall spare you the details of these Forum rules. Their rationale is, of course, that in the forum sample the best experts of the most important orientations, if any, have participated, and that the discussion, orally or in publications, of the issue has been thorough.

A few of the assets of this definition are worth mentioning. First, it is a *behavioral-scientific* definition by which the truth or non-truth of a statement becomes *empirically decidable*, in principle. Second it is a *modest* definition that becomes scientists. It does not pretend to cover any kind of 'absolute' or 'eternal' truth, whether logical, metaphysical or ideological. This is as it should be because none of these absolute conceptions goes unchallenged. Regarding absolute (certainty about) truth the only generally agreeable conception is that this is beyond human reach. Third, this definition is *broad and comprehensive*; it provides an avenue by which common sense knowledge and, more important, *qualitative statements* can be legitimated as scientifically true. It provides a bridge between 'science' and 'humanities' conceptions within psychology as a *scientia*. Fourth, in spite of this broadness it is sufficiently, not to say highly *demanding* in its requirement of unanimity. Fifth, by its modesty it is *flexible*; what is true to the best of our present knowledge may prove untrue in the future, and the other way around. Sixth, the switch to statements as the elements of scientific knowledge reminds us of the importance of their adequate phrasing, and promotes the art of *rephrasing* ideas in order to reach agreement on them.

Seventh and last,[7] the admission of a much more variegated collection of statements as candidates for being judged 'true' or 'un-true' enables us to scientifically legitimate other types of statements that may be in need of being

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[7] An eighth characteristic, of particular importance in the treatment of claims of scientific truth regarding issues of social or political consequence, is the concreteness of the Forum-theoretical definition of *non-truth*. In principle, one acknowledged expert can refute a truth claim. Moreover, after publication of his, supposedly well-argued, non-agreement, a person who in spite of such contrary evidence publicly maintains a truth claim does not only make a 'mistake' but can be called a 'liar'. This concreteness might be helpful in combating the loose, unjustified use, for *pro Domo* reasons (as contrasted with *pro Foro*), of expressions like: 'It has been scientifically proved that ...'

so judged. Next to certain common sense and qualitative statements about the world, a highly important category is that of *conventions,* and in particular, *definitions.* Logicians are right, of course, in asserting that a (stipulative) definition cannot be 'true' or 'false' in any of the traditionally accepted meanings of these words. But in the Forum-theoretical sense in which 'true' means 'unanimously agreed' a definition can be judged on being 'scientifically true' — or perhaps rather: 'truly scientific' — or not. This enables us to scientifically legitimate certain definitions of basic concepts as true — again, to the best of our present knowledge. It also legitimates working for consensus as a scientific activity — following the examples of physicists and the medical profession.

'Scientific Import': The Poverty of Labeled Knowledge

Is it possible to reach agreement on the 'scientific import' of a scientifically true statement? In many respects this is a more complex and more obstinate problem than that of truth. It is clearly a matter of degree rather than a problem that can be solved in terms of a dichotomy or a trichotomy. Furthermore, importance obviously depends on context; almost any true statement may be of some scientific import in some specific argumentative context. An absolute scale, with an absolute zero point — 'of no scientific import whatever' — is out of the question. Moreover, even within a more or less restricted context only comparative judgments appear to be feasible, on an ordinal scale of 'scientific import'. For instance: 'Considered as a contribution to our scientific knowledge regarding problem P, or, as a contribution to acquiring such knowledge, the true statement A is of greater scientific import than is the true statement B'.

This requires that A and B are comparable, implying that they belong to the same statement category. Thus statements of different facts can be compared, or, of different hypotheses or theories, or, statements of alternative explanations or interpretations regarding problem P. But A and B may also be statements of alternative conventions, such as definitions — again, judged on their respective scientific import as contributions to our attempts at acquiring and formulating knowledge regarding P. All such statements, if duly specified, can be judged, and actually are judged quite often by scientists. And sometimes these judgments are even agreed upon.

But what does 'duly specified' mean? Generally speaking it means: specified in such a way that the comparison of A and B becomes decidable. This may be effected by various means. First, specifications — in the sense of particularizations — of the P-context are possible; that is, replacing P by one of its subproblems. Second, the intended meaning of the expression 'scientific import' may be specified by replacing it by one or a few of its aspects or dimensions. If A and B are, for instance, statements of alternative theories, then A may be preferred because of its more comprehensive coverage, or its

higher precision, or its greater heuristic value, and so forth. Finally, the
formulations of A and B, themselves, are not God-given; it may be possible to
change them somewhat, in form or in content but retaining their tenor, so as
to make them comparable (compatible, commensurable) and the comparison
of their scientific import decidable.

All this holds, *mutatis mutandis*, in the case where A and B are alternative
definitions. In this case the possibility of revising the phrasings of A and/or B
are, of course, particularly crucial.

Here I shall not go into any more detail about these various possibilities.
In principle, they are well-known. The *conclusions* to be drawn from the
preceding general considerations are rather simple.

> 1) On the scientific import of statements comparative judgments
>   are often possible, especially if adequate specifications are
>   provided.
>
> 2) If the members of a pertinent issue forum are unanimous in
>   their agreement that statement A is of more scientific import
>   than is statement B, this comparative assertion itself is scien-
>   tifically true — to the best of our present knowledge.
>
> 3) The latter situation may amount to a rejection of statement
>   B to the given purpose, and in the given context.
>
> 4) Consequently, the criterion of 'scientific import' can be used
>   to eliminate certain scientifically true statements. That is, in
>   addition to the truth criterion but on the same Forum-
>   theoretical basis, scientific import can be used as a scientific
>   demarcation criterion.
>
> 5) If A and B are both true, as presupposed, but no agreement
>   can be reached on their comparative virtues in terms of
>   scientific import, then the issue of A versus B — if not
>   rejected itself as, for instance, being fundamentally un-
>   decidable — remains a pending issue, that is, a comparative
>   statement on which there is as yet no scientific knowledge.
>
> 6) The present treatment of 'scientific import' can be general-
>   ized. It applies as well to other, more specific comparative
>   qualities of two or more alternative options regarding an
>   issue. For instance, their degree of objectivity, plausibility,
>   heuristic value, simplicity, understandability, and:
>   agreeability. On all these, judgmental, comparative qualities
>   scientific knowledge is possible, in principle.

One application of the scientific import argument is of particular inter-
est for our judgment on the state of the art in psychology, and thereby for the

unifying problem. A true statement may be true only because a label of a particular school of thought is attached to it. For example:

'The young son wishes — whether or not unconsciously — to murder his father and to possess his mother.' This statement is true provided that the label 'according to psychoanalysis' is attached. Another example:

'Of course a computer can think. The only way we judge human beings to be thinking is by noting that they are capable of doing certain things.' Here the label 'according to certain cognitive science postulates' is needed in order to make the statement scientifically true.[8]

The latter example may require a brief explanation. It is, in particular, the expression 'the only way' that is true only with the label. When I suppose that someone 'is thinking', or, that I am thinking — or, for that matter, when Descartes tells us that he thinks and concludes that he exists — an entirely different criterion is used, namely a subjective human experience which we all evidently know, and *agree* to share. Of course, I do not object to the use of the restrictive cognitive science definition or to, for instance, the use of the well-known Turing criterion. I do object, however, to the dogmatic qualification expressed in 'the only way'. The second statement, too, including the assertion that a computer program can think, is true-with-a-label only. In other words, the definition according to which it is true, is not 'true' itself — although it may be quite interesting.

In psychology this kind of school-labeled scientific knowledge appears to be much more extensive than unlabeled knowledge — to which it is, *ceteris paribus*, patently inferior. True propositions, P, of the following general type: 'According to postulates and terminology of theory T, school S, or author A, proposition P (is true)' are agreed, I suppose, to be of a severely limited scientific import *whenever* these postulates and terminology are rejected in other theories, by other schools, or by other authors. Such labeled forms of scientific knowledge are also characteristic of 'scientia in the making', as contrasted to established scientific knowledge on which we and our lay-clients can rely and build.

Necessity and Feasibility of Unifying Psychology

At this point, an overview of the outcomes of our argument thus far is in order, along with a few rather direct consequences. Obviously, the key to what we have got so far is the Forum-theoretical, procedural, operationalizable, definition of truth based on the layman paradigm. It has enabled us to lay down how in scientia the demarcation problem can be solved by means of procedures that define the two main criteria to be met by statements of

[8] This statement happens to have been made by Herbert Simon in an informal interview (conducted by van den Herik, March 14th 1981, as part of his Ph.D. study, published in 1983), but countless others are likely to endorse it — omitting the label.

scientific knowledge, namely, their being scientifically true and their having scientific import. In this solution it is implied that scientific work is generally agreed, that is, forum-agreed, to *aim* at producing true statements that are of the highest possible scientific import.

Consequently, *activities* — including *methods* — can be legitimated as being scientific, whenever the pertinent forum agrees that they are reasonably likely to serve this aim of scientific work. Again, the qualification 'reasonably likely' — indicating a mild, benefit-of-the-doubt-like criterion, to be supported by a reasoning — is defined scientifically, in principle, by agreement in the pertinent issue forum. *In all these demarcation problems, of the general type: 'Is X scientific or not', this same criterion, or 'device', of the truth definition is available — if needed.*

At first sight the requirement of unanimous agreement may seem a heavy obligation. However, in the majority of cases there is little risk in taking it for granted, or, in shortcutting the consulting procedure, for instance, by trusting an authoritative handbook or person — or, one's own expertise, or, common sense. The unanimity requirement actually functions as *security* to which deviant experts may appeal, and by which errors of our conventional wisdom can be prevented or corrected.

If all this is taken into account, the strong emphasis on agreement among experts is not a peculiarity of the Forum theory. It is a basic and self-evident characteristic of scientific communication wherever scientia prospers; that is, in fields with a high production, past and present, of true and important scientific knowledge, without school-labels. Physicists, for instance, do not need to be reminded that they must agree on basics; they practise it. It is true that in modern science, as Latour has shown (1987), highly unorthodox competitive processes within and without the scientific arena can be observed in the phase of 'science in the making'. But as soon as 'ready-made science' has been attained a peace is signed.

Viewed from this angle the problem of psychology is that it is insufficiently productive of established, label-free scientific knowledge. Since this is largely due to the fact that its compartments do not really communicate but rather compete, for grants, by talking at cross-purposes, even within the same problem areas, the verdict must be that psychology cannot be claimed to be a serious, fully-fledged scientia. We have not yet learned how to apply the French verb 'Pour se disputer il faut être d'accord'. Learning this, and attempting to unify the field by agreeing on basics, is a *scientific necessity*.

Evidently, the imperative: 'Unifying is a must' applies as well to less general and *more substantive problem areas* than that of demarcation. Moreover, and more important, the same approach, this same initial device can be applied. Its rationale can be described in terms of three steps:

1) In attempts at reconciling different conceptions select, or seek and adapt, one (or a few) basic *concept(s)* that are commonly

used or presupposed, but conceived and defined differently by the pertinent schools of thought.

2) *Name* this concept (or, these concepts), possibly adapted by a minimal linguistic transformation or contextual specification, and analyze the meanings of this 'concept term'.

3) Design a definition that encompasses as efficiently as possible all relevant school particularizations.

In the unifying context where these different conceptions, (1), pertain to the same or overlapping fields of phenomena but are phrased in incommensurable languages this device is always feasible because the various school conceptions, or theories, have part of their subject matter in common. The question to be answered then is: which types of *phenomena* are investigated, supposed to be understood, or pretended to be explained or predicted? This common study object is then to be analyzed and named in step 2, and defined in step 3.

The device is called 'initial' because its result is no more than a hoped-to-be-agreed, very general definition of a very broad concept. But this first result is important. A 'smallest common product' of relevant meanings (or, their logical sum) has been constructed on the basis of which other concept-analytic operations can be carried out. One of these consists of finding, and attempting to maximize, the 'greatest common divisor' (the logical product) of the various meanings of the concept term(s) in question. Another one consists of proceeding to particularizations, that is, to analyzing and defining the particular acceptations current in specific schools of thought. As a matter of course the initial device can be applied iteratively.

The fact that this device is always possible supports our statement that *unifying* is not only 'a must' but also *feasible* — provided that the abstract three steps scheme is implemented with concept-analytic skill, methodological expertise, and with some common sense cunning. For the fulfillment of the latter conditions Signific Concept Analysis (SCA), is again recommended as a helpful tool.

SCA for Unifying: 'Intuition' as an Example

Signific Concept Analysis has been developed for the more general purpose of "criticizing, clarifying, and economizing the way in which concept terms", abstract and problematic ones in particular, "are used in serious non-fiction texts." SCA is a pragmatic instrument, that "can be described as a rather loosely knit system of heuristic rules." It is an art rather than a precise technique, but an art that can be learned (de Groot & Medendorp, 1988, p. 247).

In order to give an impression of how the SCA approach works and can be applied in the unifying context a few of its rules — most of which have been either mentioned or tacitly applied in the preceding text — are presented.

1) In selecting a concept to be analyzed and defined, do not restrict the search to frequent and obvious concept terms but also look for *implicitly assumed conceptualizations and meanings.*[9]

2) 'Start from language' — as previously mentioned. This includes a whole subsystem of rules such as: taking *lexical* meanings into account (including etymological overtones); distinguishing *types of concepts* and, in particular, possible uses of concept terms that are consequential in deciding on how to define them; exploring *word class transformations*; specifying meanings, when necessary, by means of adjoining a minimal *context*; and so forth.

3) Ensure the definability of the selected concept term by *eliminating* anomalous lexical meanings as well as methodologically unacceptable (too vague, or inconsistent) conceptualizations.

4) *Do not jump* prematurely to (school-) specific meanings, let alone to operationalizations or to strictly definable, logical model conceptualizations if these evidently do not cover the concept-as-intended.

5) Start with definitions which are as *phenomenal* as possible.

6) In definitions, do *not* use specific *theory-loaded concepts* that involve the risk of losing a part of the target audience, either because they do not know the specific underpinnings, or reject certain implicit postulates. This rule amounts to an exhortation to use generally understandable, natural language — with the obvious exception of a few technical concept terms that are either generally familiar in the prospective consensus forum or previously agreed.

What to do, for example, with 'intuition' as a psychological concept? (cf. de Groot, 1986). Without doubt it is worth being studied, but then, a down-to-earth phenomenal definition is needed. 'Down-to-earth' implies that we do not want a restrictive elitist definition; we do not want a concept with a halo: only for the happy few. This amounts to discarding what some

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[9] In applying this rule the inherent danger of ascribing 'tacit' beliefs to persons who do not subscribe to these, mentioned in the section *'Scientia'*, must be reckoned with. However, since SCA is a tool for reaching agreement such erroneous ascriptions are not fatal but solely inefficient; they will be undone in the consensus phase where actual agreement is sought.

philosophers have to say. In particular to be rejected is the idea that intuition would reveal some absolute truth. Furthermore, a choice must be made between the three kinds of lexical meaning of the term that, in one sentence, can be shown to be in use: 'In a person with intuition, intuition may lead to an intuition'. Which of the three concepts is to be defined, 'intuition' as a capacity, as a process or as a product? The choice is easy. Process and product deserve priority. Both are 'intuitive', so the switch to the word class of adjectives is warranted. The next choice is that between intuitive behavior and intuitive mental processing. Since attributing intuitive behavior to a person implies the assumption of intuitive mental processing, or operating, the latter is to be primarily defined. Then another question arises: how to distinguish intuitive processes and products from purely instinctive ones on the one hand and from purely impulsive or emotional-preferential ones on the other? 'Not solely instinctive' implies: to some extent learned, based on experience; 'not solely emotional' implies that intuitive operating makes sense, that is, leads to results such as preferences, insights, hypotheses, and generally *anticipations*[10] that need not be true but are presumably better than chance. Otherwise, intuition could not be considered as a person capacity. Finally, it seems wise to introduce, contextually, a domain specification. I have opted for 'intuitive operating within a process of problem solving — of any kind', because that was and is my primary interest. I propose the following definition:

> Within a person's process of problem solving a mental operation is called 'intuitive' whenever it is assumed to be based on one or more generally valid (better-than-chance), heuristic rules of experience that the person himself is not able to fully explain and/or to justify objectively.

One of the assets of this definition is that it covers elementary heuristic rules, that are intuitive, as well as complex judgmental-anticipatory intuitions. Moreover, for purposes of reaching agreement, the definition can be adapted and generalized without much effort. Finally, it ensures that a computer program cannot process intuitively, by definition, because it needs explicit, non-fuzzy operating rules. However, the main point of this example is, that the construction of this kind of phenomenal definitions of broad concepts that stand for areas of scientific study is expected to fulfill an important precondition of any unifying program. Apart from being necessary, unifying psychology is feasible in this way.

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[10] Carl Gustav Jung, the only psychologist (psychiatrist) who considers intuition as a basic cognitive function — *'Intuieren'*, next to *'Empfinden'*, *'Denken'*, and *'Fühlen'* — has emphasized rightly the anticipatory nature of all intuitive processing (cf. de Groot, 1986).

Other Examples: 'Interpretation'; 'Innate Intelligence'

In the preceding argument many questions had to be left open. First, other defining problems may prove much more difficult to solve than that of 'intuition'. The concept term 'interpretation' is a case in point. It is an important concept because a comprehensive definition could be an instrument by which believers in hermeneutics and predictionism — or, hypothetico-deductive operating — might be reconciled; or, at least, be interconnected in a way that is acceptable to both sides. Let me just give my proposal in the form of a *mapping sentence* — almost a definition — on which particular definitions can be based (cf. de Groot & Medendorp, 1986, pp. 149-150).

> An interpretative *activity* (Ac)
> results in one or more interpretative *propositions* (Pr)
> in which, according to *rules* (Ru)
> that belong to or follow from an interpretative *system* (Sy),
> it is asserted that a colligendum of *materials* (Ma),
> as regards certain *aspects* of the latter (As),
> must or can be considered *justifiably* (Ju)
> in a certain interpretative *sense* (Se).

This sentence is almost empty, but it does pin down all the elements that are basic, *and* debatable, in interpretations; and, had better be taken into account in defining them. The term 'colligendum' is a technical term, used in SCA whenever a set or system is incompletely defined as yet, that is: still to be 'collected', in part. In attempts at obtaining agreeable definitions the mapping sentence is a useful tool. But I must now leave it at that.

The case of 'intelligence' is, of course, a particularly difficult one. It differs from that of intuition in that replacing the substantive by the adjective 'intelligent' is not a good idea. This replacement runs the risk that only 'intelligent behavior' and/or, more or less, 'intelligent people' are studied, whereas the supposition of *intelligence as an instrument of cognitive functioning* is forgotten. Here another rule of SCA applies: never ask whether such an instrument or system 'exists' or not. The relevant question is whether it makes sense to introduce, or rather to maintain, it as an abstraction. There are good reasons for doing so, one of which is that the concept of intelligence is some two thousand years old and has proved its usefulness long before psychologists adopted it and tried to clarify its functioning. This argument may be invalid in a science but it is valid — although not in itself sufficient — in psychology as a scientia.

Is there any chance that the inconclusive IQ debate, and, in particular, the nature versus nurture question can be pacified by means of signific concept analysis? I am convinced that even this, ideologically loaded, controversy can be overcome, or at least mitigated by means of agreements. The

strategy consists in the main of not solely concentrating our analytic and research efforts on the, in itself, biologically and culturally silly question to what extent differences in *test scores* or experimental achievements are genetically determined. What is known about intelligence as it functions in daily life must be studied as well. Particularly instructive is the comparison of intelligence with other, more specific, but also complex subsystems and capabilities, such as that of playing chess. Elsewhere I have examined this way of viewing intelligence. In the present context the first step of this approach is crucial. It is that agreement should be sought on how intelligence as it functions in real life is to be defined. *This real life intelligence* is a concept on whose meaning a reasonable consensus can be reached. It is the study object about which we are speaking, the thing that matters, or the *concept-as-intended*. At this level reasonable agreement is crucial; we had better speak about the same complex phenomenon in our attempts at interconnecting different views. The preconditions to this agreement are difficult to meet but not impossible. They include a successful 'dedogmatizing' operation, and a thorough concept analysis based on what is known from various specialities — such as developmental psychology — *and* from general experience in daily life, for instance, the way in which other complex abilities develop in individuals, who may or may not reach their individual 'ceiling'. This (ordinal) concept, a person's individual ceiling in a given Complex Capability, CC, is crucial because it provides a theoretical definition of 'innate CC-talent', that applies to, and can be implemented in, the case of intelligence as well. In environmental conditions that optimally promote a duly motivated person's CC-development she/he is certain to attain or to approximate her/his CC-ceiling. In brief: 'ceiling equals innate talent'. Applied to intelligence, this postulate opens an avenue towards an alternative research program, by means of which a substantially enhanced agreement on intelligence will come within reach. But here I must end and refer to those other publications (de Groot, 1982, 1983 — and a few papers in Dutch).

Conclusion

Ultimately the proof of the pudding is, of course, in the eating. After the preparatory, concept-analytic work, in which some of the preconditions can be met, then actual consensus meetings are to be held — interscholar, interspecialist, international, and *not* unilingual. The only thing I have to say about this decisive phase is that such meetings are indispensable if psychology is to become a scientia.

My last precondition is, that the two pedestrian activities called concept analysis and consensus meetings are to be *acknowledged and rewarded as truly scientific work*; acknowledged, and valued, within our scientific community, and rewarded by scientific foundations by means of grants for pertinent programs and projects. This is my primary answer to the first of the two

questions with which I started this paper: how to raise a general interest in unity problems? Proponents of unifying scientia will have to work hard on the fulfillment of this precondition.

References

Altman, J. (1987). Centripetal versus centrifugal tendencies in psychology. *American Psychologist, 42,* 1058-1069.

Baer, D. M. (1987). Do we really want the unification of psychology? A response to Krantz. *New Ideas in Psychology, 5,* 355-359.

Bakan, D. (1987). Psychology's digressions. A response to Krantz. *New Ideas in Psychology, 5,* 347-350.

Bolles, R. C. (1975). *Learning theory.* New York: Holt, Rinehart, & Winston.

Bunge, M., & Ardila, R. (1987). *Philosophy of Psychology.* New York-Berlin: Springer-Verlag.

Creel, R. E. (1983). Eudology: the science of happiness. *New Ideas in Psychology, 1,* 303-312.

Eysenck, H. J. (1988). The concept of "intelligence": useful or useless? Editorial *Intelligence, 12,* 1-16.

Feyerabend, P. K. (1975). *Against method.* Reprint London: Verso.

Feyerabend, P. K. (1978). *Science in a free society.* Reprint London: Verso.

Fiske, D. W., & Schweder, R. A. (Eds.) (1986). *Metatheory in social science. Pluralisms and Subjectivities.* Chicago/London: University of Chicago Press.

Gergen, K. J. (1988). United we fall: a response to Krantz. *New Ideas in Psychology, 6,* 219-222.

Groot, A. D. de (1969). *Methodology. Foundations of inference and research in the behavioral sciences.* (Rev. trans. of (1961) *Methodologie).* The Hague: Mouton.

Groot, A. D. de (1982). On the limits to 'developed capabilities'. In Netherlands Institute for Advanced Study in the Humanities and Social Sciences, *Limits to the future, Essays on the occasion of the second NIAS-lustrum* (pp. 98-129). Leyden: NIAS/Leyden University.

Groot, A. D. de (1983). Heuristics, mental programs, and intelligence. In R. Groner & W. F. Bischof (Eds.), *Methods of heuristics* (pp. 109-129). New Jersey-London: Lawrence Erlbaum.

Groot, A. D. de (1984). The theory of the science forum: subject and purport. *Methodology and Science, 17,* 230-259.

Groot, A. D. de(1985). Kern en consequenties van de Forum-theorie: over wetenschappelijke 'waarheid'. *Mededelingen Koninklijke Nederlandse Akademie van Wetenschappen, 48,* 157-178.

Groot, A. D. de (1986). Intuition in chess. *ICCA-Journal, 9,* 67-75.

Groot, A. D. de (1989). Unifying psychology: a European view. Invited opening address, First European Congress of Psychology, Amsterdam, 1989 (in press).

Groot, A. D. de, & Medendorp, F. L. (1986). *Term, Begrip, Theorie. Inleiding tot signifische begripsanalyse.* Amsterdam/Meppel: Boom.

Groot, A. D. de, & Medendorp, F. L. (1988). Signific concept analysis (SCA). A modern approach. *Methodology & Science, 21*, 247-274.

Herik, H. J. van den (1983). *Computerschaak, schaakwereld en kunstmatige intelligentie.* The Hague: Academic Service. (Ph.D. thesis Erasmus University Rotterdam).

ISO Standards Handbook. (1982). *The units of measurement.* Genève: ISO Central Secretariat.

Joergensen, J. (1951). *The development of logical empiricism.* International Encyclopedia of Unified Science, 2. Chicago: University of Chicago Press.

Jung, C. G. (1920). *Psychologische Typen.* Zürich/Leipzig: Rascher Verlag.

Krantz, D. L. (1987). Psychology's search for unity. *New Ideas in Psychology, 5*, 329-339.

Latour, B. (1987). *Science in action.* Cambridge MA: Harvard University Press.

Mao-Tse-Tung. (1966). *Quotations from chairman Mao-Tse-Tung.* Peking: Foreign Languages Press.

Newell, A. (1987). Unified theories in cognitive science (preliminary draft).

Popper, K. R. (1963). *Conjectures and refutations. The growth of scientific knowledge.* New York: Basic Books.

Popper, K. R. (1974). Autobiography. In P. A. Schilpp (Ed.), *The philosophy of Karl Popper* (pp. 3-181). The library of living philosophers, Vol. XIV Book I. La Salle, IL: The Open Court Publishing Co.

Royce, J. R. (1987). More order than a telephone book? A response to Krantz. *New Ideas in Psychology, 5*, 341-345.

Sarason, S. B. (1981). An asocial psychology and misdirected clinical psychology. *American Psychologist, 36*, 827-836.

Spence, J. T. (1987). Centrifugal versus centripetal tendencies in psychology. Will the center hold? *American Psychologist, 42*, 1052-1054.

Staats, A. W. (1983). *Psychology's crisis of identity. Philosophy and method for a unified science.* New York: Praeger.

Staats, A. W. (1984). Scientific chaos is not science. Invited address, American Psychological Association, Toronto, Canada.

Toulmin, S. (1987). On not overunifying psychology: A response to Krantz. *New Ideas in psychology, 5*, 351-353.

Ziman, J. M. (1968). *Public knowledge. An essay concerning the social dimension of science.* Cambridge: University Press.

A PHENOMENOLOGICAL VISION FOR PSYCHOLOGY

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SUMMARY: The viewpoint of this article is that the precise articulation and theoretical understanding of the 'proper object' of psychology is an achievement that still lies ahead of us. However, an effort is made to advance the solution of the problem by raising the issue again and attempting to formulate a solution. Specifically, the Jamesian fourfold framework for discussing the science of psychology, namely : 1) the psychologist, 2) the thought or feeling studied, 3) the thought's object, and 4) the psychologist's reality, is adopted, modified and interpreted in a phenomenological way. It is affirmed that psychology deals with a field of experience constituted by an intentional relationship between a subjectivity and a world that is greater than sheer life ('bios') but less than logical. It is argued that psychology could be defined as the subjectively dependent and contingently expressed meanings of individual subjects. It is assumed that metaindividual factors (sociality, history, etc.) can be expressed by individuals even if they do not originate in individual subjects.

Introduction

In the psychological literature that appeared during the late nineteenth century and the early twentieth century, one could still find statements indicating that psychologists were struggling to clarify the "nature, scope, problems and legitimate methods of psychology and the relations it sustained with other forms of sciences and with metaphysics" (Ladd, 1892). After the demise of introspection and the advent of behaviorism, psychology settled into a period of conventional consensus with respect to methodology wherein some combination of experimentation and statistical design were the primary acceptable strategies. Questions concerning psychology's scientific status and its unique contribution to scientific knowledge disappeared and it was assumed by most members of the profession that psychology had arrived, at least conceptually speaking, and all that was left to do was gather mounds of empirical data. A century of experience with this approach has left more to be desired than actualized, and so it seems to me that one must return to those foundational questions once again so that they can be answered with consistent theoretical justification rather than having a consensus based upon merely conventional criteria. The purpose of this paper is to sketch such a vision based upon phenomenological thought.

Phenomenology

I am aware, as well, that phenomenology is more misunderstood than correctly understood, so I shall try to sketch its appropriate philosophical framework. Firstly, it should be stated outright that the sense of phenomenology that I adopt is a strict, continental sense following Husserl and Merleau-Ponty, and not the ahistorical, looser sense of the term as developed in North America. Secondly, it should be appreciated that phenomenology as a philosophy operates within the tradition of critical philosophy, even while radicalizing it, begun by Descartes and developed by Kant and Fichte, among others, in the sense that it partakes of the Copernican philosophical revolution that grants a certain primacy to consciousness when it comes to the problems of experience and knowledge. For phenomenologists, consciousness is the medium of access to anything whatsoever that can be known or spoken about and in that sense it holds a privileged position. Moreover, the minimum that can be said is that the mode of being of consciousness is quite different from the mode of being of (physical) things and this fact has important implications for psychology since all definitions of psychology refer to phenomena that are non-thing like. Thirdly, phenomenology is a philosophy that studies and thematizes the field of consciousness in the sense that anything that presents itself to experience or consciousness is a legitimate topic of study, the purpose being to try to understand just how such 'givens' become objects of consciousness. In other words, phenomenon takes on the specialized meaning, 'an object *as it exists for* consciousness', and the study of phenomena leads to a delineation of the structure of the concrete experience in which objects are present to consciousness in various modes. Fourthly, phenomenology offers a method for the investigation of the field of consciousness, a method that never has specific *a priori* content expectations. Rather it is discovery oriented and its primary obligation is to describe what it finds by the use of certain procedures. Finally, one of phenomenology's key discoveries is the notion of intentionality as the essence of consciousness rather than awareness. Intentionality refers to the directedness of consciousness to other than itself, or strictly speaking, it refers to the fact that the objects of consciousness always transcend the acts in which they appear. It is a relationship that is other than causal and it forms the basis of the relationship between consciousness and world.

Jamesian Framework

Now, of course, the critical question is: Why should such a philosophy be helpful, or even important, for the science of psychology? I shall try to answer that question in the remaining time allotted me. I shall do it, however, within the framework posed by one of the leading philosophical-psychological thinkers at the time of psychology's founding, namely, the framework

provided by William James in his *Principles of Psychology* (NY: Dover, 1890/1950). It was the Dutch psychologist, Hans Linschoten (1968), who first pointed out the affinity between James' thought and the phenomenological perspective. I think that Linschoten's thesis does hold, and so while I shall use James' framework, I want to make clear that it will be fleshed out from a strictly phenomenological perspective.

James, you will recall, defined psychology as the study or science of mental life, and he then outlined a fourfold framework for the articulation of the proper object or subject matter, method, and scope of psychology. His schema was as follows: psychology could be understood in terms of four categories: (1) the psychologist, (2) the thought, feeling, or mental state studied, (3) the thought or mental state's object, and finally (4) the psychologist's reality.

These four factors are obviously interrelated, but for the sake of analysis I shall consider them singly. I'll begin with the psychologist who obviously is first a biographical person in the world and grows into, or is educated, to become a psychologist. No one is a born psychologist except metaphorically speaking. But what does it mean to be educated as a psychologist? What does it mean to look at the world and its phenomena from the perspective of a psychologist – merely one of hundreds of perspectives that human beings can assume?

In order to answer the last question, one must traverse through the thorny issue of the 'proper object' of psychology, and so we will turn to the second category introduced by James, the thought, feeling, or mental state studied. In the section of the *Principles* where James introduced the fourfold categories of psychology, he admitted having difficulty finding a generic term in English for the proper object of psychology, and so he vacillated between thought and feeling, even finding 'mental' to be restrictive. Ultimately, James chose the term 'experience' as the best term for psychological reality, but it clearly encompassed both thoughts and feelings.

However, in order to do justice to the phenomenological perspective, the third of James' four categories will have to be brought into the discussion. We saw that that was the thought's or feeling's object. For James, thought had a cognitive relation to its object and feelings implicitly referred to affective relationships with objects. In the phenomenological vision, consciousness and its object can be distinguished but not separated, and the relationship would not be essentially defined as either cognitive or affective, but intentional. Thus, a creature capable of consciousness is intentionally related to objects in the world and that would be a minimum condition for the psychologist to be able to constitute psychological reality.

Contextualization of Issue

Some contextualization is required for these last points. It is a matter, again, of specifying certain necessary preconditions for the appearance of psychological reality, and the minimum is that there be living creatures open to and aware of worlds. On the one hand, there cannot be a psychology of a stone or (despite California New Age ideology) the psychology of a crystal. Stones and crystals are things with only causal or external relations with their environments or other objects. Of course, there can be a psychology of the relationship of a person to a stone or a crystal, as when one makes an amulet or a fetish of them — but it is the person that brings the psychological dimension to the relationship. Thus, the minimum condition for the constitution of psychological reality is that a living sensory-motile creature establishes a relationship with an object in his or her world. Inter-personal or inter-creatural relationships obviously complicate matters and can also be the basis for psychological reality but they are not necessary in terms of minimal criteria.

If the scope of the psychological is to be properly delineated, then relationships that transcend the psychological on the upper level, so to speak, will also have to be specified. In other words, if cause-effect relations, as purely external, cannot be considered to be proper relationships in the realm of the psychical (since they can be conceived without relationship to the psychical), and more properly should be considered limiting conditions, are there other relationships given to consciousness that might also be considered to be metapsychological? Of course, in one sense, the issue begs the question since the answer depends upon how psychological reality is defined! But at the very least, one could say that logical relationships transcend the perspective of psychology. In other words, if the relationship of living creatures to objects in the world is minimally necessary for the constitution of psychological reality, it does not necessarily follow that all such relationships are intrinsically psychological. Indeed, the contemplation of the rules of a syllogism is qualitatively different as are conscious processes in which individual subjectivity is effaced in order to let the object present itself as it really is, that is, where genuine objectivity and universality are achieved. I would call these examples in which the psychological has been transcended. To say this another way, the scope of the psychological is between the biological and the logical. One represents the domain of life and the other the domain of reason and the psychological is the realm between them. In this view, the psychological would then be the domain of objects presented to consciousness as determined by individuated subjectivity. The extent to which the world is construed as relative to subjectivity would constitute the psychological. This means that in order to appreciate psychological reality, as such, one would have to assume a perspective that would thematize that domain.

Thus, if we return to the Jamesian schema, we would express the relationship between the second and third categories not as a cognitive relation between a mental state and its object, as James did, but rather as the intentional relationship between an individual subjectivity and its experiential object. This object could be either immanent or transcendent; if the former, it means that the object of consciousness belongs to the same stream of subjectivity as the act even if it transcends the act. In such a way are images, fantasies, dreams, and so forth, understood. If the object is the latter, that is, transcendent, then it means that the object is beyond the stream of a subject's consciousness but it is still for the subject in some subjectively dependent way. That is how one accounts for the physiognomies of the objects in the world (e.g., scary woods), for our idiographic perceptions (e.g., the ugly chair), our tastes (the attraction to Mozart), our prejudices (I prefer coffee over tea), and so forth. In brief, the psychological is the transcendent world construed as our personal, phenomenal world.

Another way of pursuing the issue of the boundaries of the discipline of psychology is to ask what is given in perception or to consciousness when 'psyche' is being thematized that, at the lower level, 'bios' in and of itself does not possess, and at the upper limit, what rationality possesses that makes it transcend 'psyche'. I would say that 'bios' deals with the sheer presence of life and of the processes and conditions that sustain life; whereas 'psyche' is present where worlds are given to creatures with sensory mobile abilities. But the worlds so constituted out of the geographic environment are relative to species and to social-cultural groups as well as to individuals. It is psychology's task to determine the various subjective dependencies of these worlds. That is, to determine the species dependencies, the social dependencies, or the linguistic dependencies, and so forth, that individual subjects express in the constitution of their worlds. Rationality, where it is present, expresses the ability to transcend the subjective dependencies that go into the constitution of worlds and to be able to be present to the world or object 'in itself'. In other words, with rationality, genuine objectivity and universality are possible because individuated subjective dependencies are transcended.

A second point to be contextualized is the way that one knows that one is in the presence of the psychological. If one were to stick strictly with the Jamesian concepts, thoughts and feelings, one would have to be limited to introspection. However, the proper object of psychology is not thought or feelings as such, but *expressed* thoughts and feelings. More precisely, psychology deals with the expressions of individual subjectivity as it construes its world. Thus, behavior, language, art, and so forth, could all be appropriate expressive modes of subjectivity that would be analyzed psychologically. Of course, this would also include the modalities subjects use to express their worlds to themselves, and in this sense, self-reflection is also a mode of expression.

This then brings us to the question of whether or not one has to use oneself or others as psychological subjects. But it really doesn't matter because here I would follow Merleau-Ponty and say that the structures of experience are neutral with respect to the distinction between inner and outer. Thus, whether one gets a description of an experience from others, or the researcher him or herself describes the behavior of another in a situation, should be indifferent to the description of the structure by means of which one tries to understand the concrete behavior or experience. However, a phenomenologist would not try to obtain an objective description by assuming an abstract, formal, objective posture prior to the concrete expressiveness that the psychological subjects would present. For example, while writing about introspection, James made clear that the introspective method was as objective as any of the methods of the natural sciences precisely because it never involved the use of personal intimacy with our own subjective processes. In other words, in order to introspect properly, one had to assume the attitude of the 'neutral observer' or the 'generalized other' toward oneself. But if the proper object of psychology is precisely the idiographic or personal subjectively dependent perceptions, memories, and so forth, then introspection as James understood it was precisely screening out what was most interesting for the discipline of psychology. The expressivity of the subject was being filtered in the name of objectivity. But another way of grasping the personal or subjective objectively is to let all of the expressiveness come out and then describe the structures that hold the relationship together in an objective way. In other words, objectivity is a way of grasping phenomena, not an outcome. It is not a matter of transforming subjectively based data into objective data, but precisely a way of grasping subjectivity as it expresses itself, that is, to grasp it in its subjectivity would indicate objectivity. Thus, one would let one's intimate connection with one's own processes be expressed and then one would try to re-express the initial expression in such a way that all other interested psychologists could see that the subjective expressions were not interfered with in the redescription. Similarly, one would describe the behavior of others in situations so that individual subjective meanings would be captured and then the scientific expression of the same data would consist of a structural description that would deepen the more naive subjective meanings previously expressed. In fact, this is what James did when he studied religious experiences and it is what Freud did in the development of psychoanalysis. Thus, one recognizes that one is in the presence of psychological phenomena when one perceives subjective dependent expressions that are revelatory of the worlds of the subjects being studied. Of course, implied in that statement are also categories that would be germane to psychological interests, such as the world of the paranoid, or the world of the depressed, or the perceptual world, and so forth. Psychology's interest need not be limited to idiographic worlds, although they certainly would not be excluded.

The third contextual factor is whether or not the essence of psychological phenomena can be articulated in a more positive way. We have established the boundaries as between 'bios' and logic, but still one could ask about possible unique characteristics of 'psyche'. Now, the claim is *not* being made here that in the history of psychology since 1879 there have been no good psychological studies or adequate understanding of psychological issues. I am only saying that when such knowledge has been produced it was not always clear just what made the studies good, nor would the community of psychologists agree on the same studies or theoretical conceptions as the best expressions of psychology. Thus, an understanding of 'psychical' is required that would be both comprehensive and precise. Comprehensive would mean that no legitimate psychological phenomenon would be excluded and precision would refer to the positive denotation of that feature that would be the basis of inclusion or exclusion. Thus, to further delineate the meaning of the psychological, I would say that it is intrinsically pararational or paraobjective. Since we are dealing not so much with the world as it is in itself, but as it is for the subjectivity that is relating itself to the world, and since we are seeking the expressed subjective dependent meanings in relation to the world, the meanings so grasped are contingent rather than necessary. 'The world for subjectivity' is a phenomenal world, a world that could be other than the way it is, it is a world dependent upon subjective needs, wishes, fantasies, expectations, desires, and so forth. It's a world of aspirations and reactions for reasons that are initially usually only partially transparent. That is why the clarity of reason or the necessary consequences of events have to be brought to bear as a balance to the urges and desires of psychological subjectivity, which would want the world to be exclusively in the service of its projects.

Implied in this discussion is the notion that the purely psychical as a separate realm never appears as such. That is, whenever psychical reality makes its appearance, it is always with a substratum of materiality. It is, then, what Husserl calls a dependent reality because it always appears in conjunction with materiality. I want to make this point explicit because the phenomenological position should not be confused with a mentalistic one. Mentalism is a position in which an abstraction, opposite to that of materialism, is made with the assumption that one can study the mental as such as though it were a special kind of substance. Phenomenologists recognize, however, that while pure physicality can be an independent reality, such as stones and crystals, the psychical is dependent. However, the dependent status of the psychical does not mean that it is reducible to the physical. Psychical reality offers more to the consciousness that perceives it than sheer materiality does.

Psychologist's Reality

We have now almost come full circle and we have to speak about the fourth category that James introduced, viz, the psychologist's reality. To say that a special perspective is required in order to constitute psychological reality has many theoretical implications. The first obvious implication is that the complex reality of everyday life to which we are spontaneously present is much richer than merely psychological. Thus, phenomenologists speak about the *Lebenswelt* or the everyday world into which we are all born and which forms the basis of all further specialized perspectives. Psychology, too, would be one of the specialized perspectives, or disciplines, motivated by a theoretical interest that would thematize an aspect of the world of everyday life for its own proper object. Of course, it would have to assume in this perspective an original viewpoint towards the world that is not duplicated by any other specialized discipline. The inability to describe in a theoretically and articulately sound way this original perspective is the single most glaring failure of psychology since it became a modern science.

A second implication of the notion that a psychologist's reality is the correlate of a special attitude is that psychological reality is not ready-made. We do not merely open our eyes and see the psychologist's reality as we see stones and trees. In this sense it may well be that James' term "psychologist's reality" may have to be improved upon. This would mean, however, that the psychologist's perspective results in a type of knowledge rather than a kind of reality. Here, I follow the thought of the Hungarian-born but French-trained Marxist philosopher, Georges Politzer. Politzer's (1928/1968) criticism of early psychological schools was that they were naively realistic. According to him, introspectionism defined psychology in terms of an 'inner reality' which could only be accessible to the experiencer and behaviorism defined psychology in terms of an external reality, which according to Politzer was merely the material basis for psychology, not psychology properly speaking. Thus, Politzer argued that psychology was rather a type of knowledge, a certain way of perceiving and understanding a complex reality. In other words, Politzer would say that psychology is constructed or interpreted. Now, to say the same thing phenomenologically is to say that psychological phenomena are constituted, but that does not mean that they are created. It means rather, as we said, that a certain complex event can support an analysis motivated by a unified theoretical perspective, which I have tried to characterize as the subjectively dependent and contingent world construals of individual subjectivities. It implies that meanings and values by which an individual subject lives his or her world with others is the kind of knowledge that psychology seeks.

The bestowal of these primary meanings on the world is so fundamental that scholars are often tempted to make psychology a primary discipline and this usually ends up being some form of psychologism. However, the perspec-

tive from which I am speaking does not make that assumption. The psychological perspective, however vital, is a derived perspective based upon a more primordial but complex set of experiences. As an aside, I would say that the same is true of all of the human sciences. Thus, social consciousness or political consciousness is a theoretical perspective brought to bear upon the complex pre-theoretical consciousness of the world. In the same way, a specialized perspective brings about specialized knowledge of a more complex reality.

Of course, if psychological phenomena were simply realities, rather than a way of understanding reality, then two fundamental types of errors that have been haunting the field would not have persisted as long as they did. I am speaking here of psychologism and the psychologist's fallacy. Psychologism states that the subjectively relative contingent facts are the best one can do with respect to ascertaining the truth value of the world. It posits, as primary, the derived specialized perspective of psychology and wishes to foist it upon the everyday world without mediation. In other words, it absolutizes a relative perspective. The psychologist's fallacy James himself brings up in conjunction with the psychologist's reality and it involves a confusion of standpoint. Sometimes the psychologist believes that the experiencer is experiencing the object of experience in the same way as the psychologist is, whereas how the experiencer is experiencing the object is precisely what has to be ascertained. In another form of the fallacy, the psychologist confuses the knowledge of the object, or logical necessities associated with the object with the experiencer's experience of the object. It seems to me that many models in contemporary psychology are also guilty of the psychologist's fallacy to the extent to which necessary logical steps replace contingent descriptions. In any event, it seems to me that if psychological phenomena were hard realities, these types of errors would not take place.

In brief, then, the vision being offered here is that of the psychologist being aware of the relationship between a subject and his or her world from such a perspective that the psychologist's understanding of that relationship can ensue. The psychological understanding of the relationship is derived from pretheoretical understanding that is more complex than the specialized attitude of the psychologist.

Psychology As Science

Since we have described and framed psychological phenomena, the only remaining question is whether such phenomena can be approached with a justifiable sense of science. It seems to me that this project is distinctly feasible, and the argument runs as follows. Science is an institution invented by humans for the production of knowledge. However, in order to claim scientific status for knowledge, it has to have certain characteristics, and in my perspective at least three such are necessary: scientific knowledge should be

methodical, systematic, and critical. Methodical means an intelligent, articu-
late access to a problem that is communicable and capable of being performed
by others; systematic means that relationships among various aspects of
knowledge would be discernible; and critical means that obtained knowledge
is not straightforwardly acceptable, but that it is challenged, first of all by the
researcher and then by the scholarly community. I do not see why such
knowledge about subjects in situations cannot be obtained.

De facto, however, what makes the project problematic is that the word
'science' and the terms 'methodical, systematic, and critical' evoke historically
laden associations with the practice of the natural sciences and thus ways of
implementing the criteria for scientific knowledge are suggested by such
practices. Because, however, the mode of being of subjectivity in relation to
the world is so different from the mode of being of things, phenomenologists
recognize that the practice of scientific psychology, if it is to be faithful to the
proper object of psychology, will have to be different precisely in order to
meet the criteria of scientific knowledge. Thus, since the subject matter of
psychology, the subjectively dependent world construals, is intentional and
not logical, it can be neither strictly caused nor strictly deduced, thus the
initial access has to be through description. Moreover, since it is meanings,
especially expressed lived meanings, that are being sought, rather than sheer
facts, inductive processes are also limited. Instead, the phenomenological
goal of seeking eidetic meanings as a way of comprehending many lived
meanings are sought through the process of free imaginative variation. The
eidetic meanings organized into structures would then throw light on the
concretely lived descriptions, clarifying them in a way that would be inacces-
sible to a lifeworld perspective. Thus, the method is descriptive, based on
criteria of descriptive science; qualitative, because meanings of experience are
being sought, rigorous, because 100 percent of the data always have to be
accounted for and the analysis is open for critical inspection by others; and
finally, psychologically relevant because intentional relationships are under-
stood primarily in terms of the meanings bestowed through them by con-
sciousness. Finally, while I cannot demonstrate the fact here, my claim is that
all historical schools of psychology can be comprehended by this vision, but of
course, in a modified way.

References

Ladd, G. T. (1892). Psychology as a so-called "natural science". *Philosophi-
cal Review, 1,* 24-53.

Linschoten, H. (1968). *On the way toward a phenomenological psychology:
The psychology of William James*. Pittsburgh, PA: Duquesne University
Press.

Politzer, G. (1928/1968). *Critique des fondements de la psychologie*. Paris:
Presses Universitaires de France.

FOR A MATERIALIST PSYCHOLOGY

Charles W. Tolman

SUMMARY: We believe our bodies to be real and natural, that they exist in a
particular way in objective time and space without our having to think about
them, and that if we collectively put our minds to the task, we can know and
understand them as they actually function, just as we know the world in which
it all takes place. In short, when we examine the assumptions underlying
common everyday existence and action we discover something very close to the
assumptions of philosophical materialism. It is important to emphasize that
materialism does not deny the mental or spiritual aspects of our lives. The
mental is regarded as fully real, but the material is prior. This priority, however,
must be understood developmentally. It is certainly not the case that thoughts
never precede or have effects upon material processes. On the contrary,
thoughts and other mental phenomena almost always affect material processes.
But these same mental processes have their ultimate origin in material, usually
biological processes. They are themselves material processes of a developmen-
tally special kind.

Introduction

The very fact that we are here to discuss theoretical psychology testifies
to our being at least implicit materialists. We all believe that there is a place
from which we have come which is different from this one and that by
organizing the right kind of transportation we have managed to get from there
to here. And when we are ill, most of us go to our physicians in hopes that the
collective medical understanding of our human physiology, pharmacology,
and the like will indicate an intervention that will restore our health. This
means that we believe our bodies to be real and natural, that they exist in a
particular way in objective time and space without our having to think about
them, and that if we collectively put our minds to the task, we can know and
understand them in their natural functioning and the world in which it takes
place. In short, when we examine the assumptions underlying common
everyday existence and action we discover something very close to the assump-
tions of philosophical materialism.

Cornforth articulated these assumptions in the following way:

1. Materialism teaches that the world is by its very nature
   material, that everything which exists comes into being on
   the basis of material causes, arises and develops in accord-
   ance with the laws of the motion of matter.

2. Materialism teaches that matter is objective reality existing
   outside and independent of the mind; and that far from the

mental existing in separation from the material, everything
mental or spiritual is a product of material processes.

3.   Materialism teaches that the world and its laws are know-
     able, and that while much in the material world may not be
     known there is no unknowable sphere of reality which lies
     outside the material world (Cornforth, 1971, p. 25).

Even where educated people in modern industrial society prefer to state
the assumptions less strongly, and even if they would rather believe in a more
spiritual element in life, they continue to *act* like materialists and they seldom
consciously forego the advantages of the technical knowledge based upon
materialist assumptions, like using transportation and communication sys-
tems and putting themselves, when necessary, into the hands of a scientifically
informed physician.

It is important to emphasize that materialism does not deny the mental
or spiritual aspects of our lives. It can hardly appeal to the evidence of
experience regarding physical, chemical, and biological phenomena, and then
discount it with respect to psychological phenomena. What it asserts is simply
that "the spiritual is a product of the material" (Konstantinov et al., 1982,
p. 20) and not the other way around. This is equivalent to asserting that there
was a time in the evolutionary history of the world when thinking beings did
not exist, but that they arose at some point by natural processes, or that
thinking is real but depends upon the brain and some aspects of it can be
reproduced in machines. In short, the mental is regarded as fully real, but the
material is prior. This priority, however, must be understood development-
ally. It is certainly not the case that thoughts never precede or have effects
upon material processes. On the contrary, thoughts and other mental
phenomena almost always affect material processes. But these same mental
processes have their ultimate origin in material, usually biological, processes.
They are themselves material processes of a developmentally special kind.

Shortcomings of Spontaneous Materialism

Now if things are as simple as this, if every consciously acting human
being is in some fundamental sense a philosophical materialist, why is the
history of philosophy and science replete with alternatives to materialism?
Obviously things are not so simple. Volumes could be written on this, so let
me come directly to what I believe to be the central problem. Things often
seem to us different than they are. In the language of Kant, things-for-us
appear to be fundamentally different from things-in-themselves. When we
look at a book, we know it is a book. If we try to apprehend it as simply matter
organized in some particular way we generally fail. The book, like most
objects in our experience, has immediate meaning for us, yet that meaning
cannot be found in and dissected from the book itself. This has traditionally

led to an idealist distortion of our spontaneous materialism. Some quality of mind, not matter, is postulated as essential to meaning and, therefore, to our knowledge of the world, which is now limited to mere 'phenomenon'. In this way idealism appears to afford the theory of knowledge that materialism utterly fails to yield. But the idealist solution is not a happy one in the long run. It has led inevitably to skepticism, relativism, and finally solipsism. It thus leaves the problem of knowledge to appear as unsolvable and thoroughly intractable.

What is the source of the difficulty? In one word, it's 'dialectics', or, rather, the lack of it: "... ignorance of dialectics", writes Ilyenkov (1982a, p. 113), "was the catastrophe leading to the degeneration of the spontaneous materialism of natural scientists — their 'natural' epistemological position — into the most vulgar and reactionary varieties of idealism and clericalism ..." "Without dialectics, materialism invariably proves to be not the victor (or a militant), but the vanquished, that is, it inevitably suffers a defeat in the war with idealism" (Ilyenkov, 1982a, p. 143).

The dialectical failures of spontaneous materialism are many. I will concentrate here on those relating to the relationships between the subject and object, and between the abstract and the concrete. The mechanical character of naive materialism has, for instance, encouraged the conceptualization of subject and object as separate systems causally related to one another (Lektorsky, 1984). Meaning thus became the effect in the subject of causes originating in the object. The doctrine of secondary qualities is the best known expression of this idea. Idealism necessarily results when knowledge is equated to the effects of stimulation by the object. Carried to its logical conclusion, as Hume demonstrated, it becomes impossible to make knowledge claims about anything outside one's own sensations and the 'external' world becomes merely an inference or construction. This unsatisfactory consequence is overcome by a dialectical approach to the problem.

Naive materialism has also tended to equate knowledge with the abstract and its object with the concrete. The unfortunate result of this is that if knowledge is to become more general, which is the universally acknowledged aim of science, it must become more abstract and thus less informative and relevant to real concrete problems. A further consequence of this is that it allows the apparently same empirical events to be abstracted differently, leading to differing theoretical accounts.

Spontaneous materialism has proved itself utterly incapable of resolving these differences and has thus been unable to deliver on its promise of an ultimately monistic account of the singular material universe. This has understandably led to the acceptance, even advocacy, of relativism, eclecticism, and pluralism, all forms of subjective idealism.

Materialist Dialectics

Before proceeding, it will be necessary to say something about dialectics in general, which, in Engels' words, comes down simply to "[t]he great basic thought that the world is not to be comprehended as a complex of ready-made *things*, but as a complex of *processes*" (Engels in Selsam & Martel, 1963, p. 100). The meaning of this for social questions was put succinctly by Lenin:

> What Marx and Engels called the dialectical method — in contradistinction to the metaphysical method — is nothing more or less than the scientific method in sociology, which consists in regarding society as a living organism in a constant state of development (and not as something mechanically concatenated and therefore permitting any arbitrary combination of individual social elements), the study of which requires objective analysis of the relations of production that constitute the given social formation and an investigation of its laws of functioning and development. (Lenin in Selsam & Martel, 1963, p. 110).

Elsewhere Lenin summarized the methodological implications of dialectical logic as follows:

> In the first place, in order really to know an object we must embrace, study, all its sides, all connections and "mediations." We shall never achieve this completely, but the demand for all-sidedness is a safeguard against mistakes and rigidity. Secondly, dialectical logic demands that we take an object in its development, its "self-movement" (as Hegel sometimes put it), in its changes.... Thirdly, the whole of human experience should enter the full "definition" of an object as a criterion of the truth and as a practical index of the objects' connection with what man requires. Fourthly, dialectical logic teaches that "there is no abstract truth, truth is always concrete," as the late Plekhanov was fond of saying after Hegel ... (Lenin in Selsam & Martel, 1963, p. 116).

Anything studied in its all-sidedness, its development, its self-movement, that is, as a process, soon exposes our need to grasp the nature of dialectical contradiction. As Engels noted: "As long as we consider things as static and lifeless, each one by itself, alongside of and after each other, it is true that we do not run up against any contradictions in them" (Engels in Selsam & Martel, 1963, p. 117). "But the position is quite different as soon as we consider things in their motion, their change, their life, their reciprocal influence on one another. Then we immediately become involved in contradictions" (Engels in Selsam & Martel, 1963, p. 117). The example he gives is that of mechanical motion which, as the ancient Greeks understood (compare the paradoxes of Zeno of Elea), can only come about through "a body at one and the same moment of time being both in one place and in another place, being in one and the same place and also not in it" (Engels in Selsam & Martel, 1963, p. 118; cf also Marquit, 1982).

A useful psychological example of the point being made here is found in John Dewey's well-known (though often misunderstood) essay on the reflex-arc. In this essay, Dewey argued that the mechanical conception of stimulus-

response needed, for the sake of accuracy, to be replaced by a more processual one. The mechanical conception was one dominated by:

> ... rigid distinctions between sensations, thoughts and acts. The sensory stimulus is one thing, the central activity, standing for the idea, is another thing, and the motor discharge, standing for the act proper, is a third. As a result, the reflex arc is not a comprehensive, or organic unity, but a patchwork of disjointed parts, a mechanical conjunction of unallied processes. (Dewey, 1896, p. 358).

(Note the similarity of Dewey's language to that of Engels and Lenin.)

In the familiar example of the child and the candle, the usual account was one of stimulus followed by response. But Dewey found that:

> ... we begin not with a sensory stimulus, but with a sensori-motor coordination, the optical-ocular, and that in a certain sense it is the movement which is primary, and the sensation which is secondary, the movement of body, head and eye muscles determining the quality of what is experienced. In other words, the real beginning is with the act of seeing; it is looking, and not a sensation of light. The sensory quale gives the value of the act, just as the movement furnishes its mechanism and control, but both sensation and movement lie inside, not outside the act. Dewey, 1896, p. 358).

The act is, in short, the dialectical unity of these two opposites.

The end result of the act is "an enlarged and transformed coordination," "not a substitution of a motor response for a sensory stimulus." Dewey was fully aware of the contradictory nature of this process: "The burn *is* the original seeing, the original optical-ocular experience enlarged and transformed in its value" (Dewey, 1896, p. 359, emphasis added; cf. Hegel, 1975, p. 167).

There are other 'classical' examples in psychology, such as James' conception of self as the dialectical unity of identity and difference (in contrast to Hume who rejected the concept of self because the empirical differences appeared to logically exclude the possibility of identity) (Tolman, 1989). A particularly accessible discussion of the methodological implications of dialectics is found in Vygotsky's *Mind in Society* (Vygotsky, 1978, chap. 5). Dialectics is not an esoteric doctrine. It has been with us nearly from the beginning of modern psychology in the late 19th century, even where it has not been consciously formulated as such. As Lenin might have said, it is to be found wherever psychological phenomena have been subjected to sound scientific thinking.

Subject and Object

Our task now is to see how dialectical thinking helps us to overcome the methodological problems of spontaneous materialism, the most serious of which is found in our understanding of the subject-object relationship. Although the subject-object relationship embraces a large number of par-

ticular aspects or problems, it is specifically the problem of cognition, the relation of knower to known, that is most urgent for theoretical psychology. Psychology has in fact a double interest in this. First there is the scientific question of the cognitive process itself, and, second, there is the question of the cognitive status of scientific theories, including psychological ones. The materialist position is, of course, that a determinate scientific theory of the cognitive process (or any other process or object) is in-principle possible. How then?

We must begin by clarifying what is meant by 'subject' and 'object'. Neither simply exists in some primordial sense. They are both the outcomes of processes. The individual human being must *become* a subject just as things must *become* objects. This happens when the human organism enters into an active (acting upon) relationship with the thing. Subject and object emerge as opposing poles of the developing activity that links the individual human being with things, their properties and relations. In this activity the object is altered, as when things are moved by the probing motions of a child's hand. At the same time the child as subject is also altered: it gradually becomes conscious of itself as an initiator of motion. In this sense the subject creates itself through its active, altering appropriation of things as objects.

Now when we consider the context of this development of the individual subject, it is clear that the things and activities available to it are those provided by other human subjects. In a very real sense the individual human subject develops only through participation in the collective subject. As Konstantinov et al. (1982, p. 152) have put it: "... individuals can be the subjects of cognition only thanks to the fact that they enter into certain social relations with one another and acquire the instruments and means of production accessible to them at a given level of social organization." This means, on the one hand, that individual cognition is largely 'cognition through others'. It does not depend upon the direct experience of the individual subject. It also means that what a subject can cognize, that is, what can potentially become an object for it, is determined by the state of social and historical development of its collective subject. For example, only in a society in which scientific activity is well developed can the electron become an object of common cognition.

An important aspect of the human subject, both individual and collective, is its capacity for reproducing its objects, their properties and relations, in thought, that is, on the plane of the ideal (Dubrovsky, 1988; Ilyenkov, 1977). This is carried out largely with the use of language which provides the signs and symbols with which the reconstruction is made. These reconstructions or thoughts also become objects and can be altered by appropriate instruments and activities. This is the basis of the subject's creative influence upon objects in the real world. But practical and creative activity with respect to objects, which is characteristic of all historically evolving production,

would be impossible if the objective properties of the object world were not reflected in the subject: "Knowledge can be an instrument of transformation of the world only when it is an objective and active, practically oriented reflection of reality" (Konstantinov et al., 1982, p. 157). This makes evolutionary sense in that cognition must be an outgrowth of natural processes of interaction in which one thing impresses itself upon another, and such a process would only have developed under the selective pressure of external objective reality. Human activity guided by an essentially nonobjective cognitive process could neither be appropriate to the demands of objective reality nor have been shaped by it.

In the formation of the subject's ideal reflection of reality, it is important to distinguish two kinds of knowledge, sensory and logical (Kharin, 1981; other authors use other labels such as perceptual and conceptual, empirical and inferential, or empirical and theoretical). In sensory knowledge the reflection of objects is direct and there is a distinct limit to the properties and relations of a thing that can be reflected in this way. Sensory knowledge is, however, the absolutely essential basis of all other knowledge. As expressed by Kharin: "It is only on the basis of sense perception that factual material is accumulated which forms the groundwork for theoretical generalizations that help [us to] discover the laws of nature and society" (1981, p. 217). Most knowledge that is important to modern human existence, however, is nonsensory or logical. It is knowledge of what is not immediately given in sensation. We need merely think of what knowledge of molecular structure of materials means to our lives. Such knowledge is indirect in two ways: first, it is for most of us, as individuals, societal or collective in origin; second, even for those individuals who are directly engaged in producing such knowledge it is produced by inference from the observable effects of experimental activities, not from the direct perception of molecular structures themselves.

Both kinds of knowledge are objective reflections of reality. Molecular structure is as objectively real as the instrument readings from which it is inferred. This is also the case for objects which are heavily loaded with meaning such as a book. To apprehend the object before us as a book, and not just as a particular configuration of immediately-given physical properties is to reflect it in all its social and historical relations which give it its meaning. Meanings are thus not projected upon objects but apprehended in them, that is, meanings are essentially objective.[1] We cannot make of things just anything that we want: within the objective relations in which we live a book is a book and a hammer is a hammer. And we had to learn this from others as we grew up.

Now none of this implies that we as subjects cannot be mistaken, either at the sensory or the logical levels. There is ample opportunity for this at both

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[1] This does not preclude the existence of "personal sense" that may be quite subjective (Leontyev, 1981, pp. 227ff., see also Jantzen, in press).

levels. And even where we are not mistaken, that is, where our reflections are not distorted, the infinite nature of things will ensure that even our objective knowledge will be incomplete. The truth of our propositions will, therefore, be relative only, not absolute. Lenin put it succinctly in his "Conspectus of Hegel's *Science of Logic*:"

> Knowledge is the reflection of nature by man. But this is not a simple, not an immediate, not a complete reflection, but the process of a series of abstractions, the formation and development of concepts, laws, etc., and these concepts, laws, etc. (thought, science = "the logical Idea") *embrace* conditionally, approximately, the universal law-governed character of eternally moving and developing nature (Lenin, 1976, p. 182).

There is much more than can and needs be said about the subject-object relationship. It all points, however, to one conclusion: the dialectical understanding of this relationship yields the materialist theory of cognition that spontaneous materialism failed to yield. This theory is internally coherent; it affirms the common sense intuition that we can and do know the world; and it is the only theory that is entirely consistent with our actual societal-historical practice.

Abstract and Concrete

It has been the source of great puzzlement to many materialist thinkers in psychology that claims to objectively true empirical knowledge have not led automatically to unambiguously determinate theories (e.g., Hilgard & Bower, 1966, p. 582f). It has become apparent that this problem cannot be solved by an explication of the subject-object relationship alone or of the cognitive processes in general. The source of the difficulty lies in a very particular aspect of the cognitive process, the dialectic of the abstract and concrete in thought.

An example of the customary understanding of the abstract and concrete can be found in the well-known psychological dictionary by English and English (1958). Here "abstract" means "characterizing any quality of something considered apart from the thing itself" (p. 3) and "concrete" means "pertaining to a specific item or thing, as a whole; characterizing an individual fact at a particular moment; the opposite of abstract" (p. 106). Considering the apparent identification of the concrete with the particular, it is not surprising to learn that "the abstract idea is also a general idea, and the often-used term abstract general idea is somewhat redundant" (p. 3). This view has dominated bourgeois scientific thinking at least since Locke and received its clearest articulation in the work of J. S. Mill. Ilyenkov summarized this 'standard view' as follows:

> The concrete is that which is immediately given in individual experience as an "individual thing", an individual experience, and a concrete concept is a verbal symbol that may be used as a name of an individual object. That symbol which cannot be used as a direct name of an individual thing is "the

abstract".... "The abstract" is here consistently treated as everything that is
not given in individual experience as an individual thing and cannot be
defined in terms of those types of objects that are given in experience, cannot
be a direct name of individual objects.... (Ilyenkov, 1982b, p. 31.)

What is wrong with this view from a dialectical point of view follows from
the principle of the unity of opposites. That is, if abstract and concrete are
true opposites then they must be taken as correlative. We have seen this in
our consideration of subject and object. The one develops with the other and
is an expression of the other: while the subject and object are distinct, they do
not exist without each other. The methodological implication of this is clear:
we cannot study one without the other, but only in terms of the other. This is
also true of quality and quantity, chance and necessity, content and form,
essence and appearance, and so forth.

What the standard view of concrete and abstract attempts to do is to
separate them, put them into different places. The thing out there is concrete,
while the idea in the head is abstract, or, if in the head, sensuous knowledge
is concrete, while logical, theoretical knowledge is abstract. Following the
dialectical principle, nature, sensuous experience, and theoretical concepts
must each be both concrete and abstract. The concreteness of a thing, of a
sensuous experience, or of an idea, can only be correlative to its own abstract-
ness.

This leads us to a new definition of the two categories. Marx defined the
concrete as "the concentration of many determinations, hence unity of the
diverse" (1973, p. 101). What this means for a concept is that it is abstract "to
the extent to which it reflects the separateness, isolation and specificity which
are objectively inherent in things" and it is concrete "to the extent to which it
reflects the integration, unity and mutual complementarity of things"
(Natelov, 1984, p. 275). One methodological result of this is that at least two
distinct kinds of generalization can be identified: (1) abstract generalization
based upon common characters of diverse entities (e.g., all mammals have
hair), and (2) concrete generalization that seeks to discover the essential unity
of something (the essentially human has its origin in labor) (cf. Gorsky's
"analytic" and "synthetic" modes of generalization, 1987; see also Davydov,
1984).

Now why should this be important for the development of a specifically
materialist psychology? The answer is that if our psychological scientific
activity is guided by a methodology that recognizes theoretical concepts,
propositions, and laws as mainly abstract, and if generalization is equated with
abstractness, then our theories are likely to remain abstract only. The prob-
lem with this, aside from the fact that abstract knowledge proves to be
inherently unsatisfying, is that any number of abstractions are possible from
any particular material phenomenon, with the result that any number of
empirically verifiable theories, many apparently contradictory (as with be-
havioral and cognitive theories of learning, or trait and situational theories of

personality), can be abstracted. The necessary end-result is an irremediable theoretical indeterminacy. This is particularly serious for materialism because it leads inevitably to an idealist (often subjective idealist) treatment of theoretical issues. This, in turn, undermines the original materialist assumptions that led to scientific practice in the first place by putting into question the possibility of unambiguous and complete objective knowledge of the object of investigation.

The alternative, dialectical methodology recognizes the value of the initial abstractions from empirical phenomena, but specifies its aim as the eventual concretization of the abstract through identification of concrete universals. In Natelov's words:

> The universal is not equivalent to the similar represented in each individual object and regarded as their common feature. It is, first and foremost, a law-governed relationship of two or more individuals in which they pose as the moments of one and the same concrete and real, and not only formal, unity. According to Hegel, whose view was also shared by Marx, the form of universality is a law or the principle of connection of details within a whole which is totality. The universal can only be obtained through analysis, and not through abstraction (Natelov, 1984, p. 297).

The common example is labor as the concrete universal for the human species. It is this evolutionarily peculiar form of the subject-object relationship that forms the key to a concrete understanding of what is distinctly human (language, consciousness, societal organization, etc.) and it does this, in part, by allowing us to sort out which of all the possible abstractions are essential and which are not (e.g., the form of the human hand is essential, but soft ear lobes are not; cf. Ilyenkov, 1982b, pp. 62-70).

It is only this methodological 'rising from the abstract to the concrete', based upon a dialectical understanding of the abstract and concrete, that permits the ordering of abstractions in terms of essentiality and relevance needed for the resolution of theoretical differences. It is, therefore, the key to producing the unambiguous theories, subject, of course, to relative ignorance, that are expected of materialism.

Materialist Psychology

The implications of the above considerations (and others like them) are profound for a materialist psychology, for both its subject matter and its methodology. It should be apparent, for example, that a subject matter that is defined abstractly and non-dialectically, one that ignores the concrete universal, will be seriously limited. This is the case (which is easily substantiated historically) with psychologies of consciousness, whether as act or content, and even with materialistically intended psychologies of response and behavior. As well, methodologies that are based exclusively upon abstraction, as all statistically oriented ones are, will necessarily fail to discover the

concrete essentiality that comes only from a dialectically-informed theoretical analysis.

It is not fortuitous, therefore, that dialectical materialist psychology specifies its subject matter as 'activity'. According to Leontyev:

> The importance of this category [activity] hardly needs to be emphasized. We need only recall Marx's famous theses on Feuerbach, in which he said that the chief defect of earlier metaphysical materialism was that it viewed sensuousness solely as a form of contemplation, not a human activity or practice. Therefore, the active aspect of sensuousness was developed by idealism, the opposite of materialism. Idealism, however, understood it abstractly, not as the real activity of man (Leontyev, 1979, p. 41).

But what is this 'real activity of man'? It includes but is a long way from identical with the activeness that merely opposes passiveness. Again in Leontyev's words:

> Activity is the nonadditive, molar unit of life for the material, corporeal subject. In a narrower sense (i.e., on the psychological level) it is the unit of life that is mediated by mental reflection. The real function of this unit is to orient the subject in the world of objects. In other words, activity is not a reaction or aggregate of reactions, but a system with its own structure, its own internal transformations, and its own development (Leontyev, 1979, p. 46).

This is obviously a psychology of the subject-object relationship, dialectically understood, in its peculiarly psychological form. Leontyev (e.g., 1981) and others have gone on to elaborate a complete theory of the 'internal transformations' and of the phylogenetic, historical, and ontogenetic development of activity that has been receiving increasing attention in the West in recent years.[2] It is obviously impossible to go further into detail here.

Perhaps the most important single methodological implication is one that confirms what many psychologists have come increasingly to recognize in recent years, and of which the formation of the International Society for Theoretical Psychology and of Section 25 — History and Philosophy of Psychology — of the C.P.A. are symptomatic. This is that generalization based upon abstraction from empirical data alone is helpless to resolve important theoretical problems. What is missing is a specifically theoretical methodology. Hilgard and Bower (1966) appear to have had something like this in mind when they wrote that: "Accumulation of knowledge means neither mere fact-gathering nor isolated hypothesis-testing, but thoughtful systematic approaches to meaningful questions leading to *conclusive thinking*" (p. 583). This "conclusive thinking" is the *analysis* alluded to above which goes beyond empirical abstraction. What is the nature of this theoretical methodology? Details would obviously take us well beyond present limits of time and space

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[2] See the new journal *Activity Theory*, edited by G. Rueckriem (Berlin), C. Tolman (Victoria) and V. Lektorsky (Moscow). Information and subscriptions can be obtained from Professor Georg Rueckriem, Institut fuer Allgemeine Paedagogik, Hochschule der Kuenste, 1000 Berlin 15.

(a useful and brief introduction is found in Davydov, 1984; see also Engestrom, Hakkarainen & Hedegaard, 1984). It is apparent, however, that it is a methodology based upon a dialectically revised understanding of scientific cognition. As we have already seen, this requires new understandings of the nature of the empirical and the theoretical, of notions and concepts, of the process of generalization, of subject and object, and of the concrete and the abstract. All of this leads eventually to the kind of effective mental reconstruction (or re-modelling) of reality such as to permit the apprehension of the deep structure of psychological processes in the way that we now apprehend the molecular structure of matter.

In conclusion, it is undoubtedly true that the usual spontaneous, naive, and mechanistic forms of materialism cannot produce a satisfactorily coherent theory of psychological processes. A dialectical analysis of its categories, however, provides the breakthrough needed for such a possibility. Given the genuinely dialectical understanding of subject and object and of the concrete and the abstract, the way is clear for a complete revision (or replacement) of 'bourgeois' psychology with very promising prospects for psychological theory that will more adequately reflect the essence of its object than any other theory before it.

References

Cornforth, M. (1971). *Materialism and the dialectical method.* New York: International Publishers, Inc.

Davydov, V. V. (1984). Substantial generalization and the dialectical materialistic theory of thinking. In M. Hedegaard, P. Hakkarainen, & Y. Engestrom (Eds.), *Learning and teaching on a scientific basis* (pp. 11-32). Aarhus: Aarhus Universitet.

Dewey, J. (1896). The reflex arc concept in psychology. *Psychological Review, 3,* 357-370.

Dubrovsky, D. (1988). *The problem of the ideal.* Moscow: Progress Publishers.

Engestrom, Y., Hakkarainen, P., & Hedegaard, M. (1984). On the methodological basis of research in teaching and learning. In M. Hedegaard, P. Hakkarainen, & Y. Engestrom (Eds.), *Learning and teaching on a scientific basis* (pp. 119-190). Aarhus: Aarhus Universitet.

English, H. B., & English, A. C. (1958). *A comprehensive dictionary of psychological and psychoanalytic term.* New York: Longmans, Green & Co.

Gorsky, D. (1987). *Generalisation and cognition.* Moscow: Progress Publishers.

Hegel, G. W. F. (1975). *Hegel's logic.* (W. Wallace, Trans.). Oxford: Oxford University Press.

Hilgard, E. R., & Bower, G. H. (1966). *Theories of learning.* New York: Appleton-Century-Crofts.

Ilyenkov, E. B. (1982a). *Leninist dialectics and the metaphysics of positivism.* London: New Park Publications.

Ilyenkov, E. V. (1982b). *The dialectics of the abstract and the concrete in Marx's Capital.* Moscow: Progress Publishers.

Ilyenkov, E. V. (1977). The concept of the ideal. In Fedoseyev, et al. (Eds.), *Philosophy in the USSR: problems of dialectical materialism.* Moscow: Progress Publishers.

Jantzen, W. (in press). The evolution of subjective sense. *Activity theory.*

Kharin, Y. A. (1981). *Fundamentals of dialectics.* Moscow: Progress Publishers.

Konstantinov, F. V., et al. (Eds.). (1982). *The fundamentals of Marxist-Leninist philosophy.* Moscow: Progress Publishers.

Lektorsky, V. A. (1984). *Subject, object, cognition.* Moscow: Progress Publishers.

Lenin, V. I. (1976). *Philosophical notebooks* (Collected works, Vol. 38). Moscow: Progress Publishers.

Leontyev, A. N. (1979). The problem of activity in psychology. In J. V. Wertsch (Ed.), *The concept of activity in Soviet psychology* (pp. 40-71). Armonk, NY: M. E. Sharpe, Inc.

Leontyev, A. N. (1981). *The problem of the development of the mind.* Moscow: Progress Publishers.

Marquit, E. (1982). Contradictions in dialectics and formal logic. In E. Marquit, P. Moran, & W. H. Truitt (Eds.), *Dialectical contradictions: contemporary Marxist discussions.* Minneapolis: Marxist Educational Press.

Marx, K. (1973). *Grundrisse* (M. Nicolaus, Trans.). New York: Vintage Books.

Natelov, I. (1984). *Alternatives to positivism.* Moscow: Progress Publishers.

Selsam, H., & Martel, H. (1963). *Reader in Marxist philosophy.* New York: International Publishers, Inc.

Tolman, C. W. (1989). Pluralistic monism: William James as closet-Heraclitean. *Psychological Record, 39,* 177-194.

Vygotsky, L. S. (1978). *Mind in society.* Cambridge: Harvard University Press.

REALITIES AND THEIR RELATIONSHIPS

Kenneth J. Gergen

SUMMARY: From a social constructionist standpoint, foundational ontologies, such as materialism and phenomenology are hammered out of the discursive resources of the culture. Both generate their 'sense of reality' through rhetorical procedures. Thus, problems in epistemology are superfluous byproducts of simultaneously accepting two or more ontological posits (e.g., world and mind). In the constructionist view, 1) there is no transcendental means of justifying any given ontology, 2) new realms of reality are open to construction, and 3) important questions must be raised regarding the pragmatic consequences of competing reality posits.

To declare the reality of an entity is to make a powerful statement. The declaration can serve in the rhetorical capacity of a medalion to be worn with honor, an amulet to protect one from danger, a signal to call comrades to arms, a quest for a lifetime, a fortification against forces of error. Reality is a curious word, equated in 13th century English with the term 'regal'. To be real was, in many cases, just another way of saying 'royal'. Reality, such an essential word to our efforts as scientists, philosophers, theologians, political leaders, and industrial and military specialists — and yet so very difficult to locate. In the present offering I shall sketch out a social constructionist view of reality — or more precisely, of multiple realities. In doing so I shall first attempt to locate my fellow protagonists in this dialogue, namely representatives of both the materialist and phenomenological accounts of the real. This conceptual archeology will place such endeavors in a larger context of discourse. Then, with this expanded context in hand, it will be possible to speak more directly to three major issues surrounding debates on competing foundations of inquiry. Specifically, we can consider (1) limits to our conceptions of the real, (2) justification for choice of grounding ontology, and (3) the future of foundationalist controversies.

Construction and a Congery of Realities

Reality is a seductive word, so compelling to our interchange and so comforting when possessed. But what precisely is meant by the term? To what does it refer? Such a term could not be derived inductively from what there is, for any definition of what there is would itself presuppose a conception of the real. To suppose that our concept of 'trees' derived from the real existence of trees would already be to presume their reality. Observation cannot be trusted to inform us of the nature of reality, for the definition of observation would again presume a conception of the real — including the nature of observational processes and their supposed objects. To have a

theory of retinal stimulation and its relationship to ambient light, is already to presume the reality of retinal stimulation and ambient light. Ostensive definition is not serviceable in the present case, for the rudimentary task of pointing to (or 'dubbing') what we mean by reality presupposes that the observer knows what is implied by the act. To extend my arm to the east and utter a set of nonsense syllables does not inform one of what is intended unless there is already a forestructure of understanding available from which to make an interpretation. Why is it then that such great confidence is so often attached to claims for having discovered, apprehended, carefully researched, or corrected others' errors regarding what is real?

For a social constructionist the meaning of terms such as 'reality' cannot in principle be derived from what is the case. Words for the constructionist are not mimetic simulacres of an independent world, but derive their meaning (*à la* Wittgenstein) from their usage. Because this usage is pre-eminently social, one may thus view metatheories, theories and brute reality descriptions — all linguistic formulations — as deriving their meaning and implications from particular communities of persons engaged in particular patterns of relationship. Conceptions of the real, on this account, are essentially constituents of elaborate linguistic codes. These codes are shared within various language communities, and to the extent that they are central to their activities (e.g., relied upon for co-ordinating actions and/or rationalizing their activities) they will achieve ontological legitimacy. Their terms will take their place as constituents of the 'taken for granted' world of everyday life.

To be more explicit, conceptions of the real are typically (1) embedded within an elaborate linguistic code (including inter-related definitional and propositional networks), (2) dependent on social communities able and willing to share these conventions, and (3) interdependent with an array of practical activities which are facilitated and supported by these conceptions. In this context, it is useful first to assay a range of traditional formulations of the real, or contenders for the status of first ontologies. What is to be said of the existing range of realities available for theoretical use? We may then consider the relationship among these various realities, and then finally, draw several conclusions of more general concern to the process of metatheoretical debate and its relationship to psychological inquiry.

The Material World. Thinkers from Anaximander to Marx have proposed that reality is quintessentially material. In Cornforth's (1971) words, "The world by its very nature is material." Or in a logical empiricist mode, terms failing to refer ostensively to concrete substance, are essentially metaphysical and thus unworthy of serious concern to those seeking knowledge. It is also the materialist view that has inspired an array of behaviorally oriented psychologists concerned with charting the relationship between observable antecedents and behavioral responses.

Yet, though most scientists, public officials, medical practitioners, military strategists and the like agree that their chief concern is with events in the material world, there is little agreement about what precisely constitutes such a world. As one commentator (Johnson, 1973), viewing the array of materialist conceptions offered over the centuries, has summarized it, "the concepts of matter in the Western tradition exhibit bewildering confusion" (p. 185). For some, matter is essentially inert mass (Newton), while for others it is inseparable from motion and action (Einstein); for Descartes matter was extended in space, while for Leibniz it was composed of extensionless centers of energy; for Plato matter was unknowable and for Berkeley unintelligible, while for Locke it was the critical foundation for knowledge; for some thinkers matter is actual (Democritus), while for others it is never more than potential (Hegel); for most common folk, matter is what is given to the sense, what is here and there before us, while for the more sophisticated thinkers of the century, the atom is the basic unit of matter and it is essentially a hypothetical indivisible, beyond experience. In effect, the declaration that "the world by its very nature is material" has no purchase; it fails to inform. If the real is defined as the material, then we find ourselves as yet without an answer — save through tautology — as to what is reality.

The Mental World. Throughout the centuries many have doubted the existence of the material world. And why should they not; how could one demonstrate that whatever there is, is constituted by material; what is the justification of labelling the contents of experience 'material' as opposed to various alternatives? At least one of these alternatives is already foreshadowed in the manner of framing such questions. If we are immersed from moment to moment in nothing but experience, then why not conclude that the only reality is the reality of the mind — an internal world? It is this conclusion to which Leibniz was drawn, holding that space and time are mental constructions, and to which Berkeley drew sustenance (holding that what we take to be physical things such as rocks and tables are collections of sensations or ideas). It is such thinking that also formed a central cornerstone in the idealist movement (e.g., Fichte, Schelling, Hegel) dominating the landscape of 19th century philosophy. The view lingers today in various schools of psychological theory: in cognitive theories where top down or schemata driven processes are taken to be the primary determinants of what we take to be reality, in social research of Lewinian or Heiderian stripe, in second order cybernetics theory, and in most ongoing research in the phenomenological vein.

It is beyond the scope of this paper, but it should be pointed out that the self-legitimating problems faced by the mental realists have been no less severe (most would argue that they are indeed more serious) than those of the materialists. The specter of solipsism is forever at the door, the enigma of other minds peers through the window, and the cold winds of impracticality are forbidding. Few are content to remain.

The Inferential World. As we see, many hold to material as the only directly and immediately palpable reality, while others wish to place mind (experience, consciousness) in this honorific status. Yet, there are still others who doubt the validity of both claims. Rather, it is asked, by what rationale do we claim the 'immediately givens' as the realm of the real? What is given is only an emanation, an artifact, or an expression of what is fundamentally the case. We live in a world of appearances — both in terms of the physical and mental givens. (The fact of optical illusions is typically used to prove the fallaciousness of trusting sensory information regarding the material world; the fact of dreaming often plays the same critical role in the critique of consciousness). 'The real' it is held by this band of thinkers, lies somewhere beyond — unavailable to immediate inspection, but subject to inferential appraisal. Two candidates for 'the reality beyond' have played an especially prominent role in recent years:

Neo-Realism: Beyond Material Givens. Most realist philosophy of the present century has been devoted to demonstrating that through perception we can gain direct knowledge of external, physical objects (cf. G. E. Moore, E. G. Holt, A. O. Lovejoy). Such arguments have typically attempted to make rational distinctions between veridical perception and illusion. However, bracketing a complex set of questions surrounding this attempt, a more recent 'realist' position has been developed by Bhaskar (1978), Harré (1986), Greenwood (1988) and their colleagues. This 'neo-realism' holds that the chief concern of the sciences (and thus all legitimate attempts to establish knowledge) is not at all the directly observable or sensory events to which we are exposed. Rather, the reality of focal concern lies beyond the immediately given. Thus, the natural sciences are primarily concerned, for example, with establishing knowledge about atoms, gravitational fields, and DNA, none of which are open to direct observation. Knowledge of this hidden reality must be gained through some form of inference. One does not thus carry out psychological experiments to gain knowledge of the precise characteristics of the individual participants in particularized experimental conditions, but to generate observational bases for inference to yet another level of reality. A compelling means of generating and validating inferences is yet to be forthcoming.

Depth Psychology: Beyond Conscious Givens. In the same way neo-realists posit a world beyond material reality, so do many others commit themselves to an inner world beyond consciousness. The most obvious case, of course, is that of psychoanalysis. For Freud, Jung, and a host of analytic thinkers since their time, the content of consciousness is of trivial import. Of contrasting profundity is that which lies buried beneath the conscious world. Here one may locate the motives, memories and conflicts that orient our every action. Bereft of motives, memories and the dynamics of conflict, it is much this same world that forms the basis for Chomsky's (1968) theory of linguistic knowledge. Conscious knowledge of language is, on this account, but a

surface manifestation of a 'deep structure' of the mind. Similarly, cognitive psychologists and A-I theorists propose that consciousness is but a pale derivative of computational processes undertaken at a more fundamental level. The contents of consciousness, then, are relatively decomposed artifacts of a more remote world, one to which access can be gained only through inference. A rational legitimation of such inferences again remains absent.

Relations Among Realities

From a constructionist standpoint, each of these major candidates for reality is essentially a discursive achievement. The palpability of each is derived not from 'what there is' but primarily through rhetorical techniques evolved over the history of speaking and writing (see Gergen, in press). However, because these techniques are often powerful in fashioning the taken for granted world, additional issues emerge. As denizens of Western culture, one typically participates in more than a single discursive tradition. One's local realities may thus be multiple and inconsistent. And it is participation in what Bakhtin (1981) has termed the "heteroglossia" of normal society, the multitude of mixed linguistic traditions, that sets the stage for serious inquiry into the relationship between or among disparate realities. For illustration, we may array the realities discussed thus far as shown in Figure 1.

In this context one discerns the possibility for scholarly (or scientific) inquiry into the relationship between any two or more domains. However, historically speaking, most scholarly debate has been confined to the relationships between adjoining domains. Let us briefly scan several of the most significant incursions.

Epistemology: The Relation of Mind to Material. In the Western tradition the problem of epistemology, or how we come to have knowledge, has traditionally been cast in terms of the relationship between two forms of reality, namely material and mental (and particularly conscious) reality. The

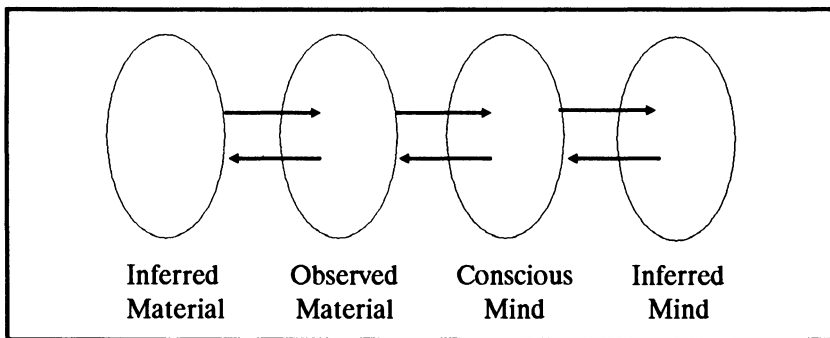| Inferred | Observed | Conscious | Inferred |
| Material | Material | Mind | Mind |

Figure 1. Reality discourses and their relationships.

critical problem has been to understand how it is that the mental world can accurately reflect the contours of the material world. For Locke, the Mills, realists of the present century, and in Eleanor Rosch's theory of natural categories it is proposed that the mental world is built up from, or in some sense a mirror of, what is materially the case. Yet, for many others, this view of mind as mirror has misleadingly placed the mind in the position of pawn. Thus, for Kant, Schopenhauer, Nietzsche, and others, there is no means by which what we take to be knowledge could be a simple replica of what we take to be the case, for in the very act of apprehension the mind actively plays a role in shaping the contours of experience and understanding. It is this view, of course, that has made its imprint in the present century on Gestalt theories of perception, Lewinian theory, constructivist theory, and 'top-down' cognitive formulations. And it is in Piaget's theory of genetic epistemology that we find a major attempt to reconcile the competing orientations. The concept of accommodation is an appeal to the Lockean or empiricist tradition, while assimilation attempts to reconcile this tradition with the Kantian or mentalist orientation.

It is also worthy of note that it is this same relationship between physical and mental reality that occupies a host of additional psychologists (and a number of philosophers) concerned with the production of behavior or conduct. The overarching question in this case is how it is that mental events make an impact on bodily conduct. Thus, social psychologists inquire into the relationship between attitudes and action, developmentalists into the relationship between moral thought and moral action, psycholinguists into the relationship between competence and performance, and action theorists into the relationship between motives and conduct. It is also this problem that has absorbed much recent philosophical debate on 'reasons' versus 'causes' in human understanding, and the possibility for intentions-based theories of human action.

Most contemporary inquiry in the phenomenological mode is essentially mentalist in its emphasis. That is, such research is typically concerned with the experience, perception, or subjective life-world of various individuals. However, such inquiry stands in strong contrast to phenomenological theory — from Husserl and Heidegger to Merleau-Ponty and Giorgi. On the theoretical level, the central concern has again been with the relationship between subjective and objective realities. However, unlike the work cited above, phenomenological theory often attempts to obliterate the distinction. The question, then, is not so much how one reality can affect the other, but how they can be simultaneously treated as a single unity. Intentionality, as it is said, is always already suffused with an object, and the separation of mind and world is a futile exercise in abstraction. Intellectually, then, the phenomenologists make an exciting move toward what may be viewed as a new form of monism.

Yet, as we find in the end, this newly fashioned reality is a derivative byproduct of the world-views already extant. Without the discursive platforms of the materialists and the mentalists, there would be no means of making a phenomenological unity apparent. It is only when we can agree that there is a material world to be inspected, on the one hand, and an experience of it on the other, that it is possible to think of an indissoluble unity. To consider the unity, irrespective of the contents, would propel one into the range of the inarticulable. The rhetoric of the phenomenological reality relies, then, upon the reality-fashionings of two pre-existing linguistic traditions. It is also largely for this reason that phenomenological research falls back on the language of the mentalist. Such researchers must speak of the individual's 'experience', for there is no other language made available by the phenom-enologist.

Psychoanalysis: The Relationship Between Consciousness and the Unconscious. The positing of a 'mind behind the mind' is of relatively recent historical vintage, and thus we find much less in the way of illustrative material. It is of course to Freud and Jung that the most significant inquiry into this relationship must be credited. Freud's theories of repression, symptom formation, and dream work are all creative attempts at forging such a relationship. Similarly, Jung's theory of the emergence of archetypes into consciousness, and the individuating of the conscious mind over the life-span add a sense of palpability to the connection. For many, Jacques Lacan's writings have more recently revitalized this concern. And in a certain sense, recent inquiries into person memory, cognition and consciousness, and category accessibility all speak to the relationship between psychological surface and depth.

Affordances: The Relationship of Material to Supra-Material. It would be a tidy finish to the present argument if I could demonstrate longstanding concern with the relationship between the material level of reality and that which lies beyond (the 'supra-material'). Alas, this aesthetic flourish will remain unexecuted, for there are no ready contenders of recent vintage. However, one of the neo-realist projects is of particular note, for it concerns not the simple relationship between these two adjoining realities, but the interdependency of three realities: material, supra-material and the mental.

It is Harré's recent *Varieties of Realism* that is most cogent, for here Harré sets himself the task of (1) demonstrating that the activity of individual scientists does increase our knowledge of the real world, (2) that this knowledge is not about the immediately given world of material but the underlying supra-material, and (3) this knowledge is not simply built up from empirical observation (*à la* traditional empiricist metatheory). How then is the mind to comprehend the supra-material when simple observation is obviated as a procedure?

To solve the problem Harré brings to bear a form of Gibsonian affordance theory. He argues, in effect, that the relationship of the supra-real to the materially real, though unknowable, is systematic (rather than constantly and randomly varying). Further, and most importantly, he proposes that the mind is inherently readied (by virtue of its physiological structure) to grasp from the immediately real certain rudimentary truths concerning the supra-real. On a simple level, if exposed to the sharp teeth and hissing sound of an approaching dragon, we would immediately grasp, without benefit of previous learning history, the immanent danger. This is so argues Harré (after Gibson) because the millennia of relationships between brain and supra-reality have left the human being — via Darwinian process of natural selection-singularly capable of reacting appropriately to the localized signals of the supra-reality. In effect, we are prepared in a rudimentary way by natural selection to read off the nature of the underlying reality by virtue of its signals in the world of the immediately given.

On the Character of Metatheoretical Debate

The analysis thus far is hardly intended as a systematic review of major views of reality and their relations in philosophy and psychology. Rather, the attempt is to set out a means by which we can assay such attempts, a discursive landscape as it were, along which we can array the various incursions into fundamental ontology as they have emerged in the Western intellectual tradition. Such a treatment allows us, in the end, to develop several more general arguments concerning the character of metatheoretical debate within the discipline and its implications for theory and research. Four points are particularly worthy of discussion:

The Infinite Laminations of Reality

Most metatheoretical and theoretical debate within recent years has been confined to the four forms of reality discussed thus far. However, one may also view these debates as restricted both culturally and historically. That is, such limitations on our literary renderings of reality does not derive from the nature of reality itself but from the forms of relationship (and particularly linguistic) emerging within recent Western culture. What principled limitations, if any, could be placed over such debate? What could set an upward limit on the possible realms of reality? From a constructionist perspective, it is difficult to discern the possibility for such limitations. Differing cultures in differing historical epochs have posited numerous alternatives to the contemporary Western view — a world of dreams, for example, said to be a second-order reality, a world of gods and goddesses somewhere beyond the material world and not derived from its character, a world of 'the absolute' beyond language and description, and so on. These are not our worlds, but there is

nothing standing between us and them save the further negotiation of meaning.

Even within the more delimited range of realities outlined above we can see the possibility for an infinite expansion. At the outset there is nothing save conventional habits of labelling that would prevent us from replacing the concepts of material and conscious experience with other forms of reality. Already physical scientists are capable of arguing against the existence of any form of material world, and replacing such concepts with those of energy. And there are many AI specialists willing to argue that consciousness is but an epiphenomenon of computational systems within the brain. In effect, both the 'given realities' may be redefined in yet other terms than the conventional.

Beyond the possibilities for reconstituting the 'givens', one may also reconceptualize the inferential realities. For example, there is no compelling reason for believing that beyond the material world lies a supra-reality that is also material, nor with Freud, that beyond the level of conscious mentality lies yet another world of the mental. A localized reality is no purchase on an inferred world of the same composition.

And finally, because all realities are subject to the critique of appearances, we enter at last into an infinite regress of realities. That is, in the same way that material and conscious realities may be only mystifying appearances spread over the aperture to the essential, so may the 'essentials' be but an obfuscating layer of yet another sort. As suggested in Figure 2, the true and valiant theoriest should not stop at the time-honored junctions of the present, but should press on to the 'really real'.

Should this possibility seem remote, consider the physicist who believes that it is, after all, God's hand that writes the book of nature. To peer into a cloud chamber is to apprehend at last the emanations of the Holy spirit. Would it not be a major theoretical advance to render real a psychological world beyond the unconscious, a supra-unconscious that guides the dynamic interchanges between the more superficial layers of the psyche?

Realities without Grounds

Now that we confront the possibility of a virtual infinity of realities we may inquire into the possibilities judging relative merit. Given an array of candidates, how is it possible to argue for the superiority of one choice as opposed to another? How is a materialist, a mentalist, a neo-realist or a depth analyst to justify the favored position? There are two conceptual problems confronted by the protagonist at this juncture, both of similar form and both formidable.

The first derives from the nature of justificatory language itself. As Saussure proposed, language is essentially a discriminating device; it functions on the basis of distinctions. In this sense, to name something is to
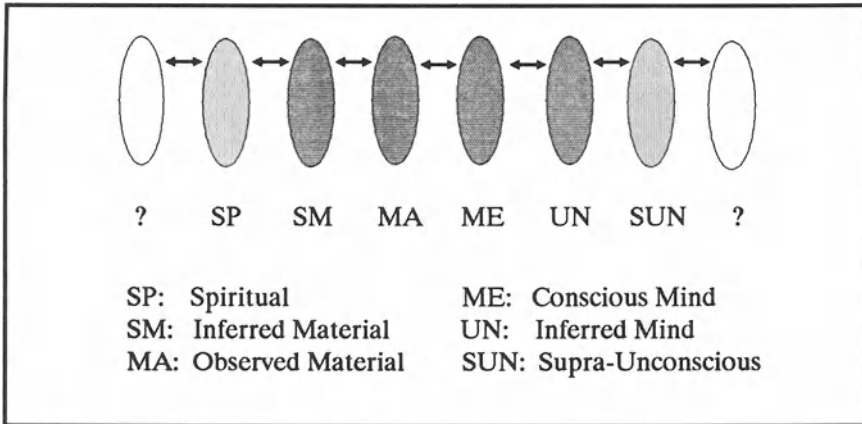
Figure 2. Expanding the discourse domains.

distinguish it from all that it is not. The advantage is thus that words such as Hank, Andy, and John allow us to fine-tune our social practices — making distinctions in conduct appropriate to variations in practice. Yet, if we presume that all reality is of a special kind (e.g., wholly material, or fundamentally mental) the linguistic sign fails to function in any practical way. If *all* is material, then there is nothing to which one can refer that is not material. And, if there is no contrast possible between 'material' and 'not material', then such a term ceases to carry any weight in the game of language. In this sense, proclamations of the real are maintained by an ironic silencing of that which is essential to granting them a sense of validity. For 'material' to stand as 'the real' demands a corresponding recognition of 'non-material', which recognition must simultaneously be silenced. For to allow such recognition would be to negate the fundamental contention that all is material.

  The second conceptual issue concerns the grounds for justification. For example, if one is to justify the world as a 'phenomenological unity', on what body of discourse can one rely? At this juncture one can make recourse to either one of two options: one can either refer to discourse cast in terms of the ontological unity, or to some other form of discourse. One can thus speak in terms of the phenomenological unity which is the case, or can rely on an alternative discourse to argue that this is so. Both options are inherently flawed. In the first instance, if one relies on the discourse declared to be real, one has simply repeated the initial injunction. If one says that the phenomenological unity is real because it is given in one's phenomenological unity, one has not furnished a justification but a tautology. And, if one employs some other form of discourse, then one typically admits into the realm of reality yet another domain, which admission negates the initial proclamation. Thus, to justify the phenomenological unity by recourse to experience (separated from the unity but reflecting it), yields not one reality

but an embarrassing pair. Or, to say that the unity is derived from powers of logic is now to make real yet another form of reality separated from the unity itself.

On the Analysis of Realities

What does the present analysis have to say concerning past and future theory and research in psychology? At the outset one's view toward previous theoretical analyses changes radically. Traditionally the attempt to establish a 'realm of reality' has been viewed as a serious enterprise. Such attempts have been used not only to specify what there is to be known, but to rationalize the modes for its exploration. (As in the close relationship once existing between materialism, behavior theory, and the experimental method). In these ways, such attempts have also been aggressive and hegemonic. For example, what are taken to be justified problems by one group are disqualified by others. For behaviorists, there is simply no phenomenological reality to be explored; for the psychoanalyst reinforcement schedules are an absurdity. Legitimate modes of inquiry from one ontology are 'irrational' or 'irrelevant' from the others.

From a constructionist perspective we find both the attempt to establish a 'rock bottom reality', and to valorize certain modes of inquiry over others, to be essentially moves in a realm of discourse. Further, there is no upward limit on the number of such moves that can be made, and no viable means of establishing the superiority of any one over any other. The differences between materialists, realists, phenomenologists, psychoanalysts, and the like are not ones about which adjudication is possible. Nor should adjudication (the attempt to vanquish all opposing contenders) be sought. Rather, the realities are essentially differences in ways of talking and writing — more like bird songs and banners than maps of a terrain.

This is also to say that research (both conceptual and 'empirical') into the relation between the 'external' and 'internal' world (or between any other two or more forms of reality) should primarily be viewed as an exercise in discourse. 'Empirical observations' do not, on this account, furnish us with reliable indicators, sound guidance, or incorrigible criteria for judging the adequacy of propositions relating realms of the real. Such observational exercises may possess a degree of rhetorical power in the game of persuasion, but methods cannot yield what is the case about reality when the adoption of method already commits one to a conception of the real.

This is not, however, to disparage the development and elaboration of the conflicting discourses. Constructionism is not in this sense aggressive; the point is not at all to abandon traditional forms of discourse and their expansion. Such forms of discourse are, after all, embedded within various social practices. To abandon the discourse would be to threaten or destroy the

practices. Thus, the more pregnant question is not whether any particular form of ontology is correct or incorrect, but what social practices do such forms of discourse serve to sustain? What forms of social pattern are discouraged?

At this point the issue can only be illustrated. For example, both realist and materialist discourses seem to be embedded in a host of activities typically called 'real world problem solving'. Without the discourses available for co-ordinating such activities, many would be difficult to accomplish. Yet, such forms of discourse also tend to 'dehumanize' the individual — rendering words (and their associated practices) such as 'sympathy', 'spirit', and 'morality', irrelevant to social life. There is much to regret, then, in such forms of intelligibility. In contrast, phenomenological discourse seems little embedded within the world of practical problem solving, but is found immensely serviceable within various therapeutic domains. For therapists to lose such forms of talk would be to render their activities ineffectual. Much more needs to be said. A pragmatic, value oriented analysis of extant psychological theory is of vital concern at this point. Nor should this analysis exclude the constructionist intelligibility itself.

References

Bakhtin, M. (1981). *The dialogic imagination*. (C. Emerson and M. Holquist, Trans.). Austin: University of Texas Press. (Originally published 1934-35.)

Bhaskar, R. (1978). *A realist theory of science*. Brighton: Harvester Press.

Chomsky, N. (1968). *Language and mind*. New York: Harcourt, Brace, & World.

Cornforth, M. (1971). *Materialism and the dialectical method*. New York: International Publishers.

Gergen, K. J. (in press). *Construction, critique and community*. Chicago: University of Chicago Press.

Greenwood, J. D. (1988). *Explanation and experiment in social psychological science*. New York: Springer-Verlag.

Harré, R. (1986). *Varieties of realism*. London: Blackwell.

Johnson, P. (1973). Materialism. *Encyclopedia of philosophy*. New York: MacMillan.

THE MYTH OF MIND AND
THE MISTAKE OF PSYCHOLOGY

John Shotter

SUMMARY: In psychology, it is thought 'natural' to speak of people as possessing within themselves something called their 'mind', and to think that minds have their own discoverable, intrinsic principles of operation, which owe nothing either to society or to history for their nature. But the 'mind' as such is, I think, a mythic entity. And attention to it diverts our attention away from the detailed social processes involved, not only in negotiating the making of common meanings, but also from those involved in the everyday methods of *testing and checking* we use in establishing socially intelligible and legitimate common goals. It is its failure to notice the importance of these processes of normative evaluation which is, I think, psychology's mistake. What I want to claim below is: 1) that psychology is not a *natural* but a *moral* science, 2) that instead of what might be called a theoretical/explanatory approach, aimed at producing theoretical knowledge, it must use a practical/descriptive approach, aimed at gaining practical-moral knowledge, 3) that this aim is much more difficult to achieve than might be imagined, as more than simply academic activities are involved, and 4) that although 'social constructionist' studies are required at present, our embodied nature is what is our ultimate problematic.

Psychology as a Moral not a Natural Science

Not only in psychology but also in our "official common sense doctrines" (Ryle, 1949), it is thought 'natural', so to speak, to think of ourselves as possessing within us something we call our 'mind' — an internal, secular organ of thought which mediates between us and the external reality surrounding us. And furthermore, to think that as such, our minds have their own dis-coverable, natural *principles of operation* which owe nothing either to history or to society for their nature. It is the task, of course, of a natural scientific psychology to discover what these principles are.

This conception of 'mind' is, I think, a myth, one which has led psychol-ogy into a number of dangerous mistakes. Here, I want to explore just one of them, the one which I think is the most central and the most dangerous: the failure to take account of the fact that in our everyday social life together, we do not find it easy to relate ourselves to each other in ways which are *both* intelligible (and legitimate), *and* which also are appropriate to '*our*' (unique) circumstanes; and the fact that on occasions at least, we none the less do succeed in doing so. Attention to the actual, empirical details of such trans-actions reveals a complex but uncertain process of testing and checking, of negotiating the form of the relationship in terms of a whole great range of, essentially, *moral* issues - issues to do with entitlements, judgments, matters

of care and concern, and so forth. For in our social lives together, the fact is that we all have a part to play in *a major corporate responsibility*: that of maintaining in existence the communicative 'currency', so to speak, in terms of which we conduct all our social transactions. For our ways and means of 'making sense' to (and with) one another have not been given us as a 'natural' endowment, nor do they simply of themselves endure; what is possible between us is what we (or our predecessors) have 'made' possible. It is this respons-ibility that modern psychology has ignored, and which has led it, mistakenly, to give professional support to the view "that 'I' can still be 'me' without 'you'" — a view which renders most of our actual social life 'rationally-invisible' (Shotter, 1989).

Thus, against the claim that psychology is 'naturally' a biological science, requiring for its conduct the methods of the morally neutral natural sciences, I wish to differ, and to claim (yet again, cf. Shotter, 1975, 1984) that it is not a *natural* but a *moral* science, and that this gives it an entirely new character. The major change introduced is this: the abandoning of the attempt simply to *discover* our supposed 'natural' natures, and a turning to the study of how we actually do *treat* each other as being in everyday life activities — a change which leads us on to a concern with 'making', with processes of 'social construction' (Harré, 1979, 1983; Gergen, 1982, 1985; Shotter, 1975, 1984; Shotter & Gergen, 1989).

What I want to do in this paper, then, is to discuss two issues: 1) one is to explore why we are so attached to (in fact, 'entrapped' by) this myth of a 'naturally principled' mind; and 2) the other is to explore the nature of an alternative assumption in terms of which to orient psychological investigations, an alternative which gives just as much a place to 'making' as to 'finding'.

Why Do We Still Believe in the Systematic Nature of Mind?

Why do we seem so 'at home', so to speak, with the idea that there *must* be some already existing or 'natural' *systematic principles* of mind and behavior to be discovered, somewhere? There are, I think, at least two main reasons, both to do with our concern with *systems*. So let me discuss them in turn.

Firstly: because ever since the ancient Greeks, people in the West have believed that 'reality' is to be 'found behind appearances'. Thus it has long been thought that a very special power resides in the nature of reflective or theoretical thought: it can penetrate through the surface forms of things and activities to grasp the nature of a deeper 'form of order', an underlying order from which all human thought and activity *must* in fact spring. Thus society at large has accepted it as a legitimate task of a certain special group of people — called scholars, priests, philosophers, scientists, or intellectuals — to attempt to articulate the nature of this deeper order. But the problem is: where is this order to be found?

In the West, we first looked for this deeper order unsuccessfully in religious and metaphysical *systems*. But then, during the Enlightenment, having lost faith in the "spirit of systems," we adopted in our investigations, says Cassirer (1951, p. vii), "the systematizing spirit."

> Instead of confining philosophy within the limits of a systematic doctrinal structure, instead of tying it to definite immutable axioms and deductions from them, the Enlightenment wants philosophy to move freely and in this immanent activity to discover the fundamental form of reality, the form of all natural and spiritual being (Cassirer, 1951, p. viii, first published 1932).

Thus,

> ... the fundamental tendency and the main endeavour of the philosophy of the Enlightenment are not to observe life and to portray it in terms of reflective thought. This philosophy believes rather in an original spontaneity of thought; it attributes to thought not merely an imitative function but the power and task of shaping life itself (Cassirer, 1951, p. viii, first published 1932).

And this, I think, is still the project implicit in modern psychology, which we have inherited from the Enlightenment: the task of 'discovering' a supposedly neutral set of 'mental' principles upon which the rest of life *should*, rationally, be based. Few of us now, however, possess the intellectual and the moral confidence still to accept that brief in good faith. Yet, although we cannot entirely give up the belief that there must be *some* worth in the effort to think seriously about life's choices, we find it very difficult to devise alternatives: we keep finding ourselves as if 'entrapped' within an invisible maze, from which there is no escape — this is because, within our professional academic practices as they are currently conducted, there isn't!

This brings me to the second of my two reasons why we find it so difficult to formulate intelligible, alternative accounts of ourselves — in fulfilling our responsibilities as competent and professional academics, we must write *systematic texts*. Until now we have taken such texts for granted as a neutral means to use how we please. This, I now want to claim, was a mistake. But why should a concern with the nature of the literary and rhetorical devices constituting the structure of a *systematic, decontextualized text* now be of such concern to scientific psychologists?

Because theorists, in attempting to represent the open, vague, and temporally changing nature of the world as closed, well-defined, and orderly, make use of certain textual and rhetorical strategies to construct within their text *a closed set of intralinguistic references*. They have not, however, appreciated the nature of the social processes involved in this achievement. But the fact is, in moving from an ordinary conversational use of language to the construction of systematic texts, there is transition from a reliance on particular, practical, and unique meanings, negotiated 'on the spot' with reference to the immediate context, to a reliance upon links with a certain body of *already determined* meanings — a body of special, interpretative resources into

which the properly trained professional reader has been 'educated' in making sense of such texts. Being able to make reference to already determined meanings, thus allows a decrease of reference to what 'is' and a consequent increase of reference to what 'might be'. One must then develop *methods* for *warranting* in the course of one's talk, one's claims about what 'might be' *as being* what 'is'. It is by the use of such methods, that those with competence in such procedures can construct their statements as 'factual statements' — and claim authority for them as revealing a special 'true' reality behind appearances, without any reference to the everyday context of their claims (see Dreyfus & Rabinow, 1982, p. 48).

But this process can produce, and for us in the social sciences, *does* produce what Ossorio (1981) has called, *ex post facto* fact fallacies: the fallacious retrospective claim that, for present events to be as they are, their causes *must* have been of a certain kind. Someone who has already studied the general nature of this fallacy in relation to scientific affairs, is Fleck (1979). He comments upon its general nature as follows:

> ... once a statement is published it constitutes part of the social forces which form concepts and create habits of thought. Together with all other statements it determines 'what cannot be thought of in any other way' ... There emerges a closed, harmonious system within which the logical origin of individual elements can no longer be traced (Fleck, 1979, p. 37).

In attempting retrospectively to understand the origins and development (and the current movement) of our thought, we describe their nature within our to an extent now finished and systematic schematisms. But in doing so "we can no longer express the previously incomplete thoughts with these now finished concepts" (Fleck, 1979, p. 80).

But the trouble is, once 'inside' such systems, it is extremely difficult to escape from them. We can, as Stolzenberg (1978) puts it, become "entrapped" in the following sense: that "an objective demonstration that certain of the beliefs are incorrect" can exist, but "certain of the attitudes and habits of thought prevent this from being recognized" (Stolzenberg, 1978, p. 224). This, I think, is the trap within which we have ensnared ourselves in our academic thought about ourselves and our psychology. But it means that our scientifically acquired knowledge of the world and ourselves is not determined by our's or the world's 'natures' to anything like the degree we have believed (and hoped) in the past; but instead, our knowledge is influenced by the 'ways', the literary and textual means, we have used in formulating our concerns? To go further: it means that we have spent our time researching into myths of our own making — the myth of mind being a case in point. How can we escape from this entrapment?

The 'Conversation' of Humankind

Well, I feel that it is important to study the actual, empirical nature of our ordinary, everyday, nonprofessional, nontextual, conversational ways and means of making sense together. As I have already mentioned above, the essence of textual communication is its so-called *intertextuality*: the fact that it draws upon people's knowledge of a certain body of *already formulated* meanings in the making of its meanings — this is why texts can be understood without contexts, that is, independently of immediate and local contexts. But, as Garfinkel (1967) points out, in ordinary conversation people refuse to permit each other to understand what they are talking about in this way. A meaning unique and appropriate to the situation and to the people in it is required. But that is not easy to negotiate. Thus, what precisely is 'being talked about' in a conversation, as we all in fact know from our own experience, is often at many points in the conversation necessarily unclear; we *must* offer each other opportunities to contribute to the making of agreed meanings.

Thus, only gradually do we come to an understanding (and even then it is often limited just to matters in hand, so to speak). As Garfinkel (1967, p. 40) says about such understandings: they are developed, and developing, *within* the course of the action; indeed, to quote him, they are only known by both parties *"from within* this development ...."

Indeed, a quite special but unrecognized kind of knowledge is involved here; it is not a 'knowing-that' (theoretical knowledge) for it is practical knowledge known to us only in practice, but neither is it a 'knowing-how' (technical knowledge) for it is particular to the proprieties of its social situation. It is a third kind of knowledge of a *practical-moral* kind (Bernstein, 1983; Shotter, 1986a and b). Ignoring it, leads us to ignore the unique nature of situations *and* of the people within them. We can thus begin to see why, when Garfinkel had his students try to talk as if words should have already determined clear meanings, it produced a morally motivated anger in the student's victims. People felt that in some way their rights had been transgressed — and as Garfinkel shows, they had!

What should we say then about the nature of words and their meanings, if we are not to see them as having already determined meanings? Perhaps, rather than *already* having a meaning, we should see the *use* of a word as a *means* (but only as *one* means among many others) in the social making of a meaning. Thus then, 'making sense', the production of a meaning, would not be a simple 'one-pass' matter of an individual saying a sentence, but would be a complex back-and-forth process of negotiation between speaker and hearer, involving tests and assumptions, the use of the present context, the waiting for something later to make clear what was meant before, and the use of many other 'seen but unnoticed' background features of everyday scenes, all

deployed according to agreed practices or 'methods'. These are in fact the properties Garfinkel claims of ordinary conversational talk. And as he says:

> People require these properties of discourse as conditions under which they are themselves entitled and entitle others to claim that they know what they are talking about, and that what they are saying is understandable and ought to be understood. In short, their seen but unnoticed presence is used to entitle persons to conduct their common conversational affairs without interference. Departures from such usages call forth immediate attempts to restore a right state of affairs. (Garfinkel, 1967, pp. 41-42)

Moral sanctions follow such transgressions. Thus, to insist words have pre-determined meanings is to rob people of their rights to their own individuality. But even more than this is involved: it is to deprive one's culture of those conversational occasions in which people's individuality is constituted and reproduced. It is also to substitute the authority of professional texts in warranting claims to truth (on the basis as we now see of the unwarranted claim that they give us access to an independent, extralinguistic reality), for the *good reasons* we ordinarily give one another in our more informal conversations and debates.

The Foundations of Psychology: In Principles of Mind, or in Everyday Life Conversation?

The move from a decontextualized concern with a theoretical/explanatory 'psychology of mind', to a 'situated' concern with a practical/descriptive 'psychology of socio-moral relations', entails a change in what we take the *foundations* of our discipline to be.

As we know, our Cartesian tradition has it that our investigations must, if they are to be accounted intellectually respectable, possess foundations in explicitly stated, self-evidently true, propositional statements. And to deny this (as indeed Rorty [1980] has done) seems to open the door to an 'anything goes' chaos. It seems as if there is nothing at all in terms of which claims to knowledge can be judged.

This, however, is simply not the case. For let me state again what seems to me to be the undeniable empirical fact which a natural scientific psychology has consistently ignored: the fact that our daily lives are not rooted in written texts or in contemplative reflection, but in oral encounter and reciprocal speech. In other words, we live our daily social lives within an ambience of conversation, discussion, argumentation, negotiation, criticism and justification; much of it to do with problems of intelligibility and legitimation. Anybody wanting to deny it will immediately confront us with an empirical example of its truth. And it is this 'rooting', of all our activities in our involvements with those around us, which prevents an 'anything goes' chaos. For only if we possess a special kind of sensibility, a certain kind of *common sense*, a morally aware social competence acquired in the course of our growth

from childhood to adulthood, do we qualify for such an involvement. Lacking it, our right to act freely, our autonomous status, is denied us. It is this sensibility, the different *feelings* (or emotions) to which it gives rise in the different situations in which we are involved, which work as the 'standards' against which our more explicit formulations are judged for their adequacy and appropriateness. In fact, I want to claim along with Wittgenstein that:

> We judge an action according to its background within human life ... The background is the bustle of life. And our concept points to something within this bustle ... Not what *one* man is doing now, but the whole hurly-burly, is the background against which we see an action, and it determines our judgment, our concepts, and our reactions. (Wittgenstein, 1980, II, pp. 624-629).

Although, I hasten to add, that it does not determine them in an instant, nor is all the *possible* background bustle and hurly-burly of life present 'in' an instant either.

It is this view — that the roots or foundations of our actions are to be found generally just within everyday activities (including the uncompleted 'tendencies' to action they contain), and not within certain, already ordered principles of mind — which intellectuals have found, and still find, difficult to stomach. For it means that anything we propose depends for its acceptance, just as much (if not more) upon the common, collective, but 'disorderly', *embodied* sensibility of people in society at large, as upon the refined, systematic, and self-consciously formulated notions of academics and intellectuals. And what this means, I think (if you want a prediction for the future), is that in the growth of a noncognitive, non-Cartesian, rhetorical, social constructionist approach to psychology as a *moral science*, an obvious next step is a growing interest, not in the mind or the brain, but in the living body — or more correctly, in unreflective bodily activities. For paradoxical though it may be to say it, it is in bodily activities, I think, that ideas start, not in the mind; they are both the *terminus a quo* and *terminus ad quem* of all our social constructions.

References

Bernstein, R. J. (1983). *Beyond Objectivism and Relativism.* Oxford: Blackwell.

Cassirer, E. (1951). *The Philosophy of the Enlightenment.* New Jersey: Princeton University Press.

Dreyfus, H. L. & Rabinow, P. (1982). *Michel Foucault: Beyond Hermeneutics.* Sussex: Harvester.

Fleck, L. (1979). *The Genesis and Development of a Scientific Fact.* Chicago: University of Chicago Press.

Garfinkel, H. (1967). *Studies in Ethnomethodology.* Englewood Cliffs, NJ: Prentice-Hall.

Gergen, K. J. (1982). *Toward Transformation in Social Knowledge.* New York: Springer-Verlag.

Gergen, K. J. (1985). The social constructionist movement in modern psychology. *American Psychologist, 40,* 266-275.

Harré, R. (1979). *Social Being.* Oxford: Blackwell.

Harré, R. (1983). *Personal Being.* Oxford: Blackwell.

Ossorio, P. (1981). Ex post facto: the source of intractable origin problems and their resolution. Boulder, CO: Linguistic Research Institute Report No. 28.

Rorty, R. (1980). *Philosophy and the Mirror of Nature.* Oxford: Blackwell.

Ryle, G. (1949). *The Concept of Mind.* London: Hutchinson.

Shotter, J. (1975). *Images of Man in Psychological Research.* London: Methuen.

Shotter, J. (1984). *Social Accountability and Selfhood.* Oxford: Blackwell.

Shotter, J. (1986a). A sense of place: Vico and the social production of social identities. *British Journal of Social Psychology, 25,* 199-211.

Shotter, J. (1986b). Speaking practically: Whorf, the formative function of language, and knowing of the third kind. In R. Rosnow & M. Georgoudi (Eds.), *Contextualism and Understanding in the behavioural Sciences* (pp. 211-217). New York: Praeger.

Shotter, J. (1989). Social Accountability and the social construction of 'you'. In J. Shotter & K. J. Gergen (Eds.), *Texts of Identity* (pp. 133-151). London: Sage.

Shotter, J., & Gergen, K. J. (Eds.) (1989). *Texts of Identity.* London: Sage.

Stolzenberg, G. (1978). Can an inquiry into the foundations of mathematics tell us anything interesting about mind? In G. A. Miller & E. Lenneberg (Eds.), *Psychology and Biology of Thought and Language: Essays in Honour of Eric Lenneberg* (pp. 221-269). New York: Academic Press.

Wittgenstein, L. (1980). *Remarks on the Philosophy of Psychology,* Vol. II. Oxford: Blackwell.

HERMENEUTICS OF EXPLANATION: OR, IF SCIENCE IS THEORETICAL WHY ISN'T PSYCHOLOGY?

Leendert P. Mos & Casey P. Boodt

SUMMARY: Following a critical review of the nature of explanation, it is argued that explanations, whether of singular events or empirical regularities, are fundamentally theoretical in nature. Theoretical explanations invoke hypothetical structures which are neither deductively related to empirical regularities, or nomothetic laws, nor simply predictive of singular events. The value-ladeness of our theoretical explanations in psychology and, hence, our rationality, is only bounded by the historical, social-cultural, life forms which characterize our linguistic and communal practices. It is concluded that psychology, in its search for theoretical explanations, is inescapably a hermeneutic endeavor.

Introduction

Students interested in the applications of psychology often prefer theoretical studies over experimental ones. This is rather puzzling, considering that those interested in the applications of psychological knowledge ought to be, in the first instance, interested in the empirical regularities which are formulated on the basis of experimental research. They should be interested in nomothetic laws which, together with statements of initial conditions, would render intelligible any singular, observed event. It is, after all, singular events which are of interest to the practitioner.

Equally puzzling is that there are so few students who, having chosen experimental research as a vocation, also express an interest in theoretical studies. For consider that those who are interested in establishing empirical regularities ought to be interested in their explanation. In order to understand these puzzling combinations of academic interests, we turn to the nature of scientific explanation, for it is here that we find reasons why students with applied interests frequently have theoretical interests, and students interested in experimental research frequently eschew theory. Moreover, we will find that an understanding of scientific explanation yields nothing to those who conceive of psychology as a natural science. Indeed, our understanding will disclose why those who conceive of psychology as a hermeneutical endeavor can rightfully claim 'to explain'.

Empirical and Theoretical Explanations

Within the empiricist tradition, explanation consists of fitting an event to be explained into a pattern of law-likeness. However, as far back as Francis Bacon (Butterfield, 1968/1949; Holton, 1973), we find that the explanation of some singular event was also sought in unobserved and, hence, hypothetical entities that were believed to be causally related to the event (McMullin, 1985). This duality of explanations is familiar to us from the writings of Carl Hempel (1966) who, from within the logical empiricist tradition, distinguished between deductive-nomological (nomothetic) and hypothetical-deductive (theoretical) explanations.

Deductive-nomological explanation is a mode of explanation used to fit an event to be explained ('explanandum') into a pattern of regularities ('explanans'). In order to explain some singular event, it is necessary and sufficient that it be deductively derivable from one or more empirical regularities, together with a statement of initial conditions. If the explanation is successful, the event is then shown to be law-like in its occurrence and, hence, intelligible. Empirical regularities, or nomothetic laws, are, presumably, inductively derived, either from experiments or 'naturalistic' observations.

Hypothetical-deductive explanation is of a very different kind. Thus, once some empirical regularities have been formulated in a domain of inquiry, a hypothetical-deductive explanation is intended to explain these regularities. A theoretical explanation takes these empirical regularities to be a manifestation of some 'deeper' processes, entities and their relations, that are causally related to them. Thus, a theory, or hypothetical structure, is intended to explain these empirical regularities, and predict new ones. Theoretical explanations do not subsume empirical observations under nomothetic laws, rather, they introduce hypothetical entities and relations to explain these nomothetic laws. While theoretical explanations may be in part deductive, their nomological nature is only secondary. The explanans, the theory, is not another set of laws but a postulated structure, or model, hypothesized to causally explain the empirical regularities, or explananda.

Thus, while a postulated hypothetical structure may itself be law-like, these laws are definitely not empirical regularities. It is not the theoretical laws describing the hypothetical structure which are explanatory, rather, it is the hypothetical structure which may suggest these laws and, perhaps, others besides. In fact, the hypothetical structure, the explanans, is unlikely to be simply deductively related to the nomothetic laws which are their explananda. Rather, the hypothetical structure goes beyond both the empirical regularities and, hence, the original observations, in a way that is much stronger than how the nomothetic laws go beyond the original observations.

It is sometimes erroneously suggested that a theoretical explanation is merely a deductive-nomological explanation at a 'higher' level. However, we must be careful to distinguish between empirical and theoretical laws. Empirical, or nomothetic, laws describe, with a minimum of idealization, method derived observations. Theoretical laws describe a hypothetical structure (hence, theoretical explanations are sometimes called 'structural explanations', e.g., McMullin, 1978), or model, that is the theory *qua* theory. Theoretical laws are not based on observations, rather, they explain by showing that empirical laws are derivable from certain assumptions about the intrinsic nature of the observed events subsumed under them.

Theoretical Explanations are Hypothetical. On the empiricist account, empirical laws are inductive and direct, while theoretical laws are ampliative, and, hence, indirect. Thus, theoretical laws derive their warrant from the "explanatory power" of the theory, whereas empirical laws derive their warrant from empirically derived observations (McMullin, 1984). Hempel's characterization of these two modes of explanation assumes a sharp distinction between the theoretical and the observational. Or, in contemporary terms, the question is raised whether deductive-nomological explanations are theory-neutral? If not, as the current 'omnitheoretical' (Rozeboom, 1972) philosophy of science maintains, then the question is, can nomothetic laws be 'deductively' subsumed under theoretical laws in a manner, presumably, analogous to the 'logical' subsumption of observed events under nomothetic laws? In whatever manner the theory, or value, ladeness (McMullin, 1978) of these empirical regularities may be eventually conceptualized, it has become abundantly clear that they are not foundational as the logical empiricists maintained or hoped. However, they do, presumably, remain relatively stable against the background of changing theories. For, even as Thomas Kuhn (1970) and others have acknowledged, empirical generalizations as explanations are cumulative and progressive ('normal science'), even as they may become irrelevant in the context of shifting theories or, better, perspectives ('revolutionary science').

But if empirical regularities are stable, how is it that theories change, or new theories are proposed, often independently of the empirical regularities? In considering this question, and in noting both the historical shifts in, and proliferation of, competing theories, one begins to appreciate that many experimental researchers prefer to stay away from theoretical explanations and remain at the level of empirical generalizations. But, then, do nomothetic laws truly explain, or do they merely describe their observed regularities?

Evidently, nomothetic laws may, or may not, be explicitly causal. Laws asserting concomitant variation in two or more parameters usually do not specify one of the parameters as cause (a determination which usually requires theoretical analysis and further experimentation). Empirical regularities in psychology tend to be of the latter sort, which is why these often

go unchallenged in spite of changing theories. Whatever their formulation, nomothetic laws furnish only the weakest of explanations since what we often want to know, but what such nomothetic laws do not give us, is an answer to the question of *why* the observed events can be so subsumed or, why particular nomothetic laws hold (Carnap, 1937; MacCorquodale & Meehl, 1948)?

Of course, it is always possible to 'subsume' nomothetic laws of the first order under higher order 'empirical' laws. This is what is sometimes intended by hypothetical-deductive explanations in psychology where such endeavors have been noted for their failure. However, the explanation of nomothetic laws is inevitably theoretical, namely, ampliative in the strong sense of proposing, of opening up, new domains of concepts which are neither part of the language of nomothetic laws nor, indeed, the observations they subsume. Such new domains of concepts may be analogical extensions of familiar concepts, familiar concepts with new meanings or extensions, or, indeed, totally new concepts. Whatever their formulation, theoretical explanations will have to deal with the Duhem-Quine language 'network' thesis, with the proposed strong role of metaphor (e.g., Hesse, 1974) and, more recently, with Putnam's (1988) claim that language is holistic, normative, and historical.

Theoretical explanations are what C. S. Peirce termed "qualitative inductive" explanations consisting of two phases: abduction and retroduction (Resher, 1978). Abduction is the conjectural hypothesis-proliferation phase which begins with observations, or empirical regularities, and, then, leads backwards by postulating *conceivable* causes for these observations or regularities. Retroduction is the selective retention or elimination phase, yielding explanations that are warranted or justified to the extent that they truly serve to explain, or help us to understand, the empirical regularities. Apart from deficiencies in the theory, the various idealizations in the model will preclude a perfect fit, the empirical regularities may be said to be explained by the theory.

We must be careful not to impute more or less to these hypothetical structures. Instrumentalists take them to be merely useful fictions; reductionists claim that they are more 'real' than the empirical regularities; and both these camps, as well as scientific realists, may claim that they are in principle unobservable. But none of these claims necessarily hold, although it is the case that with respect to specific empirical regularities, these hypothetical structures may be unobserved or, at least, non-obvious, and, hence, their proponents must appeal to a different theoretical warrant than predictability or confirmation.

Empirical and theoretical explanations should dovetail in actual scientific practice. Although, as McMullin (1984) argues, 'Aristotlean' science excluded hypothetical explanation, because of its provisional character, from the status of scientific knowledge, over the past three centuries, theoretical explanation, incorporating structural, genetic and, perhaps, dynamic explana-

tions, has become normative in the natural sciences. Nevertheless, psychologists, in the 20th century under the sway of a positivistic philosophy of science, and regardless of whether they were phenomenalists, positivists, operationists, or nominalists, have taken nomothetic explanations as foundational whereas they have viewed theoretical explanations as suspect or, more charitably, as merely heuristic. This is curious for theoretical explanations should appeal especially to scientific realists and for two reasons. First, the success of theoretical explanations is the success of science itself as a mode of inquiry and, second, a theoretical explanation can be successful only if the postulated structures are real. But, then, logical empiricism, especially in psychology, has always backed away from scientific realism.

One reason that psychologists have backed away from scientific realism is the problem of what to do with hypothetical constructs — non-referring concepts — on a non-reductionistic view of their subject matter. The 'logical' devices which were intended to ground these hypothetical concepts empirically, either failed or led to reductionism. Theoretical concepts were either assumed to refer to hypothetical brain structures, or to structures of mind in which case they were blatantly hypothetical and, in principle, unobservable. Contemporary cognitive psychologists, eager to distance themselves from the strictures of logical behaviorism, are either reductionists with high hopes or else formalists whose postulated algorithms and heuristics are neither hypothetical nor structures of mind or brain. Between these biologists and formalists we are in danger of losing the discipline of psychology.

Scientific Explanation. But which type of explanation is more basic? Consider Carl Hempel's (1959/1942) famous example of deductive-nomological explanation. "The slush on the sidewalk remained liquid during the frost because it had been sprinkled with salt." We note that the explanans (sprinkled with salt) explains the explanandum (slush on the sidewalk) only if, (1) a person seeking the explanation was unaware that the sidewalk had been sprinkled with salt, or, (2) if a person did not know the effect of sprinkling salt on snow. But if someone, who knowingly sprinkled the snow with salt, which then melted, were to ask for an explanation, it would be a weak explanation indeed to answer that salt always has this effect on snow. This nomothetic explanation is satisfactory only insofar as the questioner learns that the explanandum falls under a particular description of a natural regularity. But subsumption even under a causal regularity is insufficient as a scientific explanation or, at least, it demands much more. What we want to know is *why* salt has this effect on snow and it does not help much to be told that salt always has this melting effect on snow. What we want is a theoretical explanation and, presumably, one in terms of a postulated molecular structure.

Deductive-nomothetic explanations, as I noted above, may appeal to more than one empirical regularity which, in turn, may be ordered into some

kind of hypothetical, deductive-like pattern. Take the following contrived example: "John hit his sister because she teased him." To explain why John hit his sister, we might refer to him being teased and the effects of teasing. Someone who understood the referents of these terms quite well might still find it explanatory to be told that "frustration causes aggression," that "John had a low frustration-aggression threshold," that "sibling rivalry causes frustration," and so forth, for the manner in which we combine these empirical regularities, together with the initial conditions (John being teased by his sister) turn out to be quite complex and, therefore, a nomothetic explanation can indeed be very helpful. But questions remain: Why does frustration cause aggression? Why is John's frustration-aggression threshold so low? Why does the sibling rivalry between John and his sister result in frustration?, and so forth.

Notable in both examples is that explanations are given of singular occurrences, precisely the sort of events that students interested in the applications of psychology would be expected to understand and explain. Thus, one would expect students interested in applied psychology to be quite compulsive about learning as many empirical regularities, nomothetic laws, about human behavior as they might be able to find in the sciences. But while the practitioner of psychology might do so, the scientist is surely not, first of all, interested in the explanation of singular events. 'Galilean' science, as Kurt Lewin (1935/1931)pointed out long ago, is characteristically the theoretical explanation of empirical regularities which are themselves already one step removed from the natural world. The scientist is not attempting to explain causal complexity of the natural world but the empirical regularities, the 'facts', derived from controlled observation or experiment. It is precisely their success, or lack thereof, which provides the justification for their theories. The application of these theories in understanding singular events always and necessarily involves an imperfect understanding, and it is usually left by scientists to practitioners to resolve the lack of 'fit' between theory and the observed event.

Nomothetic explanations are not trivial. Unfortunately, when researchers take them to be the end-all of scientific inquiry, the discipline tends to be barren and lack integration (Koch, 1981; Staats, 1987). Under these conditions, we ought not to be surprised that bodies of empirical observations are often 'lost', that empirical regularities are said to be without understanding, or that there seems to be little by way of theoretical progress. It is precisely under a deductive-nomological explanatory view of science, that we cling to fiduciary and non-rational views of scientific progress. Empirical regularities simply cannot serve to extend our conceptual resources which are fundamentally responsible for 'discovering' causal structures and formulating theoretical explanations.

Evidently, those with applied interests are not merely satisfied to explain an individual's ideation and actions by subsuming these under nomothetic laws. For the language of nomothetic laws is frequently the same language that describes the individual's ideation and actions. Rather, their concern is to explain singular events, as instances of regularities, such that the individual may come to understand his ideation and action within the social-cultural context of daily living. This quest is surely as theoretical as it is practical. Nothing is so practical as good theory, at least for the practitioner, and nothing is so theoretical as a good explanation, for both the practioner and the researcher.

Theoretical Warrant. If theoretical explanation is the fundamental form of explanation in science, why is it suspect? One answer, and one we glean, for example, from the history of opposition to psychoanalytic theory, is that theoretical explanations are just wrong in the *prediction* of singular events. Moreover, they frequently require that one accept the entire hypothetical structure postulated by the theory to account for the empirical regularities which are themselves empirical regularities formulated under the model. Thus, it is said that the structural model of psychoanalytic theory holds only for psychoanalytically established empirical regularities, and not for the way people really behave!

But is this a failure? Only if we hold theoretical explanations to be true, when they are not. And, indeed, there are those who hold more tenaciously to the truth of theoretical laws and explain all failure in their application to singular events as due to the complexity of the world in which the singular event is embedded. These true believers invoke all manner of *ad hoc* accounts to justify the failure of their theoretical explanations to explain the singular event. But this is not the only option, surely, for theoretical laws and their postulated structures may also be wrong. Moreover, 'Galilean' idealization to which theoretical explanation is committed, implies that theoretical explanations are, initially at least, and perhaps for may years thereafter, separated from the causally complex natural world (McMullin, 1985). The ultimate success of theoretical explanations is their explanation of empirical regularities, or classes of naturally occurring situations; the fact that they cannot immediately explain singular events should not be counted as a liability.

In case of the structural models offered by psychoanalytic theory (and it is not our task here to defend psychoanalysis), these too must stand or fall with their success in explaining what it takes to be the empirical regularities of human behavior. However, the difficulty in psychology, and the social sciences more generally, is that the empirical regularities to be accounted for are themselves problematic. The empirical regularities which are the explanada of psychoanalytic explanations are most often formulated in ordinary language at the level of lived experience (Grünbaum, 1984), or else in terms of

psychoanalytic concepts which are themselves derived from the theory. All this is deemed to be in sharp contrast, for example, to the formulation of empirical regularities in physicalist or operationist terms, in 'theory-neutral' terms, characteristic of the discipline more generally. But the apparent theory-ladeness of the explanada in psychoanalytic theory, and the consequent charge of circularity, only begs for further theoretical clarification, especially as to the appropriate level of description for psychology.

Ironically, the post-positivist claim that our theoretical explanations are not subject to 'instant assessment', has been taken by many to justify a retreat to the 'data' for, at least these permit of nomothetic formulation and, hence, are progressive. If theoretical explanations are to be tolerated at all, then only so as heuristics, which have no truth value but belong strictly to the realm of speculation or, perhaps, discovery. This residual positivism has various suppressed premises, one of which is that psychology, as a natural science, must first formulate thoery-neutral empirical regularities and, once these are in place, it can begin the task of formulating deeper, theoretical, explanations. The irony of this point of view is that our cumulative empirical regularities should be most useful to the practitioner and yet, we find that researchers reject the applications of psychology as premature, while practitioners find the empirical regularities to be inadequate as explanations of singular events. But, then, our scientific explanations were never merely nomothetic, but theoretically infused by what we assumed to be the scientific 'data', and our understanding of singular events was always thoroughly theoretical, even if the theory was one given with the meaning of our linguistic descriptions.

It may be objected that I have misrepresented the nature of scientific investigation. For, after all, scientists are interested in theoretical explanations but only when the hypothetical structures, that are postulated to be causally related to the empirical regularities, can also be causally 'traced back' from these regularities (as hypothetical-deductive explanations were *de facto* employed). To maintain this view is to be interested in theory and, also, to remain a scientific realist. In fact, it is rather remarkable that many experimental researchers still find themselves in tacit agreement with the kind of hierarchical explanatory model proposed by Henry Margenau (1950) wherein theoretical concepts at all levels are either inductively derived from the empirical regularities, and, eventually, the raw observations, or else deductively confirmed by these same regularities and observations. But the difficulty with this view is that such causal 'tracing-back' can only be accomplished through theory, and the deductive confirmation of theory by the observations ('instant assessment') turned out to be mythical. The causal is indissolubly linked to the theoretical and not the inductively inferential, or the deductively observational (Weimer, 1984; Rozeboom, 1984).

Logicism versus Value-ladeness

In spite of the demise of positivistic philosophy of science, psychology, in its preference for nomothetic over theoretical explanations, still adheres to a fact-value distinction, and finds the purported theory-ladeness of inquiry a threat to the 'integrity' of its science (Mos, 1987). But just what sorts of theory permeates scientific inquiry and just how do theory considerations undermine the 'integrity' of its explanations?

Inductive inference, that stronghold of deductive-nomological explanation to which positivists assimilated all forms of ampliative inference, usually involves standard techniques of sampling and curve-fitting. Of course, neither technique is free from assumptions: sampling and measurement assumptions, mathematical assumptions of simplicity and extreme values, and, above all, assumptions pertaining to the nature of the meaning of the variables. While commonplace, these assumptions are blatantly theoretical. Even nomothetic laws do not just fit the world and the assumptions inherent in their formulation must be evaluated. However, there is little here which is contentious, at least in the natural sciences, and maximizing the 'values' of these 'epistemic variables' tends to be a pragmatic and routine affair.

Not so, however, when it comes to ampliative inference, for here we are dealing with explanatory theory, whose hypothetical structures are the products of the creative imagination of the individual investigator. The literature is replete with discussions of criteria in terms of which explanatory theory may be appraised or evaluated: predictive accuracy, internal and external coherence, unifying power, fertility and a host of other criteria or values. This is not the place to review these, except to note that the pre-eminent criterion of predictive accuracy is not as important, especially in the early years of a theory, as it is sometimes held to be for, after all, an *ad hoc* theory might well be predictively accurate. In contrast, that amorphous criterion of fertility, namely a theory's capacity to predict new domains of explananda, is perhaps more important, especially in the long-term, than is generally acknolwedged. It is, after all, the fertility of a theory, in the long term, which is crucial in evaluating its truth-likeness.

Unlike the theoretical assumptions which inhere in the formulation of nomothetic laws, the assumptions of theory evaluation, as Kuhn (1977) argued so convincingly, are values. It is the *manner* in which we instantiate these values in the appraisal of a theory which constitute an argument for the truth of the theory. While, presumably, none of these values are equivalent, separately or as an aggregate, to the ultimate value which is the truth of the theory, our scientific understanding, the determination of the truth of our explanations, is permeated with value judgments.

Of course, all this was known long before the current omnitheoretical philosophy of science. But psychologists have forgotten all that. The Aristot-

lean ideal of certainty was resurrected in the positivistic ideal of scientific knowledge as deductive-nomological. Within the context of justification, all values were eschewed in favor of one value, namely, confirmation (or, falsification). The acquisition and progress of scientific knowledge was to be a logical endeavor, objective, free from values, and certain.

This myth of logicism was undermined by the recognition that, historically, our scientific rationality are theory-laden; that our theories and explanations are complexly and problematically related to the empirical regularities and the singular events they subsume. It was also undermined by the recognition that our scientific rationality is always hypothetical and that our explanations are bounded only by our understanding. Indeed, some of the positivists even realized that the failure of logicism extended beyond rationality to include observation itself. For if there was no logic of language neither was there a logic of objects; the data constituted by our empirical observations were also inescapably value-laden. As Richard Rorty (1979) argued so well, there is no picture-of-the-world language, and the classical empiricist assumption about there being available a purely descriptive, and unproblematic, language of observation was itself without foundation.

Post-positivistic philosophy of science which holds that the entirety of our scientific knowing involves theory, or value, determinations is far-reaching. For one thing, it helps us to understand the persistent and pervasive controversy, at all levels of scientific inquiry which has characterized our discipline from its inception. If there were but one scientific method, then controversy ought to be easily resolvable, but it is not. Moreover, while the assumptions involved in theory appraisal may be conceived of in general epistemological terms, even as 'epistemic' values, they can only be justified within the context of the history and culture of our scientific practices. That is to say, there is no *a priori* way of knowing how the assumptions of theory appraisal are to be instantiated in the practice of science. The philosophy of science and, hence, the philosophy of psychology, is a historical-empirical, communal, enterprise, just as is science itself, disclosing our evolving notions of scientific rationality.

It may well be objected that the epistemic values, listed above, are not the kinds of values which are usually mentioned by those who insist on the fact-value distinction. Epistemic values are not social, political, economic, moral, or religious values. But this objection fails to appreciate the point that once *any* values are admitted into the endeavor of science, the question then becomes one of deciding which values belong and which do not. This question not only leaves the fact-value distinction without justification, but brings us to the abyss that separates the natural from the social sciences.

Value-ladeness and psychological science. The programatic status of psychological science is difficult to account for on the 'presumption of standard rationality', as stipulated by the 'received view' of the logic of explanation.

However, once we realize that our explanations are infused with epistemic values and our language does not picture the world, the presumption of standard rationality not only gives way, but becomes itself a theoretical 'object', one which encompasses values external to those epistemic values already inherent in positivistic science. In fact, the presumption of standard rationality as informed by positivistic science was based, from the inception of our discipline, on non-epistemic value assumptions of what constituted the subject matter of psychology. It remains a question of some contention whether or not these non-epistemic values were explicitly part of the presumption of standard rationality or whether these values imposed themselves, unwittingly as it were, on our efforts to pose psychological questions and formulate psychological answers. Certainly a large part of post-positivistic philosophy of science has been devoted to enlarging the boundaries of scientific rationality. Not merely are there epistemic values inherent in our explanations, but psychological, scientific inquiry is throughout embedded in the social-historical life forms that characterize our individual and communal understanding.

But what remains of objectivity and truth on such an unbounded view of rationality? Surely in the natural sciences the hypothetical structures postulated by successful theoretical explanations are deemed to exist. Natural scientists are likely to react with incredulity at the suggestion that these structures are merely convenient fictions. Yet, even here, recent challenges to realism suggest that the objectivity of our knowledge of the world is not so much a question of *methodology* as it is a question of the adequacy of our theoretical explanations. The break is not between ourselves and the world, but between what we have explained or understood and what is as yet beyond our understanding and resists our explanations. Explanatory success leads us to believe in the truth of our theories or, better, in the existence of those structures postulated by our theories. What is understood is real and, as Martin Heidegger (1982/1959) was wont to say, only that which we understand can we think.

Psychology, and the social sciences more generally, study human actions which are themselves value guided, and the intended and unintended consequences of those actions which are embedded in a world of praxis. Our explanations always presuppose a mutuality of understanding which is conditional on the polyvalences of our language. It is in our *understanding* of human action and its consequences, that the subject matter of psychological inquiry acquires its initial objectivity which culminates, through a sensitive elaboration of its full context, in an explanation, a new understanding, which aspires to finality, or truth. Just as successful theoretical explanations in the natural sciences disclose the reality of their postulated structures, just so, by analogy, do explanations in the human sciences, through interpretation, clarification, and systematic argumentation, disclose the reality of the meaning and significance of human conduct. But the analogy is more than con-

venient. For what is 'postulated' by the human sciences investigator, in an attempt to formulate explanations, are 'points of view' and 'perspectives', "pre-judices," as Hans-Georg Gadamer (1976) would have it, which are our "effectivehistorical" reality, lived and understood. These pre-judices inescapably encompass all thos epistemic and non-epistemic values which bear on the questions we pose and the answers we give as members of linguistic and "disputatious communities" (Campbell, 1986).

Nor is theory appraisal simply by-passed on this hermeneutical endeavor which is psychology. While hermeneutical explanations are inevitably theoretical, value-laden, the understanding they yield is held to be warranted only insofar as the probe of further questions, inquiry beyond the explanada understood, yields no new understanding, both to those whose actions we are attempting to understand and to those who quest it is to understand their actions. That this may appear to be an endless task is only to acknowledge that each generation must first submit itself to the task of recovering those psychological explanations which in-formed its predecessors.

Theoretical Supremacy

The theoretical nature, and value-ladeness, of scientific explanation has led to a view that science, including psychology, is a search for understanding, with all other technical and logical efforts, however important, as subsidiary. However, this conception of explanation is a no less demanding endeavor. Theoretical explanations may appeal to numerous and diverse discourse genres, and involve the descernment of values deeply embedded in the peoples and cultures which are its explananda. The rationality in which those theories are anchored is the rationality of our 'lived experience', the world as we understand and live it. Not every guess is a hypothesis and not every hypothesis can be denominated a theory. *Theoria* is contemplation, and contemplation the way, the *met-hodos*, to understanding.

Students whose intuitions embrace the applied and the theoretical exemplify a pre-understanding of the intimate relationship in their lieves between theory and action. What those, fewer students, with research and theoretical intersts have in common with them, is the recognition that their respective endeavors are thoroughly theoretical and value-laden. What all psychology students share is that their endeavors are rooted in historical and communal life forms which constitute their self-understanding as a prerequisite of learning to formulate explanations.

References

Butterfield, H. (1968/1949). *The origins of modern science 1300-1800* (Revised and enlarged edition). Toronto: Clarke, Irwin & Company Ltd.

Campbell, D. T. (1986). Science's social system of validity-enhancing collective belief change and the problems of the social sciences. In D.W. Fiske & R.A. Schweder (Eds.), *Metatheory in social science* (pp. 108-135). Chicago: University of Chicago Press.

Carnap. R. (1937). Testability and meaning. Part IV. *Philosophy of Science, 4,* 1-40.

Gadamer, H-G. (1976). *Philosophical hermeneutics.* (Translated by D.E. Linge). Berkeley, CA: University of California Press.

Grünbaum, A. (1984). *The foundations of psychoanalysis.* Berkeley, CA: University of California Press.

Heidegger, M. (1982/1959). *On the way to language.* (Translated by P.D. Hertz). San Francisco: Harper & Row.

Hempel, C. (1942). The function of general laws in history. *Journal of Philosophy, 39,* 35-48.

Hempel, C. (1966). *Philosophy of natural science.* Englewood Cliffs, NJ: Prentice-Hall.

Hesse, M. B. (1974). *The structure of scientific inference.* Berkeley, CA: Berkeley University Press.

Holton, G. (1973). *Thematic origins of scientific thought: Kepler to Einstein.* Cambridge, MA: Harvard University Press.

Koch, S. (1981). The nature and limits of psychological knowledge. *American Psychologist, 36,* 257-269.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd. ed.). Chicago: University of Chicago Press.

Kuhn, T. S. (1977). *The essential tension.* Chicago: University of Chicago Press.

Lewin, K. (1931). Aristotlean versus Galilean modes of explanation. *Journal of General Psychology, 5,* 141-177.

MacCorquodale, K., & Meehl, P. E. (1948). On a distinction between hypothetical constructs and intervening variables. *Psychological Review, 55,* 95-107.

Margenau, H. (1950). *The nature of physical reality.* New York: McGraw-Hill.

McMullin, E. (1978). VIII. Structural explanation. *America Philosophical Quarterly, 15(2),* 139-147.

McMullin, E. (1984). Two ideals of explanation in natural science. *Studies in philosophy, IX,* 205-220.

McMullin, E. (1985). Truth and explanatory success. *Proceedings American Catholic Philosophical Association, 59,* 206-231.

Mos, L. P. (1987). Integrity or unity? In A.W. Staats & L. P. Mos (Eds.), *Annals of theoretical psychology, Volume 5* (pp. 345-347). New York: Plenum.

Putnam, H. (1988). *Representation and reality.* Cambridge, MA: The MIT Press.

Resher, N. (1978). *Peirce's philosophy of science.* Notre Dame: Notre Dame University Press.

Rorty, R. (1979). *Philosophy and the mirror of nature.* Princeton: Princeton University Press.

Rozeboom, W. W. (1972). Problems in the psycho-philosophy of knowledge. In J. R. Royce & W. W. Rozeboom (Eds.), *The psychology of knowing* (pp. 25-109). New York: Gordon & Breach.

Rozeboom, W. W. (1984). Dispositions do explain. In J. R. Royce & L. P. Mos (Eds.), *Annals of Theoretical Psychology*, Volume 1 (pp. 204-223). New York: Plenum.

Staats, A. W. (1987). Unified positivism: philosophy for a unification psychology. In Wm J. Baker, M. Hyland, H. van Rappard, & A. W. Staats (Eds.), *Studies in theoretical psychology* (pp. 297-316). Amsterdam: North Holland.

Weimer, W. B. (1984). Limitations of the dispositional analysis of behavior. In J. R. Royce & L. P. Mos (Eds.), *Annals of Theoretical Psychology, Volume 1* (pp. 161-204, 225-232). New York: Plenum.

DO MENTAL EVENTS EXIST?[1]

Fred Vollmer

SUMMARY: We think we have beliefs, desires, thoughts, and intentions, and that such states and events determine what we do. We are wrong, says Churchland. All we have are brains, and processes and states in our brain determine our behavior. Churchland claims, however, that if we learned to respond to our sensations with concepts from modern physics and neurophysiology, our perception of ourselves (and the world) would change radically and become more true. Instead of experiencing colors and pains, we would see electromagnetic waves and feel the firing of neurons. It is argued that this position is highly implausible, both on conceptual and empirical grounds, and that, though all observation may be theory dependent, there are limits to how the raw material of sensation can be perceptually organized.

Introduction

The idea that the human and social sciences are somehow different from the natural sciences, has always been at war with the view that science is a unitary enterprise. Early in this century psychologists and social scientists were told that all phenomena were reducible to physical ones. More recently the positivist position was that all scientific methodologies were variants of the Covering Law Model of explanation typically used in natural science. The latest and most radical version of scientism is that the whole ontology on which the human sciences are based is false and should be eliminated and replaced by the natural science conception of reality.

Problem

Eliminative materialism, as formulated by Paul Churchland (1979, 1981, 1984), is based on a general theory of knowledge according to which our sensations are effects of, and so contain information about, objective properties of the environment. But just what a sensation is a sensation of objectively cannot be read off from the intrinsic quality of the sensation itself. That is a wholly contingent matter that has to be found out by science. It is thus conceivable that sensations having totally different intrinsic qualities might be caused by, and so objectively be sensations of, the very same physical property.

What a sensation is a sensation of subjectively — that is, what it is *judged* to be a sensation of — has neither anything to do with its intrinsic quality, but depends on how we have learned to respond to it conceptually. And there are

always many different possible ways of responding conceptually to any given sensation.

According to Churchland:

> The objective intentionalities of the various sensations ... is ... a contingent matter, a matter of what features of the environment prompt their occurrence. And their subjective intentionalities are equally relational and contingent, being a matter of which of the many possible conceptual frameworks has been acquired as the habitual matrix of conceptual response to their occurrence (Churchland, 1979, p. 25).

The common sense conceptual framework we currently use is one according to which our sensations are judged to be of such things as oranges, chairs, keys, wines, shadows, roads, and numerous other things — and of properties like red, sweet, sharp, wet, hot, soft, silent, clear, strong, fast, slippery, and so on. But according to natural science, our common sense understanding of the world is wrong. What our sensations are sensations of objectively are not things like yellow bananas, but complex molecular aggregates and physical processes. The common sense conceptual framework is leading us to misperceive things badly. So the rational thing to do, according to Churchland, is to eliminate this false theory and start responding to our sensations with scientific concepts. Our observation judgments would then become truer descriptions of the objective intentionalities of our sensations, and we would no longer observe such things as yellow bananas, but instead molecular aggregates selectively reflecting certain wavelengths of light. To quote P. S. Churchland (1986) "theory revision may entail a revision of what phenomena are believed to exist and of what properties they are 'observed' to have" (p. 247). To illustrate this, Paul Churchland asks us to imagine a:

> ... culture or society in which the bulk of our ordinary empirical concepts are neither used nor even remembered, a society whose 'ordinary' 'common sense' conception of reality is the conception embodied in modern physical theory. In the process of language learning their children are taught to respond, in observational situations, with the relevant expressions from that theory. Where (roughly) we learn 'is red', they learn 'selectively reflects EM waves at ...'; where (roughly) we learn 'loud noise', they learn 'large amplitude atmospheric compression waves'; where (roughly) we learn 'is warm', they learn 'has a mean molecular KE of about ...'; where (roughly) we learn 'is sour', they learn 'has a high relative concentration of hydrogen ions'; and so on.... These people do not sit on the beach and listen to the steady roar of the pounding surf. They sit on the beach and listen to ... aperiodic atmospheric compression waves ... They do not observe the western sky redden as the sun sets. They observe the wavelength distribution of incoming solar radiation shift towards the longer wavelengths ... They do not feel common objects grow cooler ... They feel the molecular KE of common aggregates dwindle ... (Churchland, 1979, pp. 28-30).

Not only do physical things outside us cause us to have sensations — our own internal physiological processes do too. On the basis of such sensations we judge ourselves to have mental states like pain, hunger, desire, belief, thought, image, feeling, perception, and others. That we have such states we

think of as facts, something directly experienced and indubitable. But according to Churchland "... an introspective judgment is just an instance of an acquired habit of conceptual response to one's internal states, and the integrity of any particular response is always contingent on the integrity of the acquired conceptual framework (theory) in which the response is framed" (1981, p. 70).

Unfortunately the conceptual framework that leads us to judge that we have beliefs, desires, and other mental states, is just a special part of that same common sense theory of reality that misleads us to believe that we see the western sky redden as the sun sets. And the common sense theory that we have mental states is just as false as the common sense ontology of the outer physical world. According to natural science all we have inside us are physical states. What we should judge our internally produced sensations to be of, then, are bodily and not mental states. Instead of judging that we are thirsty, have pains, color-sensations, desires, and beliefs, we should learn to make observation reports about our brain states and other physiological processes inside us. Our observation judgments about ourselves would then match the objective intentionalities of our internally produced sensations.

In ascribing mental states to other people, we are not, according to Churchland, making observation judgments or responding conceptually to anything we have sensations of. We are framing hypotheses about non-observable states and processes inside them in order to explain and predict their behavior. But the conceptual framework we use to form such hypotheses about others is the same false theory we use to express observation judgments about ourselves. That other people have mental states is as false as that we ourselves have them. What we all have are brains, and brains are what determine our behavior.

But if there really are no such things as beliefs, wants, and intentions, neither in others nor in ourselves, the whole bottom drops out of the human sciences. For what these sciences claim to study, in contrast to the natural sciences, are phenomena that have meaning. But how can behavior, language, and other human products have meaning without rational agents that have intentions, beliefs, and desires? If eliminative materialism is right, then, the *Geisteswissenschaften*, have lost their *meaning*. And what is to become of psychology if there no longer are any psyches to investigate? If we want to stay in business, then, we had better ask some critical questions about Churchland's position.

The foundation of the position, is, as we have seen, a theory of knowledge. In the following, therefore, I shall discuss this theory of knowledge and attempt to show that it is highly implausible, and that it does not explain away what seems obvious to us all: that mental events exist.

Discussion

To start with: if mental events do not exist, how can we have sensations, and learn to make conceptual responses to them?

1. One possibility is that the sensations we respond to conceptually are conceived of by Churchland as purely physical events, reactions in the sense organs and nervous system to external and internal stimulation. But this way of interpreting what may be meant by having sensations fits poorly with other things Churchland says. Sensations are held to have their own intrinsic qualities. And while Churchland here may be talking about some special properties of neural processes in the sensory pathways and cortex, the only plausible interpretation is that he is referring to the 'qualia' of sensations, to the fact that sensations have their own special *experiential* qualities. This interpretation is supported by the fact that Churchland always speaks of sensations and their intrinsic qualities in a negative way. They "... might even be dispensed with so far as the business of learning and theorizing about the world is concerned" (1979, p. 15). Surely Churchland isn't claiming here that brain states could be dispensed with?

But if we have sensations with special experiential qualities, and these experiential qualities are what we respond to conceptually, how can Churchland then claim that mental events do not exist? Isn't a sensation with experiential qualities a mental event?

To have sensations with intrinsic qualities, according to Churchland, we need no theory. All we need is a nervous system, and receptors tuned in to the physical stimuli inside and outside our bodies.

Observing-perceiving-apprehending certain things with certain properties, is something else again. To see things having properties, we need concepts. Perception is a conceptual response to sensation — a conjecture concerning what is being sensed, a "judgment to the effect that something or other is ø" (Churchland, 1979, p. 14). That perception is a judgment, doesn't mean that it necessarily involves expressing an observation *statement* about what is being sensed. The judgment may simply consist in *seeing* that something is, for example, red.

What concepts we have determine what things we see and what properties we observe/judge them to have. So that if we had no color concepts, for example, we would not see any red objects. And if we had no concept of pain, we would not feel any pain. Given the right concepts, what we might see were EM waves, and what we might feel were events in our nervous systems.

If our theories concerning what things are there in the world and what properties they have — are false — do not match up with what is *really* there — we may see things and properties that do not really exist — and fail to see the things and properties that really *do* exist. Among the things that were previously observed but did not exist, were witches and caloric fluids. Among

the things still observed without really existing are colors and pains; and along with pains: thoughts, memories, feelings, desires, beliefs, intentions, and all other so-called mental events.

To deny the existence of mental events, then, is not to deny that we *have* sensations. It is to say that we are misinterpreting our sensations.

The reason why the propositions 'mental events do not exist' and 'we have senations with intrinsic qualities' sound contradictory to us, if they do, is that 'having sensations with intrinsic qualities' is taken to mean: experiencing such things as colors, sounds, pains, desires, thoughts, and feelings. But 'red', 'blue' and 'thirst' are not ground-floor descriptions of what our sensations and their intrinsic qualities are really like, in themselves, at the pure experiential (sensational), pre-theoretical level. While such a level does exist, there is no single correct description of it — no set of categories that can be read off it directly, and that mirrors it perfectly and objectively. Any description, also one like 'is red', is already a theory — one among several possible ways of grasping what is being sensed. "Human knowledge", says Churchland, "is without propositional or judgmental foundations ... there is no special subset of the set of human beliefs that is justificationally foundational for all the rest" (1979, p. 49).

2. Assuming that there is no contradiction involved in holding that we have sensations but no mental states — what exactly does our error consist in when we judge that things have colors and we ourselves pains? According to Churchland we are wrong about the objective intentionalities or causes of our sensations. When, in response to a visual sensation, I say 'that thing is red', or I just *see* that it is red — my judgment fails to mention, or I just don't see, what my sense organs are really responding to, (what my sensations are really sensations of).

But, it could be pointed out, if we have sensations, and these have intrinsic qualities as well as physical causes, as Churchland seems to admit, surely it must be possible to say two things about them: what they are like intrinsically, and what they are effects of. So, granted that 'red', 'blue', 'pain' and 'thirst' are theoretical and only *possible* ways of describing (or responding to) sensations — they may be possible ways of describing the intrinsic features and not the causes of sensations. And, to say that a judgment like 'I am in pain' is wrong, because it fails to pick out the physical cause of my sensation, would be a mistake if the judgment were not at all about the physical cause, but about the intrinsic quality of the sensation. This, moreover, may be a mistake Churchland is making, because the only kind of judgment he seems to think of as possible in response to a sensation, is one (a judgment) about its physical cause.

The following reply could now be made: in claiming that 'red' is descriptive of a sensational quality — whereas 'EM-wave' is not — but only descriptive of the cause of a sensational quality — you are tyring to sneak back in the

(old) empiricist distinction between observational and theoretical terms. You are trying to make us regard statements like 'that thing is red' as direct descriptions of the data — and statements like 'that thing is reflecting EM waves' as inferences about hidden processes — made on the basis of sensory evidence — but not as direct descriptions of that evidence. But Churchland's claim is that 'EM wave' is a non-inferential response — an observation judgment — a judgment about what is being seen — and not an inference/hypothesis about something non-observed. Churchland's imaginary people literally see EM waves instead of colors — they don't infer the presence of EM waves on the basis of seeing colors. (One could say they respond with the concept EM wave on the basis of having certain sensations. But so do we respond with 'red' on the basis of having certain sensations. The one response is no more direct or less inferential than the other).

In thinking that there could be a special vocabulary for identifying the intrinsic qualities *themselves* — and another for referring to their causes — you are still dominated by the illusion that the intrinsic qualities of sensations are really like something themselves, that they have some definite identity of their own that presents itself to us, that we ourselves do not create through our concepts, but which, on the contrary, our observational concepts are formed to grasp, and which, in consequence, has a determining influence on what we perceive.

But sensations have no such physiognomy of their own. They constitute a totally neutral material that in itself contains *no* directives for constructing observational concepts — but which numerous different observation languages can be made to fit — and which, in consequence, can form the basis for seeing such widely different things as EM waves and colors — and feeling such widely different things as pains and the firings of C-fibres. To quote Churchland again, to the extent that sensations have any identity of their own "... the intrinsic qualitative identity of ones' sensations is *irrelevant* to what properties one can or does perceive the world as displaying" (1979, p. 15, my italics).

But, I ask back, still in the grip of the empiricist cramp, what can it mean to say that a sensation — for example, the sensation I have when I have broken my leg — is something totally neutral — and that what I subsequently perceive under such conditions depends utterly on my concepts? Surely it can't mean that my leg would stop *hurting* — if I stopped calling the sensation a pain — but said instead that some of my C-fibres were firing. If that were the case, one could use words as anesthetics instead of, for example, morphine.

But if there is something distinct about the sensation I have when I break a leg — that cannot be changed by any theory — namely that it hurts — and is quite different from a tickle or an orgasm, then it can't be altogether wrong to think that sensations are really like something themselves, that they have some definite identity of their own that presents itself to us, and that they are

not a completely neutral material. But if these sensations with their intrinsic qualities are what we build our perceptions out of, isn't it also reasonable to think that our perceptions, at least in *some* way and to *some* extent, must bear resemblance to our sensations, and that we can't just perceive *anything*?

Again it may be objected that if we accept such a viewpoint, we are right back with old-fashioned empiricism, and have simply failed to grasp mainstream modern philosophy of science since Hanson, Kuhn, and Feyerabend. But I have not said that sensations are *completely* unambiguous — and that there is one and only one set of observation categories that will fit them. With Harold Brown, I am suggesting rather that:

> the objects of perception are the results of contributions from *both* our theories and the action of the external world on our sense organs. Because of this dual source of our percepts, objects can be seen in many different ways, but it does not follow that a given object can be seen in any way at all. Consider again the duck/rabbit. We have already seen that this figure can be seen as a duck, a rabbit, a set of lines, or an area, and one might plausibly imagine its being seen as a piece of laboratory apparatus, a religious symbol, or some other animal by an observer with the appropriate experience. But try as I will, I cannot see this figure as my wife, the Washington monument, or a herd of swine. Unlike the Kantian position, or, rather, one interpretation of the Kantian position, I do not maintain that theories impose structure on a *neutral* material. The dichotomy between the view of perception as the passive observation of objects which are whatever they appear to be and perception as the creation of perceptual objects out of nothing is by no means exhaustive. A third possibility is that we shape our percepts out of an already structured but still malleable material. This perceptual material, whatever it may be, will serve to limit the class of possible constructs without dictating a unique percept (Brown, 1977, p. 93).

Accepting, then, that sensation is always a material with many potentials — that can be formed, differentiated, and apprehended in many possible different ways — all of which are equally much 'there' in the sensations — is not the same as accepting that we can learn to perceive anything at all in these sensations.

But if there are limits to how the raw material of sensation can be perceptually organized — what are those limits? More specifically, is there any reason to assume that a sensation can be shaped in such a way as to directly display its physical cause? Churchland seems to think there is scientific evidence for the assumption. Thus, in referring to the results of biofeedback experiments, he writes that

> There appears to be a great deal about our physiological and neurological activities — activities currently opaque to us — that we can *come to* recognize introspectively, given the concepts with which to classify them and the training necessary to apply those concepts reliably in non-inferential judgments (Churchland, 1979, p. 119).

In the typical biofeedback experiment some physiological process, which a person normally has no awareness of and no control over, like the electrical activity in a part of his brain, is recorded from electrodes on the outside of his

body and the resulting information fed back to him. For instance, every time the state of the person's brain is characterized by alpha activity, he hears a tone and is told to try to keep the tone on as long as possible. At the end of a training period of this kind, the amount of alpha activity is significantly higher than normal.

But does this mean that the feedback stimulus has made the subject become directly aware of his own brain activity — and that achieved awareness of the brain process itself is what leads to control over it? To answer this question, Black, Cott, and Pavlovski (1977) designed an experiment comprising ordinary feedback training, and in addition discrimination training. In discrimination training trials, when alpha or non-alpha activity of a certain duration was occurring, the subject heard a tone, and had to indicate which state he thought his brain was in. He was then immediately informed whether he was correct or not. Subjects first received a series of discrimination trials, then a series of feedback trials, and finally an additional block of discrimination trials. It is of interest to note that the subjects in this experiment were chosen among faculty members and graduate students knowledgeable in the research area of biofeedback, since, according to the authors "subjects who knew more about the apparent behavioral and psychological concomitants of CNS electrical activity would more easily develop a discrimination" (p. 113).

In accord with results of previous studies it was found that feedback training led to significant changes in the occurrence of alpha and non-alpha activity. But the results of discrimination training were wholly negative. In spite of having both the concepts with which to classify their brain events and the training necessary to apply those concepts, none of the subjects learned to discriminate alpha from non-alpha activity. It was concluded

> that subjects cannot learn to discriminate the presence or absence of occipital alpha activity ... voluntary control of a CNS response can be achieved without the ability to discriminate that response, that is, without awareness of the response. In short, awareness is not necessary for voluntary control ... it appears highly unlikely that there exist direct sensory concomitants of occipital alpha and non alpha activity (Black, Cott, & Pavlovski, 1977, p. 117).

So the scientific evidence for Churchland's supposition is a bit shaky. But even if it were so that, for example, different patterns of electrical activity in the brain were registered by special receptors and produced in us different sensations that we could learn to become increasingly aware of, discriminate, and use to tell which state our brain was in, the question remains whether those sensations could ever be formed (in such a way as) to directly reveal their physical causes. This is highly unlikely if we accept that what we can perceptually make out of sensation is in some way limited and determined by a pre-given structure of sensation itself. For such a dependence implies that the relation between perception and sensation must be what Østerberg (1966) calls an inner one — where what manifests itself in perception was already

in latent form in sensation — and where what is now clearly grasped, and what was previously vaguely apprehended, are in a sense the same thing that has undergone a change or development, and not two entirely different and only extrinsically related things. But nothing could be more extrinsic than the relation between sensations and their physical causes, if we are to believe Churchland. Sensations themselves can never tell us anything about their physical causes. Totally different sensations could have been caused by the same physical stimuli — and the same sensations by totally different physical stimuli. All this suggests the idea of realities that are radically different — that bear no resemblance to one another whatsoever. This being so, it is utterly mysterious how responding to sensations with concepts from natural science could ever make those sensations change into and be seen as their physical causes. Seeing the duck/rabbit figure as one's wife would be nothing in comparison.

To sum up. We have been discussing the nature of sensations. According to Churchland, sensations are what we shape our perceptions out of. The concepts we apply to sensations, determine what we see. If we have the right concepts, we may perceive the physical causes of our sensations. The subjective intentionalities of sensations come to match their objective intentionalities. By responding with concepts like 'red' and 'pain' — we come to see things that aren't there.

Against this theory it has been argued that sensations are not a completely amorphous/neutral material, but that they have structures/qualities of their own, and that they cannot, therefore, be interpreted just anyway. Specifically, it makes no sense to assume that, by learning physics and neurophysiology, sensations can come to be seen as their physical causes. It does, of course, make sense to assume that one can learn to *infer* the presence of physical events on the basis of sensations.

The fact, then, that terms like 'red' and 'pain' do not describe the physical causes (or objective intentionalities) of sensations, does not mean that these terms do not describe anything at all, or that the pain I feel in my leg and the color I see on the wall, do not really exist. For if we have sensations with their own intrinsic qualities, that are caused by, but distinct from, physical events with physical properties, then there is something more than physical events with physical properties, there is something *there* in addition to brain states and EM waves, and terms such as 'red' and 'pain' may be possible ways of describing what this something more is like.

And here it makes no sense to argue that 'pain' and 'red' just describe the subjective side of things and not how "they really are in their innermost nature ...." To argue that though "The red surface of an apple does not look like a matrix of molecules reflecting photons at certain critical wavelengths, ... that is what it is." That though "The sound of a flute does not *sound* like a sinusoidal compression wave train in the atmosphere, ... that is what it is."

That though "The warmth of the summer air does not *feel* like the mean kinetic energy of millions of tiny molecules, ... that is what it is." That though "one's pains and hopes and beliefs do not *introspectively* seem like electrochemical states in a neural network" (Churchland, 1984, p. 15), that is what they are. As long as it is true that an apple does look like something, that a flute does sound like something, that the summer air feels like something, and that one's pains, hopes, and beliefs seem like something — and that an apple can never look like a matrix of molecules reflecting photons, a flute can never sound like a sinusoidal compression wave train, the summer air can never feel like the kinetic energy of millions of tiny molecules, and one's pains, hopes and beliefs never seem like electrochemical states, that is all psychology needs to stay in business. For the task of psychology *is* to study the subjective side of things — how they may look, sound, feel, or otherwise seem to people. That is what psyche is. How things 'really' are, in themselves, is the job of physics to tell. And if that is the only story that is told, something will be left out.

But is there any point in knowing and telling what a flute may sound like to someone, if this is just a subjective phenomenon, a phenomenon taking place so to speak on the surface of reality? I hear NN playing Bach's suite for flute (No. 2 in B minor). But this 'fact' only exists because a number of physical events in my environment and body take place that give me sensations that I have learned to hear as a certain piece of music. The same kinds of physical events might be taking place in another person's environment and body without him hearing Bach's suite for flute. (He may have heard very little classical music before.)

While such remarks may be correct, what do they imply? That describing the environments of people in terms of what they see and hear should be avoided — since what they see and hear in any situation depends on themselves, varies from person to person, and so does not tell what really and objectively goes on?

But why should psychology or any other human or social science want to describe what the environments of people are 'really' like — if that involves just describing physical features that are not there *for* anyone — or only there as inferred entitites? That would only make sense if it were in fact the physical characteristics of environments — and not how they were perceived — that determined behavior. But the reason why I clapped my hands after the concert, whereas the other person did not, was that I heard NN render a beautiful interpretation of Bach's suite for flute and orchestra, and the other person did not — not because different kinds of physical events were taking place in our environments and bodies, which indeed may not have been the case.

In general, if one accepts the idea that there is behavior that originates in us as persons — then anyone who wants to explain such behavior, will have

to find out why *we*, the agents, produced it. And that involves investigating such things as our beliefs, intentions and perceptions. For if we ever do anything at all, it is because of what we see, hear, believe and intend. These are grammatical remarks. In explanatory contexts, words like 'person' and 'action' belong together with words like 'belief', 'intention', and 'awareness' — not with 'EM waves', 'kinetic energies', and 'electrochemical states'. 'p did A because he saw that B had happened, believed that C was the case, and intended that D should happen' — makes sense. But not 'p did A because of compression waves in his physical environment and electrochemical states in his brain'. And as argued, nor does it make sense to assume that the immediate aspects of our environment *that* we see and hear, that we form beliefs and intentions *about*, and that constitutes the context of our practical actions, can ever be anything like EM waves and neural states. In other words, while it makes sense to say that 'p clapped his hands because he heard the beautiful interpretation, wanted to express how moved he was, and believed that clapping was the appropriate way to do this', it makes no sense to say that 'p clapped his hands because he heard sinusoidal compression waves and was aware of certain electrochemical states of his brain'.

One can, of course, reject the whole notion that there are any such entitites as persons who do things — and claim that all there are, are organisms whose movements are strict effects of physical states and events inside and outside their bodies. In such a system, however, it makes no sense to introduce entities like *sensations*. Nor does it make sense to assume that these organisms learn *concepts*, and that by responding to their sensations with these concepts they come to *perceive* and *understand* the world and themselves in a certain way. This is bringing in words from language games where persons belong, and leads to deep confusion. If one wants to deny that there are any such beings as persons, one must be consistent and eliminate the whole common sense ontology where persons have a place. Churchland's theory of knowledge and his philosophy of mind just do not fit together.

But what if we just drop Churchland's theory of knowledge which is full of many of the states and events which his philosophy of mind denies exist? Then his materialistic position boils down to the postulate that there are no such things as mental states. People are not aware of anything, they have no sensations, feelings, thoughts, intentions, beliefs, memories, dreams, and so forth. If I then say: 'but I *do*! At least it *seems* to me that I see a red ball, feel pain in my tooth, believe that R is president of the U.S.', and so forth. What can Churchland reply? He can't reply that this is just an illusion, something I *think* is the case because I'm responding to my sensations with the wrong concepts. For then he is using the theory of knowledge which he has to get rid of. All he can say is: 'you just don't *have* them!' To which I can reply again: 'But I *do*.'

References

Black, A. H., Cott, A., & Pavlovski, R. (1977). The operant learning theory approach to biofeedback training. In G. E. Schwartz & J. Beatty (Eds.), *Biofeedback, theory and research* (pp. 89-127). New York: Academic Press.

Brown, H. I. (1977). *Perception, theory and commitment. The new philosophy of science.* Chicago: University of Chicago Press.

Churchland, P. M. (1979). *Scientific realism and the plasticity of mind.* Cambridge: Cambridge University Press.

Churchland, P. M. (1981). Eliminative materialism and the propositional attitudes. *The Journal of Philosophy, 2,* 67-90.

Churchland, P. M. (1984). *Matter and consciousness.* Cambridge, MA: MIT Press.

Churchland, P. S. (1986). Replies to comments. *Inquiry, 29,* 241-272.

Østerberg, D. (1966). *Forståelsesformer.* Oslo: Pax.

WHAT DISTINGUISHES LAY PERSONS' PSYCHOLOGICAL EXPLANATIONS FROM THOSE OF PSYCHOLOGISTS?

Henderikus J. Stam

SUMMARY: An examination of the impact of psychology on culture leads to the not-so-startling conclusion that psychological explanations have fared badly when compared to ordinary language explanations of psychological events. I review a number of arguments proffered by psychologists that attempt to account for this failure of scientific discourse to change people's self understandings. Then I address the nature of psychological explanations and contrast these to lay explanations of human action and argue that psychology must retain the mental as its elemental data. In doing so, however, we are still faced with the need for constructing a framework within which to couch psychological explanations. Here I argue that psychological explanations for human action cannot be reductive and must acknowledge that mental events are embedded in the discursive practices of a human community that shares linguistic and cultural practices.

Despite the quantity of psychological literature produced in recent years, psychology has had little impact on lay persons' explanations of their own actions (Thorngate, 1988; Thorngate & Plouffe, 1987). Ordinary language explanations, or, if you will, common sense explanations are preferred almost exclusively to professional psychological ones. In addition, many people turn to a vast self-help literature when their problems are no longer amenable to explanation via the tools of ordinary language. This literature is deliberately non-scientific and premised on vague and often meaningless understandings of persons.

Nevertheless, one could argue that the majority of the 100,000 (plus) psychologists working in North America alone, are actually professional psychologists who see clients in one capacity or another. Through this contact, at least, psychological concepts must seep into public usage. Unfortunately, this vast coterie of professional psychologists is a rather unusual lot. If the research on the psychotherapeutic process has told us anything, it is that in order for the psychologist to be intelligible to her client she must at the very least use constructs and language understandable to her clients. Furthermore, most practicing psychologists describe themselves as 'eclectic', that is, they use whatever they feel is necessary for the situation and problem at hand without allegiance to any particular school or method, scientific or otherwise (Garfield & Kurtz, 1976). Not a reliable way to pass on a science (Slife, 1987).

Perhaps the most obvious sign of the difficulty psychology has in addressing human action occurs when psychologists speak amongst themselves. If

you have ever listened to a group of psychologists discuss the actions of family members, colleagues, or better yet, the performance of a department head or dean, you will rapidly discover how quickly their conceptual tools are left behind in the laboratory. Their accounts are intelligible only as lay explanations and have little in common with the professional pronouncements of the psychologists themselves.

But is this argument nothing but a straw person? After all, at least 99% of the world has probably never heard of Wittgenstein, yet one would be hard put to declare his work a failure. Acceptance should, therefore, not be a criterion of the utility of a new science, or any academic enterprise for that matter. This holds especially, one might argue, for public acceptance. The public is notoriously fickle and uninterested in the vagaries of academic debate.

A second argument often proffered by psychologists themselves sometimes follows these lines: Of course we have trouble explaining such things as the creation of Hamlet or the Moonlight Sonata, but who wouldn't? Aren't these examples, after all, of the highest achievements of culture? Or alternatively, some pscyhologists argue, we *have* had an impact on society. Most educated people immediately associate reinforcement with Skinner and the unconscious with Freud. Other less glamorous concepts that have found their way into ordinary language include such notions as identity crisis, extraversion and introversion, Type A behavior, and so on.

Allow me to address these points in turn and then move to the major arguments of this paper. In short I don't believe they are sound. The first argument is reasonable in so far as making academic endeavors conditional on acceptance may lead one to ignore new or interesting or original work that is not clearly understood or appreciated in its formative stages. Nevertheless, acceptance is already part of our internal criteria for academic success — certainly that is why we have peer review for journals. More telling, however, are the many claims made by psychologists themselves for the applicability of their theories and research. Psychologists claim expertise in solving personal problems, family problems, educational problems, neuropsychological problems. Psychologists claim an ability to assess a wide variety of human problems and dispositions for a wide variety of situations including personnel selection, educational placement, neurological lesions, and so on. Psychologists have tried their hand at treating neuroses, psychoses, schizophrenia, somatoform disorders, child abuse, wife abuse, drug abuse, sexual dysfunction, familial dysfunction, to name but a very few. Thus, it is not for lack of trying that psychological explanations have not fared well with the public.

The second argument alternately points to the difficulty of explaining the best that culture has to offer or to the argument that psychological concepts are already well embedded in this culture. I contend that it is not only Hamlet

or Beethoven that are difficult to explain by mainstream psychology but that
it is equally difficult to find explanations for the plays produced by amateur
theater groups and piano recitals produced by children. Or, to go further,
most people would find little use in psychology to help them explain those
nagging, ordinary problems of daily life such as the sort one encounters in
raising children, living with significant others, developing friendships, work-
ing with colleagues, and so on. There is a rich cultural lore to draw on for such
occasions and psychology seems remote and abstract by comparison. And a
quick perusal of psychological terms that have entered the larger culture
indicates that once they leave the professional domain their meanings quickly
become altered and fluid. The term 'unconscious', for example, can refer to a
variety of actions or experiences, depending on where, when and by whom it
is uttered.

Psychologists have recognized their lack of impact from time to time.
The problem is typically conceptualized as a problem of translation. For
example, in his 1969 Presidential Address before the American Psychological
Association, George Miller proposed what became a frequently cited solu-
tion. He argued that the world was "in serious need of many more psychologi-
cal technologists who can apply our science to the personal and social
problems of the general public". Furthermore, argued Miller, "when the
ideas are made sufficiently concrete and explicit, the scientific foundations of
psychology can be grasped by sixth-grade children." But Miller's paper is
probably most remembered for its slogan that psychologists should "give
psychology away." By this he meant that,

> Psychological facts should be passed out freely to all who need and can use
> them. And from successful applications of psychological principles the
> public may gain a better appreciation for the power of the new conception
> of [human beings] that is emerging from our science. ... Our responsibility is
> less to assume the role of experts and try to apply psychology ourselves than
> to give it away to the people who really need it — and that includes everyone.
> The practice of valid psychology by nonpsychologists will inevitably change
> people's conception of themselves and what they can do. When we have
> accomplished that, we will really have caused a psychological revolution
> (Miller, 1969).

Miller was hardly alone in his call for radical applications of scientific
psychology. Among others, Skinner's (1986, 1987) more recent writings have
returned to the persistent theme that his brand of behaviorism could do a
great deal for education, child rearing, workers' alienation, and world peace
to name but some applications. The failure of this strategy is relatively
obvious if we recall that the psychological literature and the number of
psychologists in the world has more than doubled since Miller's pronounce-
ments were made, and the impact of psychologists' explanations remains a
vague promise at its best. To paraphrase Guthrie's famous criticism of
Tolman's cognitive theory, the psychologist remains buried in thought at the
choice point. Should we ignore the inability to make the science of psychol-

ogy relevant, and forge ahead with the oft stated hope that psychology is a young science and maturity is far off, or do we heed the warning signals that are apparent but inconclusive, and reconstruct the discipline?

These warning signals, I would argue, have been close at hand for some time. What I will call the contextual critique of psychology, for lack of a better global term, is informed by recent developments in the philosophy of science, sociology of knowledge as well as other interdisciplinary developments. I will not try to summarize the implications of these positions for psychology, but instead want to return to the major problem of this paper where elements of the contextual critique will gradually be implicated.

What is the nature of psychologists' explanations? To attempt an answer to this I will rely on two recent works for my inspiration, one by Margolis (1984) and the other by Robinson (1985), although I must take final responsibility for what follows. (See Thorngate & Plouffe, 1987, for a slightly different treatment of this problem.)

The simple answer to the question of the nature of psychological explanations is that there is no single explanatory convention within the discipline of psychology. Assume a simple 'normal', psychological event: Aunt Sally is writing a letter to Uncle John. Now we can apply a variety of explanations to this event: She is writing because of her personality; because she was motivated; because she is intelligent enough for the task; and so on. But such explanations are circular and add nothing to our understanding of the event. They are prerequisite to letter writing perhaps but do not say much more (cf. Robinson, 1985). A different sort of explanation is created when we say she is writing because of an unconscious desire or because letter writing was a highly valued activity in her childhood. But unconscious motives cannot be incorporated into a conscious action sequence of the sort described here unless we deny Aunt Sally the status of agency. So far we do not want to do this given that we see nothing abnormal about Aunt Sally's act. That letter writing was valued in her childhood might be an interesting comment but does not account for Aunt Sally's writing this letter at this time. Thus far, any of these explanations might provide a context, or in Robinson's words, may be taken as permissive rather than determinative. But undaunted, we carry on. Consider the following statements. Aunty Sally is writing Uncle John:

1) because of her reinforcement history

2) because she is functionally equivalent to a writing machine

3) because she has a cognitive letter-writing schema

4) because of conditions in Broca's area in the left hemisphere of her brain

5) because Uncle John was two months in arrears in his child support payments.

You will no doubt recognize an important difference in these statements, especially in the juxtaposition of the first four to the last. The first four are varieties of explanation common to psychology, the last is one of many possible statements one might use in a conversation about your Aunt Sally with, say, another relative.

There are several species of arguments that have been mustered for and against the first four explanations. What I should like to argue is that they give an incomplete accounting of our Aunt Sally's act. For example, we could counter the claim that Aunt Sally is writing because of her reinforcement history by resorting to a Chomsky-like (Chomsky, 1959) proposal that, for any natural language, the processes of sentence formation have no finite limit whereas the reinforcement of operants requires some finite state process. Hence a Skinnerian account of letter writing must fail. Or we might simply agree with Nelson (1969) that internal states are not logically dispensable because dispositions and internal states are not extensionally equivalent. Therefore, this account also fails if only as an explanation for the unique event. We might want to hedge our bets that some characterization of the term reinforcement (even if we only use it synonymously with encouragement) could account for Aunt Sally's writing but not for her writing this precise letter (Robinson, 1985).

Our second explanation stated that Aunt Sally was like a writing machine. Assume now that we have some adequate conception of a very sophisticated writing machine that produced letters of the sort Aunt Sally is writing given appropriate inputs. Such a functional account has a long and venerable history in the social sciences and philosophy. Functional accounts, however, are largely incomplete explanations insofar as implicit in most functional accounts is the search for a physical realization of those functions. Fodor (1968) was perhaps most explicit about this in his distinction between 'first phase' and 'second phase' psychological explanations. The first phase refers to finding explanations that determine the functional character of the states and processes involved in the etiology of behavior. The second phase however "has to do with the specification of those biochemical systems that do, in fact, exhibit the functional characteristics enumerated by phase-one theories" (p. 109). The great majority of functional explanations are of the first sort, or "weakly equivalent" in Fodor's terms. There may be many machines that emulate some aspect of human behavior or functional abilities of other systems without ever executing the same manoeuvers in their emulation. We might then say that these machines are not psychologically endowed and provide a partial explanation at best. Functionalism, in psychology at least, points to the possibility of a reductive explanation at the cost of ignoring the cultural setting within which human beings exist. If, as I shall argue later, an adequate human psychology requires social and historical processes for a complete accounting, then any infrapsychological functional explanation is partial at best.

A similar argument can be mustered against the cognitive letter-writing schema explanation. There are several issues here that I will briefly describe but I will not claim to do justice to them. First, it is not clear how the term schema is to be understood in modern psychology. It frequently passes for no more than a kind of functional explanation of the sort I have just described. On the other hand, one might conceive of a cognitive schema along the lines of one of the varieties of cognitivism currently available. Unfortunately, there are more than a few of these. Some are philosophical in origin, such as Fodor's or Dennett's, others more psychological, such as those of Newell or Piaget, and others are concerned primarily with artificial intelligence (AI). The latter, in particular, have lead to a great deal of misunderstanding if only because we often fail to separate the many practical and technological achievements of AI with the thesis that artificial intelligence can, in principle, account for human psychological processes. In Robinson's words (1985, p. 103), "we can agree that the tape recorder is a wonderful device without regarding it as a contribution to our knowledge of human memory." But whether we espouse, say, a molar, homuncular, or nativist version of cognitivism, I concur with Margolis (1984, p. 85) that "there are no plausible or compelling grounds for postulating a psychologically internalized system capable of generating *all* the cognitively pertinent behavior of human agents." Whether functionalist or explicitly physicalist, cognitive or representational theories have failed as accounts of agency and intentionality.

I also want to argue against the explanation of Aunt Sally's letter writing as a result of a condition of her brain. Various versions of such attempts at elimination of the mental are possible both within and without psychology. This is, of course, also reminiscent of John Watson's version of behaviorism, which was to replace all talk of psychological events with concepts built up out of reflexes and stimulus response units (Watson, 1925). Suffice it to say that whatever brain talk we use to replace the mental must take on the original reporting role of the mental if we are to make sense of Aunt Sally's action. Eliminating the mental leaves us without the possibility of discussing a range of phenomena which have been characterized as possessing at least some of these properties; immateriality, abstractness, indubitability, privileged access, intentionality, phenomenal properties, agency, introspectability, and privacy (Margolis, 1984). Mental events in one form or another have seemingly survived the many attempts at elimination and remain the primary data of psychology. Despite the failure to exhibit uniform properties, mental life cannot be eliminated or replaced in favor of more homogeneous problems in biology or computer science. Robinson (1985, p. 89) makes perhaps the strongest claim in this regard when he argues that "whether or not psychology can 'get along just as well' without reference to inner life, consciousness, private mental states, etc., is a question of strategy, not ontology, and the force of the claim has little behind it".

Having argued that psychology must retain mental life as its elemental data, we have not solved the problem of what a reasonable psychological account might look like. Perhaps we can work our way back to this issue by asking first what the distinction is between the explanations we have thus far considered and the explanation proposed as a possible ordinary language explanation in our example, the one which stated simply that Aunt Sally wrote Uncle John because he was two months in arrears in his child support payments. Letter writing in this instance is immediately intelligible to psychologist and non-psychologist alike. First, such an explanation characterizes Aunt Sally as an active agent. The action in this case is intelligible because it does not consist merely of discrete responses or behaviors but is more akin to a performance. Furthermore, the entire sequence implies intentionality, in Brentano's sense of the term.

There is a second distinguishing element to this and most ordinary language explanations of motivated action, namely its teleological quality. Aunt Sally's action is purposeful or goal-directed. Such a quality is very much missing from our psychological accounts of her act. The reason for this is rather obvious, perhaps, to the experimental psychologist. Ordinary language explanations are frequently circular. But worse, they are incorrigible. We may determine from the contents of the letter whether it is in fact the case that Aunt Sally is writing Uncle John about support payments. But even in the presence of evidence to the contrary, if Aunt Sally claims that the symbols written on the page are a request for the monies owing then we cannot say she is provably wrong. We may think her claim odd or unusual for any number of reasons, but we cannot say 'you did not intend this'. But even if 'normal' conditions do not prevail, and Aunt Sally has just now been given a large dose of phenothiazines (antipsychotics) or is suffering from a slow growing neoplasm that is destroying brain tissue, we may still not give the reason for Aunt Sally's letter as being other than what she states. We may argue about the causes in this case, but (contra Davidson) not the reasons. That is, Aunt Sally's action is connected to a reason or an intelligible attempt to write Uncle John. Her ingestion of drugs or her tumor may be the cause of her psychological state, this much we must allow on most accounts of the biological sciences. Nevertheless, the reasons for which she is writing Uncle John while she is in this state are not the direct result (cause) of her bodily condition.

The implications of this account may be viewed as a species of the bifurcation thesis which argues that in general, the natural and human sciences are systematically different on methodological grounds. Bifurcation is frequently implicit within psychology itself; there are psychological processes explicable solely in terms that include the biological and exclude or at least minimize cultural variables. For example, infants can discriminate nearly all of the phonetic contrasts of human speech, even if they lose this ability in later life (Eimas, 1975). Thus all biologically normal infants can discriminate between the sounds of r and l, although Japanese adults cannot. Such

discrimination in infants is purposeful in some sense, but one might argue that it does not require an agent for discrimination to occur. On the other hand, fluent adult native speakers of the Japanese language are intending agents, even if they cannot now discriminate between *r* and *l*.

The teleological understanding of human action is intimately tied to people's self-understandings of their own actions. Such an approach will not readily lead us to scientific explanations of the sort exemplified by the psychological explanations we have already discussed. Hence we have now arrived at a point of seemingly major incompatibility between psychologists' explanations and lay psychological explanations of even a relatively simple action.

But this incompatibility is perhaps less real than we might admit once we examine the nature of some research in social, personality, clinical, and developmental psychology. Take a long line of hypnosis research, for example, that has argued that hypnosis and hypnotic phenomena are best accounted for by reference to concepts taken from social and cognitive psychology. This research has argued that we do not need to explain hypnotic acts as the result of a trance, a special state of consciousness or some other specialized, esoteric explanation. The concepts used to explain hypnosis in this case are categories of explanation that are explicitly goal-directed and emphasize the relational nature of hypnotic responding (e.g., Sarbin & Coe, 1972). The research results in this case are not causal, only rational. They require that the readers of such results be participants in the culture and that they apply criteria of plausibility and reasonableness to the results (Robinson, 1985). It is difficult to see how such explanations, which are preminently teleologic, can ever be construed as causal. Unfortunately, in this research area and most others, psychologists themselves continue to couch such explanations in causal terminology. The naive or neophyte reader who approaches such literatures as exist in social or clinical psychology is readily confused by seemingly rational descriptions couched in a scientistic language.

It may be then that a lay persons's psychological explanation and a psychologist's explanation are perhaps not always that clearly distinguishable. Each relies on some more or less rational characterization of mental life. If we agree however that it is implausible to postulate a psychologically internalized system that is capable of generating all the psychological events of human agents, we need a better account of what we mean by the mental. Such an account would, at the least, acknowledge that mental events themselves are embedded in the discursive practices of a human community that shares linguistic and cultural practices. On Margolis' account, those practices in turn cannot be accounted for or described *solely* in terms of the infrapsychological powers of the members of such a community.

Hence one reason that psychology is more appropriately a human science derives from the major properties of language, namely, that language

appears to be wholly unique; "essential to the actual aptitudes of human beings, irreducible to physical processes ... real only as embedded in the practices of a historical society, ... incapable of being formulated as a closed system of rules, subject always to the need for improvisational interpretation" and so on (Margolis, 1984, p. 90). According to Ricoeur (1981), all distinctly human aptitudes can be considered "lingual". Thus aptitudes as diverse as writing a letter to Uncle John, playing cards and building airplanes all presuppose linguistic ability. The notion of 'text' as developed by writers such as Ricoeur, Barthes and others is a direct consequence of this idea. In general it refers to the treatment of objects or realities as discourse (cf. Stam, 1987).

It is my contention that if psychology were to give up its rigid adherence to falsely placed hopes of reductive explanations for a broader view of itself as a human, lingual endeavor we could develop psychological explanations that take lay accounts seriously, rather than setting the two in continual opposition (e.g., Shotter, 1984). Or, in other words, if we were even to treat lay explanations as some kind of account of human action, that in itself would be some advance.

References

Chomsky, N. (1959). Review of B. F. Skinner, *Verbal behavior*. *Language*, *35*, 26-58.

Eimas, P. D. (1975). Speech perception in early infancy. In L. B. Cohen & P. Salapatek, (Eds.), *Infant perception: From sensation to cognition* (Vol. 2, pp. 249-267). New York: Academic Press.

Fodor, J. A. (1968). *Psychological explanation*. New York: Random House.

Garfield, S., & Kurtz, R. (1976). Clinical psychologists in the 1970s. *American Psychologist*, *31*, 1-9.

Margolis, J. (1984). *Philosophy of Psychology*. Englewood Cliffs, N. J.: Prentice-Hall.

Miller, G. A. (1969). Psychology as a means of promoting human welfare. *American Psychologist*, *24*, 1063-1075.

Nelson, R. J. (1969). Behaviorism is false. *Journal of Philosophy*, *66*, 417-452.

Ricoeur, P. (1981). *Hermeneutics and the human sciences*. Cambridge: Cambridge University Press.

Robinson, D. N. (1985). *Philosophy of psychology*. New York: Columbia University Press.

Sarbin, T. R., & Coe, W. C. (1972). *Hypnosis: A social psychological analysis of influence communication*. New York: Holt, Rinehart & Winston.

Shotter, J. (1984). *Social accountability and selfhood*. Oxford: Basil Blackwell.

Skinner, B. F. (1986). What is wrong with daily life in the western world? *American Psychologist, 41*, 568-574.

Skinner, B. F. (1987). Whatever happened to psychology as the science of behavior? *American Psychologist, 42*, 780-786.

Slife, B. (1987). The perils of eclecticism as therapeutic orientation. *Theoretical and Philosophical Psychology, 7*, 94-103.

Stam, H. J. (1987). The psychology of control: A textual critique. In H. J. Stam, T. B. Rogers, & K. J. Gergen, (Eds.), *The analysis of psychological theory: Metapsychological perspectives* (pp. 131-156). Washington: Hemisphere.

Thorngate, W. (1988). On paying attention. In W. Baker, L. Mos, H. Rappard, & H. Stam (Eds.), *Recent trends in theoretical psychology* (pp. 247-263). New York: Springer-Verlag.

Thorngate, W., & Plouffe, L. (1987). The consumption of psychological knowledge. In H. J. Stam, T. B. Rogers, & K. J. Gergen, (Eds.), *The analysis of psychological theory: Metapsychological perspectives* (pp. 61-91). Washington: Hemisphere.

Watson, J. B. (1925). *Behaviorism*. London: Kegan Paul, Trench & Trubner.

THEORY OF ACTION IN PSYCHOLOGY:
A NARRATIVE PERSPECTIVE

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SUMMARY: This chapter proposes a narrative theory of action. From a narrative perspective, actions are viewed as meaningful expressions which are integrated into a larger whole: the story of life. Explanation of action takes the form of interpretation or hermeneutic understanding. The phenomenology of Merleau-Ponty provides an example of a narrative approach to human action. The notions of dialogue and style in Merleau-Ponty's philosophy are especially relevant. A narrative theory of action may actually play a role in contemporary human sciences. Such a role is demonstrated through a discussion of a possibile narrative approach to the psychology of addiction.

Introduction

In the past decades several attempts have been made to develop a theory of action which might serve as a foundation for psychology. Psychologists became interested in the philosophy of action because they were dissatisfied with the behaviorist tradition in psychological research. They asked for a more encompassing perspective in which specifically human aspects of behavior could be taken into account. Analytic philosophy of action presented itself as an alternative, stressing aspects of intentionality and individuality.

After a promising start the application of philosophy of action to the field of psychology appeared to be more difficult than expected. One of the reasons for this is that analytic philosophy is mainly interested in individual actions, whereas psychology deals with larger patterns of actions. The relation between action and structure, which is a central theme in the human sciences, is not easily integrated with a philosophy of action.

In this chapter I propose a narrative theory of action. The notion of narratio (story) fits into the basic ideas of philosophy of action in that it stresses non-natural aspects of human behavior. It may prove useful to psychology as it accentuates the position of individual events within a larger structure.

In the first section I will sketch the outline of a narrative theory of action. I will draw heavily on contemporary hermeneutics. In the second section I will introduce the phenomenology of Merleau-Ponty as an example of a narrative approach to human action. In the third section I will take up the question of whether a narrative theory of action may actually play a role in contemporary human sciences. I will concentrate on the possibility of a narrative approach in the psychology of addiction.

Foundations of a Narrative Theory of Action

From a narrative perspective human life can be compared with a text (Ricoeur, 1978) or a story (MacIntyre, 1981, p. 190ff). Actions and thoughts which form a part of human life are conceived as belonging to a history. Human actions are viewed as meaningful expressions which are integrated into a larger whole: the story of life.

A narrative view of human action implies a specific kind of answer to the question why an individual exhibits certain behavior. From the narrative perspective specific behavior fits into the manner in which the individual gives meaning to his life. Emphasis is laid on the meaning or the 'point' of behavior. It is presupposed that the individual has some reason for his conduct. The individual is assumed to have the ability to justify his behavior in some way, to make clear why it is important to him to act as he does. The story someone tells about himself and the reasons he gives for his thoughts and actions contribute to the meaning of his life-story.

From a narrative perspective, explanation of action takes the form of interpretation or hermeneutic understanding. An interpretative or hermeneutic explanation reveals the meaning of a phenomenon by showing the context in which it is situated. An individual action is understood against the background of someone's life-story. Figure and background cannot be described independently. The interpretation of a phenomenon within a context also sheds new light on the context. Interpretation implies a hermeneutic circle in which part and whole, action and story, clarify one another (see Gadamer, 1960, p. 178ff, 250ff, 275ff).

The hermeneutic circle between part and whole is not limited to one story. The meaning of an event within a story does not only depend on other events within the same story, but on other stories as well. A story never exists on its own. It is part of a tradition. The meaning of a theme in a story depends on the way in which the theme is developed in other stories, which together form the culture in which the individual lives. The life-story of an individual is interwoven with the life-stories of others. The place of an element in a story can be made clear by showing how it is related to similar elements in familiar stories.

In a narrative approach there is no room for exact predictions. The internal relations between various elements of a story make it impossible to reduce a certain event to prior events. The meaning of a story as a whole is not determined when the theme and the components are given. The last sentence may throw new light on everything that preceded it. Even after the whole story has been told, the meaning may change, as it depends on the interpretations which are given of the story. Each story is part of an ongoing effective history ('Wirkungsgeschichte', Gadamer, 1960, p. 284ff). Although unpredictable, the development of a story is not arbitrary. It is the result of

an ongoing process in which the sense of the story gradually takes a concrete shape.

In a narrative theory of action it is assumed that persons express themselves in their actions and to a certain degree can be responsible for what they do. Behavior is explained not by a cause behind the back of the individual, but by showing its meaning in the individual's life. This stresses individual subjectivity, which may be defined as the way in which the individual makes sense of his life. Because the life-story of an individual is related to the stories of others and is part of a shared cultural tradition the development of subjectivity is internally related to the development of interpersonal relations. A specific element in a life-story has meaning while it refers to similar elements in other stories.

Merleau-Ponty as an Example of a Narrative Approach

Merleau-Ponty's phenomenology (Merleau-Ponty, 1945) provides an example of a narrative approach to human action. Merleau-Ponty concentrates on the relation between the human body and the world it lives in. According to him, bodily movements are part of a larger pattern of behavior, which shows how the body makes sense of the world. Bodily movements are not caused by external stimuli but are motivated by phenomena in the life-world.

To illustrate the notion of motivation Merleau-Ponty uses the example of a motivated journey (Merleau-Ponty, 1945, p. 299). A journey is motivated when it originates from certain facts. This does not mean that the facts bring about the journey through physical processes, but that they give the person a reason to make the journey. Merleau-Ponty concludes: "The motive is an antecedent which acts only by virtue of its meaning, and one should add that it is the decision which affirms the meaning and gives it its force and efficacy" (Merleau-Ponty, 1945, p. 299). In order to understand the journey one has to relate it to the motive, not as an external causal factor, but as the context which gives the journey its meaning, and which is made explicit in the journey. Merleau-Ponty says elsewhere: "As the motivated phenomenon is realized, the internal relation with the motivating phenomenon appears, and instead of only succeeding, it makes the motive explicit and understandable in such a way that it seems to have existed beforehand" (Merleau-Ponty, 1945, p. 61).

The meaningful relation between body and world can be regarded as a dialogue. In a dialogue a question elicits an answer without determining the latter. The question gives the conversation a certain direction. The meaning of the question, however, is specified in the sequel of the communication. According to Merleau-Ponty the body's engagement in the world can be regarded as an interpellation in which the body tries to make sense of the world. In the process of perception the body questions the world (Merleau-

Ponty, 1945, p. 179). On the other hand the act of perception can be regarded as a suitable answer to the question posed by a thing (Merleau-Ponty, 1945, p. 366).

The dialogical character of human action is especially clear in our way of learning how to handle things (Merleau-Ponty, 1945, p. 166ff). We learn how to manipulate an instrument by getting used to it. When we are accustomed to a hat or a cane, we no longer see them as objects. We have them 'in our hands' and they are a part of our body. Learning how to handle things means getting familiar with them, understanding them as we are familiar with other people and can understand them.

Human action presupposes that people live in a familiar world. The familiarity of the world is founded in our bodily habits. The process of habituation makes individual actions part of a larger pattern. This pattern may be called a style (Merleau-Ponty, 1945, p. 197). Action can be explained by referring to the style it is characteristic of. The style or pattern is the whole which makes the action intelligible. The relation between action and style is a hermeneutic circle, since the action is an expression of the style, which in its turn is modified by its instantiations. Like different paintings within an artistic movement, different actions within a pattern are similar but not identical. They all express the style in their own, unique way. Different expressions are unpredictable, but nevertheless they fit into the style. They are new without being arbitrary.

The concept of style is useful to characterize human subjectivity. By virtue of their style different actions may be recognized as expressions of the same person. The concept of style also implies a relation between individual actions and collective events. An individual acquires a specific pattern of behavior in communication with others, in very much the same way as a painter acquires his style within an artistic movement, interacting with the style of others. Merleau-Ponty mentions the example of the outbreak of a revolution (Merleau-Ponty, 1945, p. 507ff). A revolution is constituted out of individual patterns of behavior interfering with each other, becoming synchronized and melting into a new life-style.

The Psychology of Addiction

Addiction is usually regarded as a disease, to be treated with biomedical means. This view was propounded for the first time by the American psychiatrist Benjamin Rush at the end of the 18th Century (Levine, 1978). It was fiercely advocated by Jellinek in the years 1940-1950. Jellinek condemned the moral approach to addiction, popular in the Prohibition Movement, which implied that the inebriate was responsible for his addiction. Both Rush and Jellinek tried to replace the moral view by a definition of addiction which stresses the notion of 'loss of control' (Levine, 1978). People don't get

addicted out of their own free will. They are subjected to physical processes, caused by the qualities of an addictive substance.

Recent years have shown the emergence of a new approach to the problem of addiction. Investigators from the field of the human sciences reject the disease-model, and Jellinek's reductionism is heavily attacked (Beauchamp, 1980). These new developments are initiated by sociologists. From a sociological point of view the shortcomings of the disease-model of alcoholism are exemplified (Room, 1983). The concept of 'loss of control' is repudiated because it obscures the social context of alcoholism. Beauchamp says: "The idea of 'personal control' over alcohol is fatally ambiguous because it trains our gaze away from social, environmental, and cultural contexts that directly shape behavior" (Beauchamp, 1980, p. 80).

This new approach asks for a new direction in the psychology of addiction. Contrary to the DSM-III definition of addiction, which pervades current psychological work in this field, and which is molded on the disease-model, there is need for a psychological approach which enables us to situate the phenomenon of addiction in a wider social context. It is my contention that this can be achieved by adopting a narrative point of view, conceiving addiction as a specific life-style.

From a narrative perspective one has to look not for the cause but for the meaning of addiction. Although there may be biological factors involved, the behavior of someone who is addicted cannot be reduced to physical influences. The life of the addict shows a specific pattern, which can be understood. The meaning of this pattern is made explicit in a very provocative way in novels describing the life of addicts, for example, Malcolm Lowry's (1967) *Under the volcano*. Such books show the fascinating, inescapable 'logic' of an addicted way of life. This way of life is built up gradually, each step being in line with the one before, until there is one theme dominating every action: the need for the addictive substance.

From a narrative perspective addiction is neither the outcome of the individual choice of the person involved, nor is it caused by external forces, that is, the specific qualities of the substance. An addiction is the result of a process of habituation, in which a certain way of life is learned. This sheds new light on the question of responsibility. The addict cannot be said to be responsible, if by this his behavior is meant to be the outcome of a conscious decision. The situation of an addict may be compared to that of a black man being discriminated against. When it is said that negroes owe their low social status to themselves, the circumstances are overlooked. The same applies when alcoholics are made directly responsible for their problems (Beauchamp, 1980, p. 94f). This does not imply that the addict is in no way responsible. A way of life is not totally external to people. An alcoholic is responsible for his problematic relation with liquor in very much the same way a man or a woman can be said to be responsible for the relation with his or her

partner. The fact that there is not one moment on which someone really makes a choice for his or her partner, does not preclude from all responsibility. Addicted behavior is not the result of free will nor of deterministic processes. It is a way of life for which people are answerable, like they are answerable for a motivated journey.

A narrative approach to addiction precludes from simple solutions. It stresses the ambiguity of the relation between addict and substance. This does not imply that addicts cannot be helped. It means that a psychological contribution to the problem of addiction should start from the meaning inherent in the addict's way of life, relating individual behavior to the life-story of the addict and to the social context in which he or she is situated.

References

Beauchamp, D. E. (1980). *Beyond alcoholism*. Philadelphia: Temple University Press.

Gadamer, H.-G. (1960). *Wahrheit und Methode*. Tübingen: J. C. B. Mohr.

Levine, H. G., (1978). The discovery of addiction. *Journal of Studies on Alcohol, 39*, 143-171.

Lowry, M. (1967). *Under the volcano*. London: Cape.

MacIntyre, A. (1981). *After virtue*. London: Duckworth.

Merleau-Ponty, M. (1945). *Phenomenologie de la perception*. Paris: Gallimard.

Ricoeur, P. (1978). Der Text als Modell: hermeneutisches Verstehen. In H.-G. Gadamer & R. Boehm (Eds.), *Seminar: Die Hermeneutik und die Wissenschaften* (pp. 83-117). Frankfurt am Main: Suhrkamp Verlag.

Room, R. (1983). Sociological aspects of the disease concept of alco-holism. In R. G. Smart, F. B. Glaser, Y. Israel, H. Kalant, R. E. Popham, & W. Schmidt (Eds.), *Research advances in alcohol and drug problems. Volume 7* (pp. 47-92). New York and London: Plenum Press.

PROBABILITY AND MEANING:
A DIVISION IN BEHAVIORAL COGNITION
DIVIDING BEHAVIORAL SCIENCE

Enno Schwanenberg

SUMMARY: A critique of the role of cognition and affect in contemporary psychology. A theoretical model is proposed which brings affect and cognition in a common perspective of behavior regulation. The model conceives of affect in terms of the behavioral cognition of meaning. The model depicts two operational tests for the functional distinction between cognition and affect, in terms of the physical structures of the environment and the impact of these structures on the functioning organism. The two tests (cognitive mechanisms) operate according to opposite but conjunctive, complementary or integrative action according to the evolutionary design of the informational relationship between organism and environment. The probability test detects novelty (non-redundant features of the environment), while the meaning test secures redundancy equivalent to the mechanism of homeostasis. The implications of this model are briefly discussed.

Opening Remarks: On Introducing a Conceptual Idea in Psychology

The conceptual distinction between two functional aspects of the phenomenal world — the environment as experienced and cognized by the organism or person — developed in the course of an extended theoretical endeavor to gain a deeper perspective on the nature and function of affect (emotion, feeling, sentiment). In common psychological parlance, 'affect' is a paired associate to 'cognition', cognition being the leading term in that association. Cognition, in fact, has risen to paradigmatic status, with ever more journals being founded that carry 'cognition' or 'cognitive' in their labels. In comparison, 'affect' lags behind and appears in a dimmer light, as some sort of a residual variable which has to be taken into account for a more complete explanation of behavioral variance but which lacks the distinctive conceptual status which 'cognition' enjoys. There is, of course, the humanistic branch of academic psychology as well as the whole feeling and healing counterculture where feeling, emotion, and affect figure as key words and key experiences — but not as concepts in the scientific sense. Thanks to that *Zeitgeist* pervading the societal backyard of psychology, 'emotions', 'happiness', and so forth are also subjects entering the House of Research in psychology in growing proportions although they do not attain the figure-head status that 'cognition' occupies. The expanding research on nonverbal communication and emotion is basically an operationalist enterprise, with

concepts being used as they happen to fit. The conceptual weakness of the field is nicely documented in the Lazarus/Zajonc controversy (Lazarus, 1982, 1984; Zajonc, 1980, 1984) where the issues are still unsettled. This is so because the two authors — both leading researchers in the field — by starting from different operational grounds and having no common conceptual framework (except for common language terms which they use differently because of their different operational references) talk past each other. The controversy concerns the primacy of cognition versus the primacy of affect. Accordingly, the relationship (or association, to keep to the parlance metaphor) between cognition and affect is still open to debate — conceptually and, as a result, also empirically.

This being a less than optimal state of scientific affairs, the author presumed to be welcome to contribute to conceptual discussion and clarification by delivering a theoretical model that brings both cognition and affect in a common perspective of behavior regulation and by doing so assigns them distinct properties, thus improving on the conceptual vacuity connected to the operationalist here-and-now research. However, in replication after replication, the feedback message from the mainstream journals was that there is no need for conceptual vistas. The basic objection explicitly or implicitly amounted to the verdict: 'This isn't mainstream psychology. Deliver data, and we will accept any concept which you might attach to them'. General conceptual ideas which attempt to bring order to the medley of empirical approaches and results (and by doing so give intellectual status to them on both an intra- and an interdisciplinary level) are out of the ordinary; and only the ordinary is asked for. There were a few other voices that said otherwise, reinforcingly voices of renown, expressing that this type of labor is exactly what is needed and illuminating in the present state of affairs; but these are not those of the clerks who decide what is befitting the mainstream paradigm. Psychology seems to have misunderstood Popper's principle of falsification, directed against dangerously doctrinaire belief systems. Popper, a model intellectual, never pleaded for data as a stop to thinking. If he had ever come down to deal with empirical psychological research he might not have been too enchanted with what his principle has been turned into.

Test of Probability and Test of Meaning

For the reasons given above, the theoretical conception that locates cognition and affect in a functional model of behavior regulation has not yet been published (except for a forthcoming chapter which applies it to the discussion of a phenomenon which has long been neglected in psychological research but which is pervasive and momentous in everyday life: suggestion; Schwanenberg, in press; see also Schwanenberg, 1987). A conference on theoretical psychology appears to offer a more congenial forum for it than the average journal culture but the constraints on time are even higher than those

on space in the journals. Therefore, the theoretical model can be presented only in bare outline, without the conceptual derivations which develop it from, or against, or on the foil of general notions developed in turn by Gibson (1950), Heider (1958), Miller, Galanter, & Pribram (1960), Kelvin (1970), Zajonc (1980) and others (see Schwanenberg, 1984). In fact, the model, as shown in Figure 1, invites a purview of general notions in psychology; in doing so it facilitates interdisciplinary communication which is dependent on researchers being able to convey the import of specific results along general dimensions of knowledge and to receive and understand the knowledge from neighboring disciplines accordingly.

It enlarges upon Miller's, et al. (1960) TOTE model of behavior regulation by adding a second type of test, a test of meaning, and by accordingly changing the picture of information inlets, interactions of (cognitive) tests and (behavioral) operations, and exits (stops) of goal-directed, reflexive, or just reflective activities. Miller's, et al. scheme is modeled according to the operations of a computer program or robot (hammering nails); the test that figures in their model corresponds to the notion of information in classical information theory (Attneave, 1969).

In the present model it is retained as the probability test: as a test checking the probability structure of the environment and of possible or actual operations performed on it by the organism or person or collectivity of persons. That probability structure is anchored in the relational structure of the (external and internal) environment or even the universe, ranging from causal to stochastic relationships between events or features of events. With regard to Miller's, et al. Operate function, this probability aspect of the world in which the organism or person is situated corresponds to the constraints or degrees of freedom which it, he, or she faces. The concept of cognition gains its conceptual sovereignty and paradigmatic status by pointing to that aspect of the world as its unequivocal reference, a reference which is happily shared with the natural sciences.

The test that has been added to the Miller, et al. model is called the meaning test and it has not such fortunate reference shared with physics and chemistry although it does have a common reference with biology and the life sciences in general. Miller's, et al. functional model aptly depicts the workings of a robot but it falls short of the workings of an organism. An organism does orient itself according to the probability structure of its environment (like a robot does) but above and beyond such cognition of actual, possible, or impossible events and the relationships between them it orients itself according to the benefit or harm, the well-being or ill-being that they implicate or plainly mean for the organism. Miller's, et al. robot can get along without such a meaning test because it does not have to care for its own survival. Its programmer or designer might have installed a protection (stop and signalling) device against some foreseeable (standard) damage but it
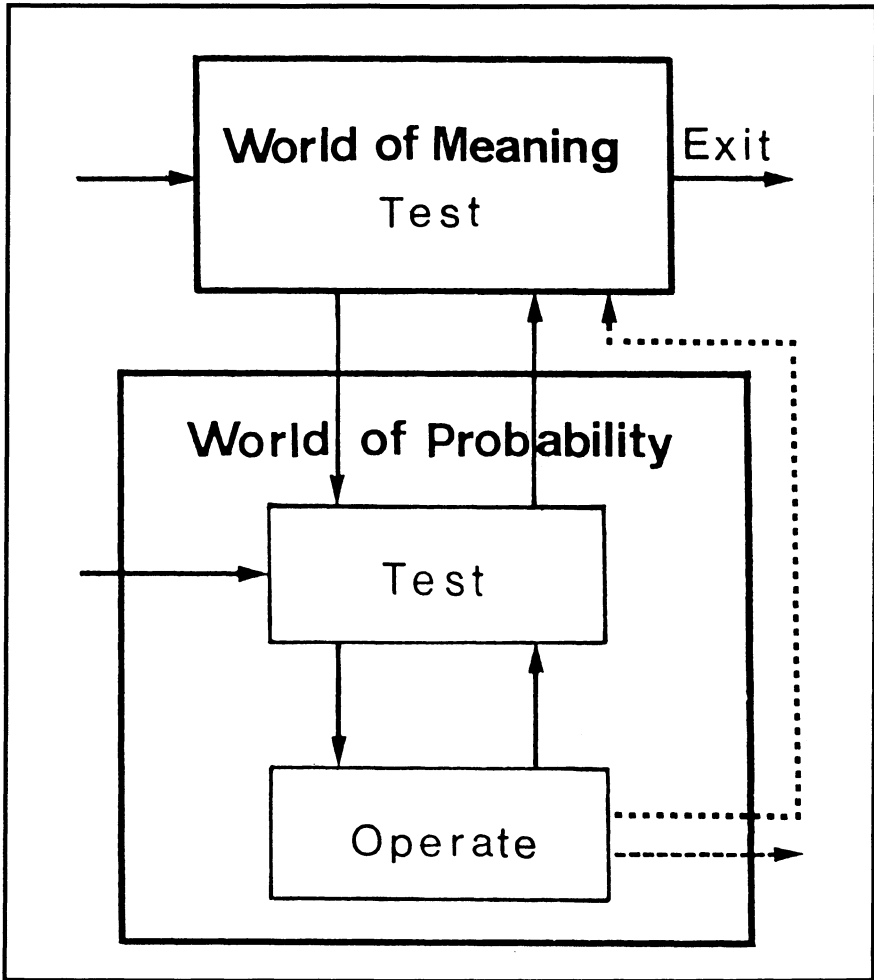
Figure 1. **Cybernetic TOTE-model comprising two different but inter-**
    acting kinds of Tests.

would be a matter of science fiction if the program or robot could be equipped
with an allround device for self-repair or a full system of needs that might keep
it 'alive' and provide for procreation.

Nevertheless, the notion of 'meaning' has puzzled the standard
psychologist in charge of editing or consulting journals, evidently being con-
nected in his or her mind with the idea of lofty speculation from the soft
sciences or of fancy counterculture babble and twaddle but not with the hard
facts of being or not-being, of life and death and the processes inbetween. In
his or her eyes, meaning is just a fuzz and not science. The fuzzy nature of the
traditional notion of meaning is, of course, intimately linked to the fuzzy

character of the traditional concept of affect. The present model attempts to
pinpoint the phenomenon of affect down to the behavioral cognition of
meaning. Perceiving or cognizing meaning results in affect. By giving mean-
ing a conceptual status in the framework of a functional model of behavior
regulation, the concept of meaning — besides entering behavioral science —
in turn provides the ground for a more focused concept of affect.

Thus the traditional distinction between cognition and affect — a dis-
tinction on the level of an empirical assortment of variables which one has
inductively to reckon with when explaining behavioral variance — is followed
through and conceptualized on the theoretical level of a general scheme that
depicts a functional division (as well as interaction) between two basic types
of tests or cognitions necessary for the functioning of organisms or persons: a
test referring to the physical structure of the environment along the co-
ordinates of time and space and the laws or irregularities of nature (including
the quasi-physical, regular or irregular structures in the social realm; see
Kelvin, 1970); and a test referring to the impact which those structures or
events have for the functioning of the organism. General approaches to the
subject of psychology could, of course, not miss this basic functional division
in the organization of behavior or action; the division is contained, for
instance, in the expectancy-times-value theories of action (Atkinson & Birch,
1978); and also in Heider's (1958) distinction between unit relations and
sentiment relations in interpersonal behavior. What the present model adds
to these conceptions is, firstly, a more general unifying notion for the func-
tional reference of the affect system: meaning. The concept of meaning
subsumes both Atkinson and Birch's (1978) notion of "value" and Heider's
(1958) notion of "sentiment relations"; it also targets the behaviorist notion
of 'consummatory' behavior (as against instrumental behavior which relates
to the probability or expectancy structure of the environment). The meaning
test, in short, is a feeling test that checks the environment as it relates to the
needs, preferences, appetites, aversions, norms, values, ideals of the organism
or person, extending from the sociobiological to the sociocultural realm.

Secondly, by applying a functional view, the model clarifies how the two
kinds of tests or cognitive mechanisms are geared to optimize the functioning
of the organisms within its environment. They operate according to opposite
principles; but their opposition functionally amounts to conjunctive,
complementary, or integrative action which according to its evolutionary
design produces an equilibrium as the optimal fit in the informational
relationship between organism and environment. The probability test is set
for detecting novelty, that is, the nonredundant events or features in or of the
environment; the meaning test is set to secure redundancy, being equivalent
to the mechanism of homeostasis. The opposite and at the same time con-
junctive action of the two tests is prototypically demonstrated in the develop-
mental co-occurrence of exploratory behavior (novelty) and attachment
behavior (constancy, safety) as first elucidated on ethological grounds by

Bowlby (1969, 1973) and subsequently corroborated impressively in study after study of child behavior.

Thirdly, the model specifies that the meaning test is superordinate to the probability test. This is no trivial statement since the paradigmatically cognitive psychologist would have it the other way around. The issue relates, of course, to the Lazarus/Zajonc controversy which would be less muddled if it would be known that there are *two types of cognitions* in a functional hierarchical arrangement which the model is intended to convey. That arrangement is conceptually not dependent on but nevertheless gives credit to lay psychology: for the layman or -woman, the natural prototype of psychology is less of a cognitive psychology (in the sense of the current paradigm) and more of an affect psychology, accounting for the appeal of psychoanalysis and similar 'meaningful' approaches. Indirect evidence also comes from Heider's (1958) lay psychology: in his attributional vocabulary, there are more terms referring to the cognition of meaning (*Suffering, Experiencing, Being Affected By, Wanting, Ought, May*) than terms referring to the cognition of probability (*Causing, Can; Try* relates to both types of cognitions).

On Speaking of Two Worlds

Figure 1 does not speak of a probability test and a meaning test but of a test relating to a world of probability and a test relating to a world of meaning. The first way of talking is more common and conventional in psychology than the second one; basically, they are complementary. The first one is identical to speaking of adaptive mechanisms *within* the organism or, as Zajonc (1980) does, of (control) systems. The second one is more akin to phenomenology by taking the perspective *from the inside out*: of the organism or person looking at its/his/her world. In a sense, the model can be designated as representing a phenomenological functionalism, deriving from Miller's, et al. (1960) "subjective behaviorism." Its phenomenological aspect is the said perspective, its functional feature is the conceptual, transphenomenal analysis of that world into two separate kinds of cognitive or informational relationships by which the organism adaptively relates to its environment. The two worlds, therefore, do not make the one phenomenal world fall apart; they are a matter of conceptual analysis for the theoretical understanding of behavior. The aspects of the environment which they refer to are integrated for the living organism into the one phenomenal world.

What is this perspective good for? Firstly, it shows that action, although deriving from the Heiderian *Want* or *Ought* located in the meaning part of the world, always takes place in the probability part — Miller's, et al. *Operate* box or function is positioned accordingly. Secondly, the terminological use of the concept of 'meaning' instead of needs, appetites, values, utilities and so forth discloses a theoretical logic: every one of the above terms involves a *cognition* referring not only to the temporal and spatial aspects of the environment but

specifically to features that relate to feeling states of the organism/person and might impel it/him/her to action. Still, the notion of 'meaning' (as opposed to motive, valence, etc.) conveys that drive-states and actions need not result from cognizing meaning; looking at a tree need not lead to climbing the tree, to cutting it down, or to carving one's initials into its bark and so forth — in most cases, when a person encounters (i.e., sees) a tree, he or she is content to *cognize* or know: this is a tree, it's really a nice tree. Thirdly, as the present discussion already indicates, the phenomenological approach and tradition (e.g., Buytendijk, 1956) is not left outside, but invited into the enterprise of psychology for mutual enrichment. Meaning, in fact, is particularly focused upon by the phenomenologists. Fourthly, the age-old, both philosophical and scientific discussion concerning the relationship between the subjective and the objective gains in perspective. Evidently, objectivity can more extensively be secured within the world of probability, a world which is more easily explored by objective (quantitative) methods and which is in part shared with the natural sciences. The meaning world is apt to be more subjective, as Wundt already knew when he discussed the nature of feelings. Still, the probability world, too, extends only so far as the respective tests operate; and Kuhn (1970), in his discussion of scientific paradigms, has pointed out that there is a definite meaning component involved in those paradigms which limits the reach of their explorations and interpretations, amounting equally to a (collective) subjective view of the world of probability.

Lastly, the notion of two worlds illuminates a state of scientific and generally human affairs in modern and postmodern times which since C. P. Snow has become part of the common intellectual lore as the cleavage of civilization (and academia) into *Two Cultures*. That cleavage can genetically be anchored in the (biological to cultural) existence of these two different kinds of tests and the resulting two different kinds of 'worlds'. Western civilization has expanded both types of tests (and 'worlds') but particularly the test and world of probability as brought to perfection and far-reaching extension by the natural sciences known as the hard sciences. The humanities, addressing the world of meaning, although following in part the lead of the natural sciences in trying to apply quantitative methods wherever possible, have a basic inclination towards the soft methods of hermeneutics. The classical debate about *Erklären versus Verstehen* — still lively in postmodernity — is thus rooted in the basic organization of cognition. On a societal level the present model will predict that, however far the expansion of the world of probability by science and technology will go, the world of meaning as the one under which the probability world is hierarchically nested will never drop from the scene but will make its recurrent appearance even in the most technologized (or probabilized) of all worlds. The plethora of video shops, sound industries, gurus, cults, therapies, drugs testifies to the no-nonsense meaning of that prediction of probable events.

References

Atkinson, J. W., & Birch, D. (1978). *Introduction to motivation* (2nd ed.). New York: Van Nostrand.

Attneave, F. (1969). *Informationstheorie in der Psychologie* (2nd ed.). Bern: Huber.

Bowlby, J. (1969). *Attachment and loss (Vol. 1): Attachment.* London: Hogarth Press.

Bowlby, J. (1973). *Attachment and loss (Vol. 2): Separation. Anxiety and anger.* London: Hogarth Press.

Buytendijk, F. J. J. (1956). *Allgemeine Theorie der menschlichen Haltung und Bewegung.* Berlin: Springer.

Gibson, J. J. (1950). *The perception of the visual world.* Boston: Houghton Mifflin.

Heider, F. (1958). *The psychology of interpersonal relations.* New York: Wiley.

Kelvin, P. (1970). *The bases of social behaviour.* London: Holt, Rinehart & Winston.

Kuhn, T. S. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago: University of Chicago Press.

Lazarus, R. S. (1982). Thoughts on the relations between emotion and cognition. *American Psychologist, 37,* 1019-1024.

Lazarus, R. S. (1984). On the primacy of cognition. *American Psychologist, 39,* 124-129.

Miller, G. A., Galanter, E., & Pribram, K. H. (1960). *Plans and the structure of behavior.* New York: Holt, Rinehart & Winston.

Schwanenberg, E. (1984). *World of probability and world of meaning: Vista on affect or Miller, Galanter, and Pribram revisited.* Paper presented at the VI. General Meeting of the European Association of Experimental Social Psychology, May 8-12, Tilburg, The Netherlands.

Schwanenberg, E. (1987). *Suggestion as a mode of social influence.* Unpublished manuscript.

Schwanenberg, E. (in press). Suggestion as social biasing of meaning tests: A Heiderian extension of the Miller, Galanter, and Pribram paradigm catalyzing McGuire's theory of attitude change. In V.A. Gheorghiu, P. Netter, H. J. Eysenck & R. Rosenthal (Eds.), *Suggestibility.* Berlin: Springer.

Zajonc, R. B. (1980). Feeling and thinking: Preferences need no inferences. *American Psychologist, 35,* 151-175.

Zajonc, R. B. (1984). On the primacy of affect. *American Psychologist, 39,* 117-123.

NATURALISM AND INTENTIONALITY[1]

H. Looren de Jong

SUMMARY: This paper discusses the notion of naturalism in relation to intentionality. Intentionality is considered 'the mark of the mental', and its reduction to physiology is a crucial issue for theoretical psychology. Naturalism seems to entail reductionism: it is usually identified with exclusive adherence to a physical level of description and a reductionist view on mentality. However, it is argued that naturalism can also be construed in a more biological way, as an evolutionary, functionalist, non-reductionist view of mind. Such a functionalist view emphasizes the role of mind in the adaptation of the organism to the environment. The computational approach to mind, which attempts to reconcile mentalism with scientific rigor, is discussed as a form of syntactic reductionism. A naturalist, non-reductionist conception of intentionality is sketched, which views mind as organism-environment relation and draws upon Searle's biologically oriented conception of intentionality and on Gibson's account of perception as reciprocity of organism and environment.

Introduction: Intentionality and Mentality

This contribution tries to sketch a naturalist view of intentionality, which on the one hand is grounded in biology, but on the other hand circumvents elimination and replacement of mental terms by neurophysiology.

Intentionality can be defined as the property of certain things (minds, maybe also computers, or even thermostats) to be about something else, to refer to some state of affairs outside itself. Mental processes (perceiving, believing, desiring, goal-directed activity) are *about* something, while physical objects are just what they are. Therefore, intentionality was proclaimed the "mark of the mental" by Brentano: the mental inexistence of an intentional object (or state of affairs) is a defining property of the mind, which clearly separates the mutually exclusive ontological categories of the mental and the physical. In this way, Brentano, and Husserl after him, used the irreducibility of intentionality to save the mental from naturalism and reductionism (de Boer, 1978).

However, it seems unacceptable to set the mind apart from the scientific investigation of nature, and many attempts have been made to reduce mental vocabulary to the non-intentional, or to eliminate intentional terms as obsolete, and to replace them by genuine scientific idiom. (Churchland, 1981; Rorty, 1982).

Intentional Behavior: What is Reduced?

Before looking into attempts to reduce intentionality let us review the 'differences that make a difference' between alleged intentional and non-intentional (causal-mechanic) behavior.

Intentional states have the property of *goal-directedness*; hence, there is an intrinsic connection between intentionality and teleological explanation in psychology (Boden, 1972, 1981). The view that intentionality is related to *representations* contains the germ of the representational theory of mind (Fodor, 1975), as an approach to mental life within the framework of scientific explanation. In this view, aspects of mind are reducible to logical, symbolic (representational) states of physical systems. Newell (1980) calls these "physical symbol systems", and thus "mind enters the physical universe" (Newell, 1980). Cognitive theories tend to define intentionality in terms of representations. It has also been emphasized (Dennett, 1987) that intentionality entails a *rational agent.*

So, intentionality is related to behavior that is goal-directed, entails representations, is knowledge-based and not caused by purely physical impulses, is rational in terms of beliefs and desires held by the organism (Dennett, 1978, 1987). Intentionality is supposed to be related to mental reference and meaning, the property of mind to reach out to, refer to, and interpret things in the world. The question then becomes, Can intentionality be accommodated in the laws of a physical, causal, and deterministic universe?

There seem to be two options, which might be called the one-plot story and the two-plot story in the philosophy of mind. The one-plot story answers the question in the affirmative, and attempts to bring intentionality under the causal laws of nature. The two-plot story wants a separate treatment for mental processes, independent of physical or physiological processes. The one-plot story tends to ignore or neglect intentionality, or even advocates downright elimination. The two-plot story tends to negelect the biological foundations and physiological make-up of intentional organisms.

Naturalism

Naturalism has often been identified with the one-plot story. It has been defined as a species of philosophical monism which holds that the laws of scientific explanation, exemplified in the natural sciences, are applicable to whatever exists or happens in the world (Danto, 1967). Naturalism holds that natural objects exist within the spatio-temporal and causal order, and that only natural causes are really explanatory. This claim of the continuity of scientific method throughout the universe entails application of the laws and methods of natural science to biology, physiology, and eventually to mental processes.

However, a slightly different definition of naturalism has been given by Fodor (1980, p. 64): naturalistic psychology says "... that psychology is a branch of biology, hence that one must view the organism as embedded in a physical environment. The psychologist's job is to trace the organism/environment interactions which constitute its behavior." This definition seems at first sight an extension of the first. It stresses the continuity of biological laws in psychology; however, it is also emphasized that organism-environment relations are the proper subject matter of psychology. This naturalistic psychology contrasts with what Fodor calls rational psychology (his own allegiance), which wants to individuate mental states without reference to the external world, upholding the cartesian claim that the character of mental processes is somehow independent of their environment (Fodor, 1980).

Naturalism and evolution. It is worth mentioning that naturalism is related to the rise of empirical research in biology and medicine in the mid-nineteenth century (and the decline of euclidean mathematics) (Richards, 1979; Mayr, 1982). This development entailed a separation of the mathematical and the natural, which were narrowly linked in the Descartes-Leibniz tradition. The rise of naturalism is also related to evolutionary theory. The concept of evolution suggests that mental and intentional properties are not an autonomous realm, but serve as means of adaptation, contributing to survival of the organism in its environment. Furthermore, the continuity between lower animals and higher, including man, implies that mental properties could be seen as more complex, higher developed forms of the survival functions seen in other animals.

So, the evolutionary perspective entails a commitment to the unity of nature, and precludes sharp distinctions between mind and lower forms of adaptation. There is a continuous development from simple to complex forms of adaptation of the organism to its environment, and mind has its place at the upper end of this continuum.

Computation: Reduction or Elimination of Intentionality

In contemporary cognitive theories, two types of proposals to deal with the phenomenon of intentionality can be distinguished: elimination of folk psychology or reconstructing its concepts in computational terms. Folk psychology is the kind of psychology that uses intentional terms, such as beliefs and desires, as postulated underlying reasons for behavior, in the logical form of the practical syllogism (see von Wright, 1971). Whereas Fodor wants to take folk psychological explanations as point of departure for scientific psychology (and can, therefore, be criticized for not fully doing so), Churchland (1981, 1986) wants to replace them by purely scientific language.

The computational construal of these intentional terms is the propositional attitude: the mind consists of mental representations, a kind of sen-

tence in the head, and the organism has attitudes (beliefs, desires, fears, hopes) towards these propositions.

Functionalism identifies such mental states with logical states of a Turing machine (Putnam, 1980, 1981). The same logical state can be realized in many physically different machines (like a computer program running on different types of computers, or a mental process in different nervous systems). Thus, mental processes can be attributed to computers. This opens the way for an approach to mind which retains mentalist concepts from intentional psychology, but which is nevertheless scientific, although independent of neurophysiology (Fodor, 1981).

Elimination: Churchland. Functionalism is rejected by Churchland (1981) as a kind of smoke screen: functional states are ad-hoc and post-hoc descriptions, not independently verifiable, and, therefore, an expression of ignorance regarding the exact mechanisms that produce these states. Consequently concepts like computation and representation in psychology must be firmly grounded in, or better, reduced to, neuroscience. Churchland's (1986) attempt to do so suggests in my opinion that the claim of neurophysiological explanation is rather exaggerated (cf. Double, 1986). The neurophysiological approach does not qualify as an account of cognition; rather, Churchland's proposal boils down to abolishing the explananda of intentionality, rather than explaining them.

Computation: Fodor. Fodor (1975, Ch. 1, 1981) is critical of the reduction of intentional terms to neurophysiology, and wants to maintain functionalism. In his theory, there is a separate level of analysis for mental processes, the level of representation or computation.

Fodor's so-called Computational Theory of Mind (CTM) is something like the ideology behind cognitive science (see also Palmer & Kimchi, 1986). It can be summarized as follows: the workings of the mind are in a literal sense computations on symbols. Mental representations are symbol strings, and mental processes are manipulations on these symbols, according to formal, syntactical rules. Since symbol manipulation is precisely the computer's business, CTM entails what has been called strong AI: computers not only emulate, but actually *have* mental processes — although at least Fodor (1984) doubts whether simulation of central processes on computers will be possible in the near or far future.

Causal relations between mental representations must be described as derivations of the logical formulas. Since these formulas actually drive the physical machine through a succession of states, it seems that mental causation has at last gained scientific respectability: computation is a real, causal explanatory factor in behavior. CTM shows "how rationality is mechanically possible" (Fodor, 1987, p. 20).

On the other hand, mentalism is salvaged from reductionism: computation is autonomous relative to its material substrate Fodor (1981). Functionalism is a mechanistic, but not a materialistic theory (Putnam, 1980).

Fodor (1975) claims that computation (ideally) accounts for content and meaning of mental states. Intentionality can be reduced to internal, syntactically defined representations. According to Fodor (1980) only the syntactic properties of these formulas can be part of cognitive science. The principle of "methodological solipsism" bans all relations between organism and environment from psychology, although it is hoped that causal semantics (Fodor, 1987) might recover them some day.

It can be questioned whether the representational and computational account of intentionality and mental causation is fundamentally different from a mechanist conception of causation. According to Maloney (1985) CTM cannot specify what distinguishes computation from brute force, and the distinction between reasons and causes, action and movement (e.g., between an eyeblink and winking) is blurred in the CTM, and of course, this is detrimental to the claim that it accounts for intentionality.

Searle (1980) demonstrated that a person who emulates a computer program answering questions about a story by pushing uninterpreted symbol tokens around, cannot be said to understand the story, even if he gives the right answers. It follows then, that instantiations of a computer program do not really understand meaning. The conclusion from this simple and elegant litmus test must be that CTM does not account for intentionality.

It can be concluded that in the last analysis that Fodor's CTM is a kind of mechanical rationality, a form of syntactic reductionism. His treatment of intentionality is in this respect not much different from the reductionism of the one-plot story.

Naturalism: Gibson and Searle

Naturalism, mentalism and the organism-environment relation. Representational theories, and CTM in particular, can be classified as mentalist (Putnam, 1988), in the sense that they try to study mental processes without reference to the outside world. The alternative for this mentalism is, in Fodor's (1980) dichotomy, naturalism, which places organism-environment interactions at the forefront. Naturalism attempts to ground intentionality in the natural, real world that biological organisms live in. I will briefly discuss two models which seem to qualify as naturalist, namely, Gibson and Searle.

Gibson. Gibson (1979) was a sharp critic of representational theories in perception. In his view, organisms do not need internal representations and computations to perceive the world: they can pick up the information directly in the optic array and tune in to the world. This direct perception is a biological process of adaptation to the environment, of resonating to objec-

tive properties of the world, rather than having mental representations. In Gibson's system, organism and environment are reciprocal and interdefined: ecological psychology is concerned with laws that abstract over the whole organism-environment system. Also, the environment is directly perceived as intrinsically meaningful: Gibson's concept of affordances suggests that the world is perceived in terms of what action possibilities it affords to a given species, what can be done with it (Cutting, 1982; see also Reed, 1987). Thus Gibson redefines reality at a functional level of description, relative to the organism, independent of the description of classical physics. Meaning thus becomes an objective property of the environment.

So, Gibson analyzes perception without invoking mentalist concepts such as representations, sense data and computations; he starts with a naturalist organism-environment relationship. Just as perception is experiencing the things themselves rather than having representations, intentionality is being directed at the world rather than at intentional objects in the mind (Reed, 1983). Thus, the aboutness of intentionality is explained in biological evolutionary terms.

Searle. Searle (1980, 1981, 1983, 1984, 1987) provides another example of a theory of intentionality within an explicitly naturalist or biological framework. He considers intentionality as a real intrinsic feature of biological systems, and tries to make mentalism compatible with biology. Mental properties are in his view higher level properties of biological systems, which are not reducible to physiology. Therefore, the necessity of an intermediate level of representation or computation which Fodor assumed to salvage mental processes from neurophysiology is rejected.

In Searle's (1983) rather complex theory of intentionality, intentional states are characterized by their conditions of satisfaction. These relate the mind to the world, in the sense that the world may or may not match the content of a mental state. An interesting concept of intentional causation is implied by the notion of conditions of satisfaction: actions are caused by the intention to bring about a match between mind and world, and conversely perceptions are caused by things in the world. The intentional structure of perception and action is similar. Thus, intentional causation is an instance of real, ontological, efficient causation (Searle, 1983, p. 135). There is a directly experienced awareness of causality in perception and action which constitutes our most primitive form of causation. This implies an irreducible teleology, since an intentional state is both intrinsically goal-directed (directed to its conditions of satisfaction), and a property of a biological system.

An important consequence of the notion of conditions of satisfaction is that, according to Searle and in contrast with Fodor, mental states (i.e., representations) refer to the outside world rather than to "sentences in the head", and, therefore, there must be a kind of direct contact between mind and world. Searle's position that the content of intentional states refers directly

to the world seems in accordance with a realist naturalist ontology at large: mind relates the organism to the world. It seems also compatible with the Gibsonian outlook of intentionality (Reed, 1983), although Searle (1987) wants to maintain an internal level of representations which Gibson rejects.

A strong criticism of Searle's biological concept of intentionality is that he does not specify the real physiological mechanisms that produce intentional states. Palmer and Kimchi (1986, p. 71) state that there is "no scientific evidence to support Searle's materialist, biological view."

It must be admitted that his view that "the brain just does it" sounds rather naive. However, it seems arguable that Searle intends to emphasize in his biological concept of intentionality the functional aspect (pertaining to the organism-environment relation), and not the physiological aspect (pertaining to the physiological mechanisms and laws). If this is correct, then we should take a closer look at the ambiguity in the use of the terms 'natural' and 'biological'.

Naturalism Revisited: Functions Versus Mechanisms

William James is mentioned by Fodor (1980) as a typical example of a naturalist psychologist. In his own words, James (1890) "takes mind in the midst of all his concrete relations," unlike "rational psychology which treats the soul as a detached existent, sufficient unto itself." The emphasis on the environment in which the organism lives follows from the observation that the organism is primarily a biological entity. Psychology as the study of "the adjustment of inner to outer relations" must proceed "in relation to (the organism's) bodily existence." James' discussion of physiology makes it clear that he is not advocating applying the laws of physiology to psychology. His naturalism is not reductionist, but could be called functional: he is interested in the function of the brain, in terms of what it *does*, in relating the mind to the world. This is naturalism, not in the sense of extending the methods of physiology and physics to psychology, but in the sense of primacy of organism-environment relationship, and seems consistent with both Gibson and Searle.

So, within the concept of naturalism we should distinguish between mechanism and function. The functional aspect concerns the organism-environment relationship; this does not necessarily entail the mechanist aspect, which regards the causal laws and the physiological mechanisms. So, naturalism does not entail a reductionist, one-plot story about intentionality.

Conclusions

The notion of naturalism is ambiguous. A biological orientation, focusing on adaptive, functional organism-environment relations does not entail physical, mechanical or physiological reductionism. It is supposed that

Searle's use of the adjective 'biological' denotes mainly the level of function-
ing animals, not the physiological mechanisms producing intentionality.
Disting-uishing these seems important since it opens up the possibility of
being a naturalist without being a reductionist. The rejection of mentalism
does not necessarily lead to physicalism — see also Mayr (1982, Ch. 2) on the
differences between biology and physics.

Mentalism is a dead alley. Fodor's attempt to define a mental realm
autonomous relative to its physical substrate ends in a kind of syntactic
reductionism. Only the formal shadows of thought are left, and reasons and
intentions cannot be distinguished from causes. In this sense computation is
no less reductionistic and eliminative than materialism.

The dichotomy of mind and matter should be relativized. James' thesis
that the boundary-line of mind is vague deserves to be taken serious. The
Descartes-Brentano tradition sets out from an a priori dualism of mind and
matter, which are mutually exclusive, and jealously defends the borders of the
mental empire against physicalist attacks. Alternatively one might consider
mentality as a graded phenomenon, some bodily events being more mental
than others. An obvious example are psychosomatic events, for example,
electrodermal and cardiovascular reactions to psychological stressors, as used
in lie detection. A discipline called Cognitive Psychophysiology (Donchin,
Karis, Bashore, Coles, & Gratton, 1986) is concerned with the reflection of
information processing in brain potentials. Ben-Zeev (1986) proposed a
prototype theory which holds that psychological events are more or less
similar to purely mental or purely physiological processes, to replace the
dichotomy of mental and physical.

Intentionality is stratified. Shotter's (1975) description of three levels
in psychology (mechanic, organic and personal) can be applied to the concept
of intentionality. Organism-environment relations may be thought of as
ordered according to such levels. At the lowest mechanic level there are
simple sensori-motor co-ordinations such as reflexes, which are largely sub-
ject to causal laws. The next, functional or organismic level is described by
Gibson's ecological psychology, where the organism is still directly coupled to
the world, and its actions are governed by direct perceptions of affordances.
Later semi-autonomous, internal, cognitive schemata develop, leading to
more complex and stable interactions, in relative independence of the outside
world. The higher personalist levels of intentionality can be thought to
develop from organismic and mechanistic levels, superseding, integrating and
organizing, but not replacing them. Piaget's (1967) biological epistemology,
reaching from invertebrate behavior to logical reasoning in humans is an
example of the way these notions could be fleshed out. Piaget shows how a
kind of intentional superstructure can be built on perception-action cou-
plings or sensori-motor schemata.

So, a thermostat may be said to possess an elementary, mechanist kind of intentionality at the first mechanic level: it has a representation of a desired temperature and intends to realize that. The case of computers (Boden) can be considered as a more complex case of such a mechanicist intentionality.

Gibson's ecological psychology might be an adequate description of the organismic level of intentionality (cf. Reed, 1983). However, it underdetermines the personal, rational level: it provides no sufficient account of intentionality as rationality, knowledge and thinking.

A multi-plot story. This idea of levels of intentionality transcends the simple dichotomy of one-plot and two-plot story, mentalism and reductionism. On the one hand mentality is salvaged (Searle, 1984), on the other hand intentionality is firmly grounded in the "adjustment of inner to outer relations" (James). There is no sharp boundary between mind and body, since the mind is considered the upper level of a hierarchy of increasingly complex adaptive functions of the body. In this view psychology is a natural science insofar as it operates at the functional (naturalist) level, but not in the sense that it uses the methods of physics.

This implies that from a methodological point of view the choice is not between one-plot or two-plot stories; on the contrary, equally valid different stories corresponding to the different levels of complexity are possible in psychology, and no commitment to reduce one story to another is needed (Sanders & Van Rappard, 1985). For example, replacing mental terms by neurophysiological language (Churchland) is not a viable option (although physiology can reveal the subservient structures of functional adaptive behavior), and ecological psychology has no satisfactory account of higher mental processes like thinking.

The naturalist view contends that the starting point (or level) for psychology should be biological or ecological, doing justice to man's natural roots. Mentalist concepts like representation and computation can be appropriate at the next higher level, that of internal cognitive processes. The latter are considered as emergent properties of complex biological adaptive systems (Piaget, 1967).

References

Ben-Zeev, A. (1986). Making mental properties more natural. *The Monist,* *69,* 434-446.

Boden, M. A. (1972). *Purposive explanation in psychology.* Hassocks: Harvester.

Boden, M. A. (1981). Intentionality and physical systems. In M. A. Boden (Ed.), *Minds and Mechanisms: philosophical psychology and computational models* (pp. 52-70). Hassocks: Harvester.

Boer, Th. de (1978). *The development of Husserl's thought.* The Hague: Martinus Nijhof.

Churchland, P. M. (1981). Eliminative materialism and the propositional attitudes. *Journal of Philosophy, 78,* 67-90.

Churchland, P. M. (1986). Some reductive strategies in cognitive neurobiology. *Mind, 95,* 279-309.

Cutting, J. E. (1982). Two ecological perspectives: Gibson vs. Shaw and Turvey. *American Journal of Psychology, 95,* 199-222.

Danto, A. (1967). Naturalism. In P. Edwards, (Ed.), *The Encyclopedia of Philosophy.* New York: McMillan.

Dennett, D. (1978). Intentional systems. In D. Dennett (Ed.), *Brainstorms* (pp. 3-22). Hassocks: Harvester.

Dennett, D. (1987). *The intentional stance.* Cambridge: MIT Press.

Donchin, E., Karis, D., Bashore, T.R., Coles, M. G. H., & Gratton, G. (1986). Cognitive psychophysiology and human information processing. In M. G. H. Coles, E. Donchin, & S. W. Porges (Eds.), *Psychophysiology: Systems, Processes, and Applications* (pp. 244-267). Amsterdam: Elsevier.

Double, R. (1986). On the very idea of eliminating the intentional. Journal for the Theory of Social Behavior, 16, 209-216.

Fodor, J. A. (1975). *The language of thought.* Cambridge: Harvard University Press.

Fodor, J. A. (1980). Methodological solipsism considered as a research strategy in cognitive psychology. *Behavioral and Brain Sciences, 3,* 63-109.

Fodor, J. A. (1981). The mind-body problem. *Scientific American, 244,* 1, 124-132.

Fodor, J. A. (1984). *The modularity of mind.* Cambridge: MIT Press.

Fodor, J. A. (1987). *Psychosemantics.* Cambridge: MIT Press.

Gibson, J. J. (1979). *The ecological approach to visual perception.* Boston: Houghton Mifflin.

James, W. (1890, repr. 1950). *The Principles of Psychology.* New York: Dover.

Maloney, J. C. (1985). Methodological solipsism reconsidered as a research strategy in cognitive psychology. *Philosophy of Science, 52,* 451-469.

Mayr, E. (1982). *The growth of biological thought.* Cambridge: Belknap.

Newell, A. (1980). Physical symbol systems. *Cognitive Science, 4,* 135-183.

Palmer, S. E., & Kimchi, R. (1986). The information processing appproach to cognition. In T. J. Knapp & L. C. Robertson (Eds.), *Approaches to cognition: Contrasts and controversies* (pp. 37-78). Hillsdale: Erlbaum.

Piaget, J. (1967). *Biologie et Connaissance.* Paris: Presses Universitaires de France.

Putnam, H. (1980). The nature of mental states. In N. Block (Ed.), *Readings in the philosophy of psychology* (Vol. 1) (pp. 221-231). Cambridge: Harvard University Press.

Putnam, H. (1981). Reductionism and the nature of psychology. In J. Haugeland (Ed.), *Mind design* (pp. 205-219). Montgomery: Bradford.

Putnam, H. (1988). *Representation and reality*. Cambridge: MIT Press.

Reed, E. S. (1983). Two theories of the intentionality of perceiving. Synthese, 54, 85-94.

Reed, E. S. (1987). James Gibson's ecological approach to cognition. In A. Costall & A. Still (Eds.), *Cognitive psychology in question* (pp. 90-114). New York: St. Martin's Press.

Richards, J. L. (1979). The reception of a mathematical theory: non-euclidean geometry in England 1868-1883. In B. Barnes & S. Shapin (Eds.), *Natural Order: historic studies of scientific culture* (pp. 143-166). Beverly Hills: Sage.

Rorty, R. (1982). Contemporary philosophy of mind. *Synthese, 53,* 323-348.

Sanders, C., & Rappard, H. V. (1985). Psychology and philosophy of science. In K. B. Madsen & L. P. Mos (Eds.), *Annals of Theoretical Psychology* (Vol. 3) (pp. 219-268). New York and London: Plenum Press.

Searle, J. R. (1980). Minds, brains and programs. *Behavioral and Brain Sciences, 3,* 417-457.

Searle, J. R. (1981). Intentionality and method. *Journal of Philosophy, 78,* 720-733.

Searle, J. R. (1983). *Intentionality*. Cambridge: Cambridge University Press.

Searle, J. R. (1984). *Minds, Brains and Science*. Cambridge: Harvard University Press.

Searle, J. R. (1987). Minds and brains without programs. In C. Blakemoore & S. Greenfield (Eds.), *Mindwaves: thoughts on intelligence, identity and consciousness* (pp. 209-233). Oxford: Blackwell.

Shotter, J. (1975). *Images of man in psychological research*. London: Methuen.

Wright, G. H. von (1971). *Explanation and understanding*. London: Routledge.

A HERMENEUTICAL ANALYSIS OF THE SOCIAL-PSYCHOLOGICAL

Casey P. Boodt and Leendert P. Mos

SUMMARY: This paper is a speculative attempt to understand the nature of the 'psychological' from the perspective of the 'social'. Following in the tradition of Wundt's *Völkerpsychologie*, we borrow the notion of 'social character' from Erich Fromm, and the notion of 'conventionality', as constitutive of the 'social order', from F. A. Hayek, to arrive at a social understanding of the psychological. From within this social perspective of the psychological, a hermeneutical conception of human science is proposed that interprets the social-psychological as a dialogical relationship between individual-psychological and social-cultural 'prejudices'.

Introduction: Two Founding Psychologies

The founding of psychology by Wilhelm Wundt was really the founding of two distinct disciplines of psychology. First, Wundt founded an experimental psychology, based on the methods of the biological sciences and, hence, which he termed 'physiological psychology'. Physiological psychology was the study of the relationship between physical stimulation at the sense receptors and conscious apperception — the relationship between body and mind along the lines of classical psychophysics. Initially, Wundt and, especially E. B. Titchener, gave this new psychology a structuralist orientation and, later, under the influence of evolutionary theory, positivist philosophy, and behaviorism, it became physicalist and functionalist in its programs.

In recent years, as a result of a re-emergence of interest in the psychology of language, psychologists have taken a renewed interest in Wundt's original writings on the founding of psychology. For example, Arthur Blumenthal's (1970) translation of passages from Wundt's *Völkerpsychologie* led psychologists to recognize Boring's one-sided history of Wundt's contribution to the founding of scientific psychology. In Wundt's ten-volume *Völkerpsychologie* written between 1900 and 1920 (Boring, 1957, p. 326), he clearly proposed another psychology, one which is empirical but not experimental. Here Wundt, who was also an authority in linguistics, accepts the 19th century philologists' premise that peoples' 'higher mental processes' can only be understood through their languages and cultures (Baker & Mos, 1984). *Völkerpsychologie,* ethnic or social psychology, was the study of languages, customs, and myths or, what Boring (1957), almost disparagingly, calls the "natural history of man" (p. 326). Indeed, Wundt's *Völkerpsychologie* has been wholly ignored by a discipline that fully embraced his experimental

'physiological psychology'. However, Wundt, recognizing the limitations of the latter, wrote the following:

> The results of ethnic psychology constitute, at the same time, our chief source of information regarding the general psychology of the complex mental processes. In this way, experimental and ethnic psychology form the two principle departments of scientific psychology at large (cited in Dennis, 1948, p. 250).

Nevertheless, it was the new physiological psychology that quickly came to include the study of *inter*-personal behavior namely, experimental social psychology. While Roger Brown (1965) correctly noted that social psychology was not "... a simple 'extrapolation to the social level' of principles developed in general experimental psychology" (p. xix), nevertheless, mainstream experimental social psychology was and remains methodologically faithful to the individualistic character of Wundt's physiological psychology. Social psychologists, as Deutsch and Krauss (1965) wrote, are "... interested in studying the *conditions* that lead a person to conform to another's judgment ... that determine a person's attitudes ... that lead to cooperative or competitive interrelations," as well as "... the *effects* of an individual's attitudes on his relations with others, the consequences of competitive and cooperative relations, and similar relationships" (p. 3). Nor did the "cognitivization of social psychology and the computerization of the cognitive" (Graumann & Sommer, 1984, p. 67) alter experimental social psychologists' adherence to a justificationist epistemology and the ideals of actuarial prediction and control. What has become only too evident in post-positivist critiques of the lack of progress in experimental social psychology (e.g. Kenneth Gergen, Ari Kruglanski), is that social psychologists, committed as they are to methodological individualism, inductivism, and objectivism, have contributed little to our understanding of "... history and society, person and environment, individual and group, and action and interaction ..." (Graumann & Sommer, 1984, p. 67). What Wundt clearly understood to be the limitations of his physiological psychology is nowhere more evident than in our failure to understand the nature of the social-psychological.

What was characteristic about Wundt's physiological psychology, in stark contrast to his *Völkerpsychologie*, was that it excluded meaning and understanding from psychology (Hörman, 1979). Of course, the exclusion of meaning from physiological psychology seemed only appropriate when it came to the study of sensations, but when, for example, Hermann Ebbinghaus studied memory using nonsense syllables, he set the stage for what was to follow for the next eighty years. For if Ebbinghaus sought to exclude meaning from his functional approach to memory, the behaviorists sought to banish meaning, and mind, altogether from psychology and this included social psychology. Ironically, the recent resurgence of mentalism in social psychology has done little to effect any change in our understanding of the social-psychological. Cognitive social psychology in its search for *intra*-personal

invariant social-psychological processes was inevitably confronted by the historical and cultural boundedness of its postulated structures and explanations. It is precisely because our higher mental processes are historically and culturally rooted, that Wundt believed these were not accessible to experimental investigation. Access to them was only in terms of an understanding, interpretation and explication, of language, myth, society, and culture, or, the natural history of man, and not, as Wundt clearly recognized, a physiological psychology.

The implications of Wundt's two psychologies are even more forceful than they might at first appear. Wundt questioned the very possibility of a scientific, experimental, ethnic — *Völkerpsychologie* — psychology. And it is here that we wish to now shift the focus of discussion to another instance, one which is more recent, of where the *scientific* study of a mode of human inquiry has come under attack. But if this case is perhaps no less controversial, its resolution also points the way to a social psychology along the lines envisioned by Wundt's *Völkerpsychologie*.

The Social and Psychological

Meaning and the 'Psychological'. Psychoanalytic theory, in spite of its prevailing intellectual influence, has always had an ambiguous status within the discipline of psychology. Even prior to Karl Popper's (1963/1933) dismissal of psychoanalysis as a *scientific* theory, psychologists in the behaviorist tradition were mistrustful of an enterprise which failed to meet the criteria of testability and operationalization. Thus if, as psychoanalysis argued, our understanding of such human phenomena as meaning and understanding are tied to the therapeutic context and not amenable to scientific investigation then, the behaviorists argued, so much the worse for meaning and understanding.

It was Charles Taylor (1973), among others, who argued that psychoanalysis can never, in principle, meet the requirements of a natural science model of inquiry. The natural science model demands objectivity on the basis of 'hard' data which can be quantified and, hence, about which there is intersubjective agreement. Taylor refers here to "brute data," that is, "data which are available without any personal discernment or interpretation on the part of the observer" (Taylor, 1973, p. 56). But human actions and experience, Taylor emphasizes, can never be defined as brute data because "... they are always of necessity in terms of thoughts, images, intentions, and ways of seeing of the people concerned" (p. 59). The data that serve as the basis for the psychoanalyst's statements about repression, transference, and so forth, cannot be conceived of within a natural science model of inquiry, but are always given within the context of meaning and understanding. There is no way, writes Taylor, "of finessing this level of interpretation and observing the forces

(repression, etc.) outside of its medium" (p. 63). The medium is language and, in case of psychoanalysis, the dialogical language of the therapeutic context.

Presumably, in psychoanalysis, and in all the sciences which deal with human actions, emotions, intentions, social relations, and institutions, the investigator is put in a position of having to communicate with the 'objects' of investigation. Outside this dialectical framework of meaning and understanding, questions about the correctness or value of the ideas being considered can never be posed or answered. Taylor concludes that for any of the sciences of human behavior, insofar as they deal with more than the body, the natural science model of inquiry is inappropriate as, we hasten to add, Wundt recognized a half-century earlier when he founded *Völkerpsychologie*.

But if the human sciences do not fit the category of nomological, justificationist, science and, if we nevertheless maintain that they do have something essential and worthwhile to say about human life, what alternative conception of inquiry is appropriate? In approaching an answer to this question, it is our contention that, as a preliminary step, we must understand the nature of the social-psychological. To that end we propose to examine two concepts, 'social character' and 'conventionality', as constitutive of the social, which we deem exemplary in an attempt to situate the individual in the social-cultural context.

'Social Character' and the 'Social-Psychological'. Erich Fromm in his efforts to integrate psychoanalysis and Marxist humanism directly poses the question of how to conceive of a "dynamic, critical, socially oriented psychology" (Fromm, 1966, p. 231). In his answer to this question, Fromm postulates a concept of 'social character'. Rejecting Freud's libido theory, which he conceives of as mechanistic and materialistic, Fromm (1955) views the person as essentially social from birth: "The archimedic point of the specifically human dynamism lies in this uniqueness of the human situation: the understanding of man's psyche must be based on the analysis of man's needs stemming from the conditions of his existence" (p. 32). Thus, instead of allowing character to be defined biologically, as Freud did in following his stages of psychosexual development, Fromm (1966) assumes, that character, social character, is the result of the "practice of life as it is constituted by the mode of production and the resulting social stratification." Or, more generally, that "... social character refers to the structure of psychic energy as it is molded by any given society so as to be useful for the functioning of that particular society" (p. 231). Consistent with more recent object-relations theories derived from psychoanalysis, Fromm, as early as 1941, used the concept of social character as a social-psychological integrating, or bridging, concept to refer to the "passionate striving towards others and nature — a striving to relate to the world" (Fromm, 1966, p. 234).

The concept of social character, reflecting the material and ideological conditions of human existence, serves as a bridging concept between the social

and psychological. The individual and world are co-constituted in the concept of social character. What Fromm accomplishes with his concept of social character is to fuse Freud's distinction between the biological-psychological and social-cultural aspects of personality: there is no pure biology, or psychology; the biological, and psychological, are always socially and culturally expressed and, hence, meaningful. However, what is meaningful, need not be conscious, transparent, or understandable; it may be simply lived. Therefore, the concept of social character, and all such similar bridging concepts, are insufficiently articulate with regard to the distinctions between what is, on the one hand, biological-psychological and social-cultural and, on the other hand, conscious and unconscious in human personality. Both distinctions cut deeply into the concept of social character, for it is only on the conception of social character as partly individual and partly social, partly conscious and partly unconscious, that we can come to speak of, for example, the 'psychopathology' of normal life or, of neurosis as a moral failure. But the difficulty is of how to conceive of social character dynamically, in terms of both biological-psychological and social-cultural, and conscious and unconscious, dimensions. To understand this, we turn to hermeneutical philosophy.

Hermeneutics of 'Social Character'. Hermeneutical philosophy, according to Gadamer (1977), asserts that reality is lingual; that it is, in principle, accessible to our understanding, either as potential knowledge or actualized, lived, viewpoints. It is language which, as a dimension of immediate experience, precedes every particular experience and every act of consciousness. The break between the subjective and objective is, on the hermeneutical account, one between what is understood and what as yet remains to be understood; it is a dialectical process of fusions of subjective horizons of meaning moving towards objective and universally valid knowledge in the context of a commitment to truth which characterizes every real dialogue. Reality is always as it appears in our prejudices, pre-judgments, or pre-understandings which, in turn, are always rooted in tradition, within a linguistic-cultural community, having intersubjective validity. The totality of our legitimated, social-cultural, prejudices are constitutive of reality. Genuine understanding is the attentive and critical participation in, and the development of, one's effective-historical consciousness of those prejudices (Lindseth, 1986, pp. 139-140).

Following Habermas' (1968) hermeneutical analysis of psychoanalysis, and contra Gadamer, Lindseth (1986) distinguishes between individual-psychological prejudices which characterize our "personal-effective" history and legitimated, social-cultural, prejudices which make up the effective-historical reality of our communal existence. Moreover, Lindseth extends Gadamer's concept of prejudice to include not only our understanding but also our behavior and conduct: we not merely have our prejudices, "We live out of them, they serve as the foundation upon which we stand, when we encounter the situations and tasks of life" (Lindseth, 1986, p. 82). This dual

nature of prejudice as well as the distinction between individual-psychological and legitimated, social-cultural, prejudices are encompassed in Fromm's concept of social character. Therefore, the concept of social character is not restricted to the social-psychological but also includes the cultural-historical as these legitimated prejudices are embodied in, and lived by, the individual.

In Habermas' hermeneutical language, psychoanalysis deals with those problematic, spontaneous experiences and behavioral inclinations, which constitute our individual-psychological prejudices. The human tragedy, according to psychoanalysis, is that we tend to distort, falsify, and rationalize, our largely unconscious, spontaneously lived behavior and experience, so as to mediate between the latter, lived individual-psychological prejudices, and those historically legitimated, social-cultural, prejudices normative in our understanding and conduct. Thus, according to Freud, our spontaneously lived behavior, the driving forces of the id and superego, as well as our rationalizations, intellectualizations, and self-deceptions, which are our pre-understandings, our individual-psychological prejudices, of that lived behavior, are largely unconscious. Presumably, a person seeks psychoanalysis because, on the psychoanalytic account, there is a division, discrepancy, or split between conscious, but mistaken, pre-understandings of unconscious, or incomprehensible, impulses, fantasies, and anxieties which constitute our personal-effective history, and those legitimated prejudices which constitute our social-cultural tradition. It was Freud's genius to recognize that psychoanalytical theory attains its validity in the dialogical context of human prejudices, perhaps, best exemplified in the therapeutic dialogue.

The connection between psychoanalytical theory and psychoanalytical practice is such that the theory is constructed on the basis of, and finds its validity in, the therapeutic dialogue between patient and analyst. The meta-psychological categories, hypotheses, and interpretations can only be made explicit, or instantiated, within the therapeutic dialogue. Habermas, (1968) conceives of this dialogue as a "fusion of horizons of meaning" between patient and analyst: between the spontaneous, individual-psychological, prejudices as these are problematically lived by the patient, and the legitimated, socialcultural, prejudices of psychoanalytic theory held by the analyst. There is, however, as Lindseth (1986) points out, a concurrent second dialogue, an inner dialogue, between the patient's spontaneously lived and, hence, largely unconscious, individual-psychological, prejudices, and conscious and reflective, legitimated, social-cultural, prejudices. Part of the success of this inner dialogue is, of course, the facilitating role played by the analyst in educating the patient in the business of psychoanalysis. The patient must come to understand his problematic, spontaneously lived experience and behavior, his individual-psychological prejudices, in terms of those legitimated prejudices of psychoanalytic theory.

On Gadamer's (1977) hermeneutical philosophy, our interpretation of Fromm's concept of social character in terms of the dual nature of prejudice and the distinction between individual-psychological and legitimated, social-cultural, prejudices is rejected. Purely individual-psychological prejudices are incomprehensible as their meaning and our understanding of them is always embedded within a historical-cultural-linguistic context as Wundt understood so well. However, if our individual-psychological prejudices never possess a 'pure nature', then our understanding of them, using whatever modes of inquiry from nomological science to hermeneutics, comes only by way of those legitimated prejudices that are historically articulated in, and reflective of, our social-cultural context. Therefore, when we earlier spoke of an inner dialogue between the patient's individual-psychological prejudices, the patient's mistaken pre-understandings of his unconscious anxieties, moods, impulses, compulsions, and behavioral tendencies, we were referring to a dialogue between conscious legitimated, social-cultural, prejudices and unconscious archaic-mythological dimensions of *meaning*, neither of which, however, can be thought of outside the dialogical context of patient and analyst and, the wider dialectical-cultural context which embeds them both. Dialogue is first of all interpersonal and only, secondarily, intrapersonal. Our prejudices are always simultaneously individual-psychological and legitimated, socialcultural. Yet individuals give expression to, and live the practices normative of, their culture in characteristic ways. If Freud deemed our individual-psychological prejudices to be largely unconscious, it remains puzzling how our legitimated, social-cultural, prejudices may also be largely unconscious or, better, continuously open to further interpretation and explication. To understand this, we now turn to the concept of 'conventionality' as constitutive of the 'social order'.

The Social-Psychological

'Conventionality' as Constitutive of the 'Social Order'. The history of society, according to Friedrich Hayek (1973), consists in the accumulation of human practices and behaviors which are its conventions. Hayek refers to this accumulation of human practices and behaviors as a "social order" which arises from our initial state of standing in relationship and coming together as individuals in a society. If the formation of social order is inescapable, it is also indispensable as "... we depend for the effective pursuit of our aims clearly on the correspondence of the expectations concerning the actions of others on which our plans are based with what they will really do" (Hayek, 1973, p. 36). Conventions arise then from the implicit realization, the lived, but not fully undertstood, individual-psychological prejudices, that in order to participate in, aspire to, achieve, or master, individuals must coordinate their actions such that these will ultimately lead to the attainment of their individual and common needs. Indeed, Hayek points out, that "... social

theory begins with — and has an object only beause of — the discovery that there exists orderly structures which are the product of the action of many men but are not the result of human design" (p. 37). In other words, that society and social order are prior to individual rationality.

Hayek's claim implies that in the history of human kind, individuals at some time or another co-operate so as to attain certain goals. This may appear to be an obvious conclusion as our modern society is extensively characterized by its cooperative nature. Nevertheless, coming together out of choice or contract is very different from having to come together out of necessity presumably, in the first instance, the necessity of survival. The former conception implies that human beings are rational and that society evolved as a result of their communal, if individual, rational planning. That is, the result of individual human beings using their higher mental processes to work towards a common goal in order to achieve their individual needs, desires, and wants. But while convention may indeed be the result of individual's rational choices, rationally planning their communal activities, convention may also result from individuals each blindly pursuing their own ends. No individual rationally and consciously designed language or politics, religion or economics, science or jurisprudence, and a host of other social structures which, we have not so much structured, as them us. These social orders are, for the most part, the results of unintended consequences of our actions and not the products of rational design. Hayek argues that the manner in which individuals pursue their own ends leads, in practice, to collaboration and, eventually, to a social order. Thus, from our individual pursuits and joint actions, there results many and varied unintended consequences which come to constitute a social order. Just as for the hermeneuticist, "being is greater than consciousness," — reality always far exceeds our individual meanings, or prejudcies — just so, for Hayek, the social order is far more expansive than our individual intentions, actions, and goals.

Hayek (1973) makes a distinction between social orders created by forces outside, "exogenously generated," and social orders created or emerging from within, "endogenously generated," matrices of interacting individuals, all pursuing their own ends, whose individual actions have unintended consequences which gives rise to the conventions of a social order. An exogenously generated social order is a 'made' order, a "... construction, an artificial order," or *taxis*, while an endogenously generated social order is a 'grown' order, one that is "... self-generating" and, "... is in English most conveniently described as a spontaneous order" (p. 37), or *cosmos*. A 'made' order, or taxis, is a rationally constructed and controlled social or institutional order directed towards explicit ends or goals, and consisting of explicit rules, regulations, and conventions, which are commonly thought to be hierarchically structured, stipulating members' practices and behaviors.

In contrast to a taxis order, a cosmos conception of social order is one which evolves spontaneously and is not the product of our rational design but, instead, consists of both intended and unintended consequences of our individual behaviors and conduct. Thus, in the endogenously generated social order of language, speakers of a particular language acquire and use the language according to convention, but no individual understands either the full complexity of their language, or, indeed, the history or consequences of their individual use of the language. From the perspective of human rationality, language is the highest achievement of our adaptation to the world, yet from the standpoint of communication, it constitutes a social order which is a matter of convention and not rational agreement. Therefore, cosmos orders, such as language, are genuinely social-psychological in nature by relying, on the one hand, on individual motivation in reason and action and, by recognizing, on the other hand, individuals' inevitable and indispensible ignorance of particulars and, hence, their having to conform to general principles embodied in conventions. Individuals did not create social orders, they evolved along with these orders in a complex interplay of individual reason, social practice, including language, and social structure. All reason can ever accomplish is to modify selected aspects of social orders while these remain largely intact and tacit, carrying the conventions of tradition through the practices of its individual members.

Cosmos social orders can be rational although no single individual is in control and no positive goals are being sought. Language, for example, is based on the abstract but regulative concept of communication; it is language which enables its speakers to intentionally communicate to the extent that it is possible on the basis of particular utterances belonging to the language to determine whether communication has occurred. Thus language is an institutionalized 'system' of expectations or conventions which enables individual speakers of the language to act consistently according to the conventions or rules that characterize the language. Clearly, the rules of language are not fixed in all their particularity, nor does any one speaker of the language know those rules. Individual speakers participate in the language and, indeed, must participate according to the conventions or rules that characterize the language, yet the language system is itself a matter of public utility, enabling communication. It is the latter, abstract notion of communication, regulative of our use of language, which constrains our individual reason and forces us to rely on convention. The rules of language are not the conclusions of our individual higher mental processes and, even though we constantly attempt to improve our use of linguistic conventions in order to more effectively communicate, as individuals we are inevitably ignorant of all the consequences of these efforts. Submission to the 'anonymous' system of language is not merely mandatory so that we might communicate, which serves our individual and common interest, it is also indispensible in the growth and regulation of our individual rationality.

Much remains to be said about the formation and function of social orders, suffice it to note here that as individuals we often go about our lives believing that the social orders in which we participate are 'made', or taxis orders, that are controlled either through individual reason, or by means of the actions of a community of rational individuals. However, as Hayek points out, we habitually engage in "anthropomorphic thought" when it comes to the 'social' and are always led to the conclusion that order exists because of a *reason(able)* design, a man-made design (p. 36). In other words, we assume that our individual-psychological prejudices in following convention are always conscious, transparent, and explicit. This attitude, or habit of thought, if you will, of 'anthropomorphizing' the social order (on the psychoanalytic account, a form of self-deception), is exemplified in our individualistic conception of the social-psychological.

But spontaneously generated social orders, cosmos orders, are abstract and participation in these orders is largely in terms of following conventions. An abstract social order, as Hayek (1973) notes, "... need not manifest itself to our senses but may be based on purely *abstract* relations which we can only mentally reconstruct. And not having been *made*, it cannot legitimately be said to have a particualr purpose, although our awareness of its existence may be extremely important for our successful pursuit of a great variety of different purposes" (p. 38, emphasis added). This is one reason why a practical hermeneutical interpretation of human sciences inquiry suggests that it is not the goal to master but to participate in, or to emancipate us from, the social, or conventional, order. In other words, so that we may come to understand our individual-psychological prejudices, including our preunderstandings of the conventions by which we learn to live, in the light of legitimated, social-cultural, prejudices which characterize our understanding of, and participation in, social orders. Indeed, while we encourage individuals, primarily through education, to understand their conformity to legitimated, social-cultural, prejudices, or conventions, this is, in principle, an interminable affair, one that may wax and wane over successive generations and must be renewed again and again by each generation. For, every advance in our understanding yields a new, abstract, social order, with its own tacit conventions, and its own unintended consequences which, in turn, demand a new understanding and a re-newed understanding of the past.

Hermeneutical Human Science

Human science, as a *theoretical* hermeneutical enterprise, never attains final understanding. It only allows us to push back the 'horizons' of understanding and renew our participation in social orders. Nor is our task merely to understand those legitimated prejudices which characterize the conventions of the social orders in which we participate but, in doing so, we are inevitably engaged in a *practical* hermeneutical endeavor in learning to under-

stand our participation - our individual-pscyhological prejudices - in the light of our understanding of those legitimated prejudices. In following convention, we are living — individual-pscyhological prejudices — those legitimated, social-cultural, prejudices which have stood the test of time. Our personal-effective history must, then, always reflect the historical-effective prejudices that characterize the social orders of our collective traditions. These latter prejudices are counted as the 'nature' of our society and culture. Through the formative role of the legitimated prejudices of nurture and education, our individual-psychological prejudices are fused in the concept of social character. It is the manner in which we live and give expression to individual-psychological prejudices which constitutes our individuality and the subject matter of *practical*, hermeneutical human science, whereas it is our understanding of, and subsequent participation in, legitimated, social-cultural, prejudices which are the subject matter of *theoretical*, hermeneutical human science. Self-knowledge and knowledge of the self are dialectically expressed in the concept of social character.

On our hermeneutical conception, the social-psychological is to be understood in terms of a dialogical relationship between our individual-psychological prejudices which characterize our personal-effective history and, hence, out of which we live, and our legitimated, social-cultural, prejudices which constitute the effective-history of our collective reality. In this relationship, understanding and participation are the achievements of *theoretical* and *practical*, hermeneutical, human science, one that is in the tradition of an ethnic or social, *Völkerpsychologie*, psychology of Wilhelm Wundt.

References

Boring, E. G. (1957). *A history of experimental psychology.* (2nd ed.). New York: Appleton-Century-Crofts.

Baker, Wm J., & Mos, L. P. (1984). Mentalism and language in (and out of) psychology. *Journal of Psycholinguistic Research, 12,* 397-406.

Blumenthal, A. L. (1970). *Language and psychology: Historical aspects of psycholinguistics.* New York: Wiley.

Brown, R. (1965). *Social psychology.* New York: The Free Press.

Dennis, W. (1948). (Ed.). *Readings in the history of experimental Psychology.* New York: Appleton-Century-Crofts.

Deutsch, M., & Krauss, R. M. (1965). *Theories in social psychology.* New York: Basic Books.

Fromm, E. (1955). *The sane society.* Greenwich, CT: Fawcett Publications Inc.

Fromm, E. (1966). The application of humanist psychoanalysis to Marx's theory. In E. Fromm (Ed.), *Socialist humanism,* (pp. 228-245). New York: Doubleday & Co.

Gadamer, H. G. (1977). *Psychological hermeneutics.* (D. E. Linge, Trans.). Berkeley, CA: University of California Press.

Graumann, C.F., & Sommer, M. (1984). Schema and inference: models in cognitive social psychology. In J. R. Royce & L. P. Mos (Eds.), Annals of theoretical psychology (Vol. 1), (pp. 31-76). New York: Plenum.

Habermas, J. (1968). *Knowledge and human interests.* Boston: Beacon Press.

Hayek, F. A. von (1973). *Law, legislation and liberty: rules and order. (Vol. 1).* Chicago: University of Chicago Press.

Hörman, H. (1979). *Psycholinguistics* (2nd ed.). New York: SpringerVerlag.

Lindseth, A. (1986). Establishing a metascientific foundation for psychoanalysis. In L. P. Mos (Ed.), *Annals of theoretical psychology. (Vol. 4),* (pp. 59-98, 133-156). New York: Plenum.

Popper, K. (1963/1933). *Conjectures and refutations: the growth of scientific knowledge.* New York: Basic Books.

Taylor, C. (1973). Peaceful coexistence in psychology. *Social Research, 40,* 55-82.

'DOING THEORY' IN PSYCHOLOGY: FEMINIST RE-ACTIONS

Mary Gergen

SUMMARY: Feminist theorists have developed a range of alternative approaches to scientific work in the last three decades. The application of these ideas would have strong implications for change in psychology. Especially vulnerable would be the notion of 'objective' and value-neutral scientific inquiry. A feminist psychology would require value explicit theorizing. The focus of change would be on discourse systems, which are the producers of meaning in science and in society. In addition, the creation of new theories in which individuals as origins of behavior would be replaced by situationally dependent relational units is recommended. Lastly subjects of study would be encouraged to participate in the making of theory, and be users of the outcomes of theoretical work.

Feminist Theory and Social Epistemology

The second wave of feminism beginning in the early 1960's has been characterized by an interest in reinterpreting the politics of culture. In particular, feminists have questioned the basic suppositions concerning the nature of reality as propounded by various cultural groups, especially scientific ones, (cf. Gergen, 1988a; Harding & Hintikka, 1983; Keller, 1985). Rather than simply (or not so simply) striving for equality within social institutions, many feminists became engaged in actively deconstructing the modes by which those in the established order maintain control. Here, I am concerned with critiquing the existing modes of theory construction in psychology and in the possibilities of reformulating psychological theories through a social constructionist epistemology, as informed by a feminist framework (cf. Roberts, 1981). The purpose of this endeavor is to encourage the development of forms of knowledge within psychology that support feminist values of emancipation, for men as well as women, and also to enhance productive and creative goals for the sciences.

Within the tradition, psychologists generally operate from some form of realist worldview, and employ empirical research methods for purposes of discovering this "objective" reality (cf. Zajonc, 1989). In contrast, most feminist metatheoriests eschew the notion of an objectively knowable reality and reject the traditional logical empiricist methods of theory building for which they were designed (cf. Gergen, 1988b; Hollway, 1989). Indeed, many feminists hold that reality is socially constructed (cf. Flax, 1987; Gergen, 1989; Kitzinger, 1987; Weedon, 1987). As a social constructionist, one rejects a realist worldview, a logical positivist philosophy of science, and empiricist

methodology — that is, the possibility of discovering an objective, morally or politically neutral, knowledge base. Rather, forms of knowledge, as incorporated into scientific theories, are defined and validated by a relevant community of consensus and open to reinterpretation and rebuttal within it (cf. K. Gergen, 1985, for a detailed explication of social constructionism in psychology).

In general, feminist theorists strongly support the contention that our scientific theories about people, including psychological theories, are patriarchal in their structures, contents, and applications (cf. Fausto-Sterling, 1985; Hare-Mustin & Maracek, 1988; Hubbard, 1988; Keller, 1985; Martin, 1987). The relevant scientific communities that have legitimated these theories have been male-dominated, and most likely unaware of the male-superiority position they uphold. Applied to psychology, the feminist view implies that existing theories generally reinforce hierarchical social arrangements, support stereotypic male attributes as the human norm, and assume the male person as the standard subject of inquiry (Vogel, Broverman, Clarkson, & Rosenkrantz, 1972). The 'female' person, on the other hand, is most often delegated to the role of the divergent or alternative model of the human species — the Other — a designation applied by Simone de Beauvoir (1953). Feminist theorists attempt to criticize and question this androcentric balance, often through critical analysis of psychological theory and practice (see, e.g., Hare-Mustin & Maracek, 1988, who discuss the ways in which mental health practices have been deleterious to women, as well as men).

With this brief introduction to a feminist perspective on the patriarchal nature of scientific endeavors, let us look more closely at three ways in which a feminist-social constructionism could alter psychological theorizing. Specifically, I shall consider forms of inquiry that expose the a priori valuational implications that have affected traditional theory development, recognize the ways in which forms of discourse limit the boundaries of theory, and employ theory for emancipatory goals of enhancing personal relationships as well as scientific practices.

Feminist Theory: A Question of Values

Central to feminist influences on psychological theorizing is the issue of the a priori value implications that shape theory construction. Feminists recognize that theoretical constructions are inevitably evaluative. This supposition annuls the strong claims of scientists to value neutrality and thus to objectivity (Gergen, 1988b; Longino, 1988). The feminist approach has been to revel in the recognition of values, and to urge that values become instrumental in shaping theories (Reinharz, 1985). Although there are exceptions, (cf. Daly, 1978), generally the value choices made by feminists are not exclusionary. Feminist values support the well-being of women, but not at the expense of men. As Emily Grosholz (1988) has said, "The feminist

revolution is different from all others in that we live with our opponents (men) and generally speaking we love them." (p. 180).

Feminism breaks down as a unified theoretical discipline especially when it comes to socio-political values. All shades of the political spectrum can find representation among feminists, including Marxism (cf. Delphy, 1980; Hartsock, 1983; Smith, 1974); however, most feminists are wary of the patriarchal values and practices that have characterized all political groups run by men. Thus, identification with any party's aims is generally equivocal. Despite differences, the values usually implicit in feminist theorizing are those that stress the quality of relationships among people. Additionally, feminist theorizing usually indicates preferences for co-operative activities, empathic understandings, communal involvement and a rejection of aggression, oppression, competition, domination, physical force, and violence.

A focus on values encourages analyses of existing theories in order to expose their underlying value assumptions (which are most often aspects of male privilege), and the development of theories that support alternative social values. Such new theories subvert androcentered values and the related modes of operating in psychology and in the society more generally. In this sense theories based on feminist values are change-oriented, and thus, revolutionary in potential. An example of the revolutionizing tendencies produced by feminist value theorizing is Carol Gilligan's (1982) work on the 'ethics of responsibility and caring'. This work undercuts the traditional, masculine value that morality can only be preserved through a codified and formal set of principles, which can be relied upon in settlements of claims of right and wrong (Kohlberg, 1981). Gilligan's approach evades the strict reliance on moral codes, and instead advocates moral choices based upon feelings of love and responsibility, in addition to considerations of moral rules.

Feminist Theory as Discourse

The major mode by which value-saturated change occurs in scientific theorizing is through altering the discourse forms within it (Kitzinger, 1987; Potter & Wetherell, 1987; Wilkinson, 1986). A major concern of feminist theorists is the patriarchal language forms by which we all communicate. Whether words, phrases, metaphors, analogies, or narrative lines — all linguistic structures used in the development of theoretical discourse in psychology guide us in our everyday behavior (cf. Daly, 1978; Gergen & Gergen, 1986, 1988; Morawski, 1988; Murray, 1986; Sarbin, 1986; Spender, 1980). Feminist scholarship requires vigilant attention to the threats posed by using patriarchal language forms. Yet, language skills are developed without a high degree of self-reflexivity concerning the gender biases implicit within them. Without sensitivity to the engendered coding of our theoretical terms, we are likely to reproduce the existing patriarchal systems, even as we try to change them (M. Gergen, in press). (For example, if I write 'feminist

theory should oppose the existing androcentric ideas, explode their premises, destroy their credibility, and annihilate their support', I would be promulgating discourse that uses a 'war' metaphor, which is stereotypically masculine rhetoric, and usually repugnant to feminist values.) A feminist approach to psycho-logical theorizing should promote discourse that is radically transformational, without utilizing the traditional 'I win-you lose' formula necessitated by hierarchical power relations.

A more detailed example is useful in demonstrating how a change in discourse practices concerning the unity of study might be executed. The foundational unit of analysis in psychology has been the single individual. This unit, the person, is usually inbued with internal qualities, including beliefs, attitudes, values, emotional responses, attributional styles, coping strategies, schemas, scripts, and/or personality traits. Thus, the individual is presumed to act as an independent entity, and almost always for some narrowly defined notion of self-gain (Henriques, Hollway, Urwin, Venn, & Walkerdine, 1984; Scheman, 1983). The optimal individual is internally controlled, field independent, achievement-oriented, and rational. Within the traditional theoretical framework, relationships are defined by their individual 'self' components. The group decomposes into single units when description and explanation are attempted.

A feminist revision of the unit of analysis in psychology opens up exciting new options for theorizing. Instead of the unit of study being the independent individual, psychologists are invited to formulate theories in which the unit of analysis is 'relational'. By this I mean that the unit would include more than one person, and could change in its constituency depending on contextual and thus relational changes. A mother-child unit is one example of a relational unit; a couple, a bowling team, or people caught together in a stuck elevator are also exemplars. Relational theories would take seriously the intimately entwined nature of historically situated persons, in relationship with each other. The goal of psychologists would be to fashion theoretical understandings out of relational units that would not be translatable or reducible to individual terms, but rather have an integrity as such. Kenneth Gergen and I have applied relational theorizing to the study of emotional expressions. We have been able to show, for example, that emotional expressions are embedded within broader units of cross-time interchange between persons. Emotional expressions derive their meaning and legitimacy from the manner in which they are embedded in these relational scenarios. Thus anger is not defined in terms of the private feelings of one person, but as a socially developed activity between two or more people (Gergen & Gergen, 1988). Certain types of systems theory, especially in clinical psychology, also emphasize relational units as critical to understanding dysphoric and disfunctional behavioral patterns (Berg & Smith, 1985). In order to understand the disfunction, therapists treat the family as a whole, while avoiding a focus on any family member as the cause or the victim of the disfunction.

From a social constructionist position, relational theories are not recommended because individualist theories are false, unparsimonious, illogical or vague, the traditional guides to judging the worth of theories. Rather relational theories are valuable because they encourage the creation of new language patterns, which have implications for other life forms. These new patterns of discourse encourage new patterns of action that might be more congenial to the values of a non-patriarchal culture, as well. For example, hierarchical organizations, favored within patriarchal systems, have strong propensities to assess blame and responsibility for actions. Supportive of this tendency, attribution theory researchers have often studied 'errors' people make in designating the causes of outcomes. These theorists try to predict and explain when and why people make mistakes about who is to blame for something (Jones & Nisbett, 1971). Were we to use a relational unit of analysis, problems typically regarded as assigning blame to one source or another would be recoded into other discourse forms. Individual blame would become an empty term. Rather than viewing social behavior as the outcome of individual intention, choice and action, whole new understandings of how patterns of behaving occur would result. Actions would be seen as emergent outcomes of ongoing sequences of interaction. With a relational perspective, organizations would be required to create their evaluation processes with new conceptual tools. The opportunities for new language forms, new theoretical relationships, new collections of data, new understandings and new social applications through relationally based theorizing seem immense.

Feminist Theory and Emancipatory Goals

Traditionally, theories have been produced by the culturally elite, for the benefit of others of the same station (Hubbard, 1988). Typically, the subjects of experiments designed to discover new psychological understandings never benefit from the outcomes of the study, and are regarded by the experimentalists as merely the cannon fodder in the war against ignorance. Additionally, the development of the study — theory, method, analysis, and conclusions — is done in isolation from the subject population. At best, subjects are debriefed; the lies they have been told are untold.

Various feminist inspired research projects have gone beyond the customary limits demarking the scientist and the subject. Efforts have been made to bring those who are being studied into collaborative relationships with those who wish to study (cf. Belenky, Clinchy, Goldberger, & Tarule, 1986; Gergen, 1989; Lather, 1986). The developing ideal is that research should be a mutual endeavor, for the benefit of those who are involved as subjects, as well as for others. Openness, honesty, mutual trust, and respect for the knowledge of those who are not professional psychologists should characterize the interchange. The costs of deception and exploitation become

regarded as too high to be worth the loss of trust and communal spirit. Through the use of dialogic methods, in which new understandings can emerge as a consequence of shared communications, alterations in research plans can occur (cf. Mishler, 1986).

Lastly, the application of the theory is an integral part of the research enterprise. Not only might the work of the theorist have immediate implications for those involved, but the theories may themselves be emancipatory. This means that the opportunity to be involved in the theoretical project changes the nature of the subjects' personal status: their sense of who they are; what they might become; what or who might lend them support in their struggles. The feminist theoretician keeps in mind that theories are used for planning, assessing and justifying action. Thus, care must be taken to envision the consequences of the theory for people's lives. For example, Mary Belenky (1988) is developing a community center in an isolated, poverty-strickened town where women who have lead lonely, self-effacing existences are coming together, as part of the research enterprise, but are also changing their ways of regarding themselves and their life potentials as an outgrowth of the study. In this sense the theoretical becomes the practical, and lives are improved through participation in the research.

In the final analysis, we must be self-reflexive about our relationships to our scientific communities and to our wider communities, and to the values that we are maintaining in the social circles of which we are a part. As psychologists we are part of a network that is responsible for the theories we create, the languages we authorize, the values we uphold, and the applications we encourage. We cannot hold that we are objective in the face of our creations. Our theories implicate ourselves.

References

Beauvoir, S. de (1953). *The Second Sex*. New York: Knopf. (Originally published in French, 1949).

Belenky, M. (1988). Personal communication.

Belenky, M., Clinchy, B. M., Goldberger, N. R., & Tarule, J. M. (1986). *Women's ways of knowing*. New York: Basic Books.

Berg, D., & Smith, K. (Eds.). (1985). *Exploring clinical methods for social research*. Beverly Hills, CA: Sage.

Daly, M. (1978). *Gyn/ecology: The metaethics of radical feminism*. Boston, MA: Beacon Press.

Delphy, C. (1980). A materialist feminism is possible. *Feminist Review, 4*, 79-105.

Fausto-Sterling, A. (1985). *Myths of gender: Biological theories about women and men*. New York: Basic Books.

Flax, J. (1987). Postmodernism and gender relations in feminist theory. *Signs, 12*, 621-643.

Gergen, K. J. (1985). The social constructionist movement in modern psychology. *American Psychologist, 40*, 266-275.

Gergen, K. J., & Gergen, M. M. (1986). Narrative form and the construction of psychological science. In T. R. Sarbin (Ed.), *Narrative psychology: The storied nature of human conduct* (pp. 22-43). New York: Praeger.

Gergen, K. J., & Gergen, M. M. (1988). Narrative and the self as relationship. In L. Berkowtiz (Ed.), *Advances in experimental social psychology* (pp. 17-56). San Diego: Academic Press.

Gergen, M. M. (Ed.) (1988a). *Feminist thought and the structure of knowledge.* New York: New York University Press.

Gergen, M. M. (1988b). Toward a feminist metatheory and methodology in the social sciences. In M. Gergen (Ed.), *Feminist thought and the structure of knowledge* (pp. 87-104). New York: New York University Press.

Gergen, M. M. (1989). Talking about menopause: A dialogic analysis. In L. E. Thomas (Ed.), *Research on adulthood and aging: The human sciences approach* (pp. 65-87). Albany: SUNY Press.

Gergen, M. M. (in press). Life stories: Pieces of a dream. In G. Rosenwald & R. Ochberg (Eds.), *Telling lives.* New Haven: Yale University Press.

Gilligan, C. (1982). *In a different voice: Psychological theory and women's development.* Cambridge: Harvard University Press.

Grosholz, E. (1988). Women, history and practical deliberation. In M. Gergen (Ed.), *Feminist thought and the structure of knowledge* (pp. 173-181). New York: New York University Press.

Harding, S., & Hintikka, M. (Eds.), (1983). *Discovering reality. Feminist perspectives on epistemology, metaphysics, method and philosophy of science.* Dordrecht: D. Reidel.

Hare-Mustin, R., & Maracek, J. (1988). The meaning of difference, Gender theory, postmodernism, and psychology. *American Psychologist, 43*, 455-464.

Hartsock, N. (1983). The feminist standpoint: Developing the ground for a specifically feminist historical materialism. In S. Harding & M. Hintikka (Eds.), *Discovering reality. Feminist perspectives on epistemology, metaphysics, methodology, and philosophy of science* (pp. 182-210). Dordrecht: D. Reidel.

Henriques, J., Hollway, W., Urwin, C., Venn, C., & Walkerdine, V. (Eds.) (1984). *Changing the subject: Psychology, social regulation, and subjectivity.* London: Metheun.

Hollway, W. (1989). *Subjectivity and method in psychology.* London: Sage.

Hubbard, R. (1988). Some thoughts about the masculinity of the natural sciences. In M. Gergen (Ed.), *Feminist thought and the structure of knowledge* (pp. 1-15). New York: New York University Press.

Jones, E. E., & Nisbett, R. (1971). *The actor and the observer: Divergent perceptions of the causes of behavior.* Morristown, NJ: Silver Burdett/General Learning Press.

Keller, E. F. (1985). *Reflections on gender and science.* New Haven: Yale University Press.

Kitzinger, C. (1987). *The social construction of Lesbianism.* London: Sage.

Kohlberg, L. (1981). *The philosophy of moral development, vol. 1.* New York: Harper & Row.

Lather, P. (1986). Research as praxis. *Harvard Educational Review, 56,* 257-277.

Longino, H. (1988). Science, objectivity, and feminist values (A review essay). *Feminist Studies, 14,* 561-574.

Martin, E. (1987). *The woman in the body, A cultural analysis of reproduction.* Boston: Beacon Press.

Mishler, E. G. (1986). *Research interviewing, context and narrative.* Cambridge, MA: Harvard University Press.

Morawski, J. (1988). Impasse in feminist thought? In M. Gergen (Ed.), *Feminist thought and the structure of knowledge* (pp. 182-194). New York: New York University Press.

Murray, K. (1986). In T. R. Sarbin (Ed.), *Narrative psychology: The storied nature of human conduct* (pp. 186-201). New York: Praeger.

Potter, J., & Wetherell, M. (1987). *Discourse and social psychology, Beyond attitudes and behaviour.* London: Sage.

Reinharz, S. (1985). Feminist distrust. Problems of context and content in sociological work. In D. Berg & K. Smith (Eds.), *Exploring clinical methods for social research* (pp. 153-172). Beverly Hills, CA: Sage.

Roberts, H. (Ed). (1981). *Doing feminist research.* London: Routledge.

Sarbin, T. R. (1986). *Narrative psychology: The storied nature of human conduct.* New York: Praeger.

Scheman, N. (1983). Individualism and the objects of psychology. In S. Harding & M. Hintikka (Eds.), *Discovering reality* (pp. 211-244). Dordrecht, Holland: D. Reidel.

Smith, D. (1974). Women's perspective as a radical critique of sociology. *Sociological Inquiry, 44,* 7-13.

Spender, D. (1980). *Man made language.* London: Routledge Kegan Paul.

Vogel, S. R., Broverman, D. M., Clarkson, F. E., & Rosenkrantz, P. S. (1972). Sex-role stereotypes: A current appraisal. *Journal of Social Issues, 28,* 59-78.

Weedon, C. (1987). *Feminist practice and poststructuralist theory.* Oxford: Basil Blackwell.

Wilkinson, S. (Ed.) (1986). *Feminist social psychology. Developing theory and practice.* Milton Keynes: Open University Press.

Zajonc, R. (1989). Styles of explanation in social psychology. *European Journal of Social Psychology, 19*, 345-368.

MÜNCHHAUSEN-OBJECTIVITY:
A BOOTSTRAP-CONCEPTION OF OBJECTIVITY
AS A METHODOLOGICAL NORM

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SUMMARY: There is a proliferation of different conceptions of scientific objectivity. Most of these conceptions are related to certain approaches in science in general or in psychology in particular, such as: the empirical-analytical approach, the interpretative-hermeneutical approach, and the critical-dialectical approach. Because of the one-sidedness or the lack of clarity of these conceptions some philosophers and scientists dropped objectivity as an inherent attribute of science altogether. In this article I will present the Münchhausen-conception of objectivity as a methodological norm. Using this conception, objectivity can be restored as an inherent attribute of the scientific enterprise; furthermore, objectivity can be seen as epistemologically relatively neutral. In addition, attention will be paid to the relation of Münchhausen-objectivity to such methodological norms as reliability and validity.

Mainstream Psychology

The empirical-analytical approach is a dominant stream in psychology and in the social sciences in general. Within this empirical-analytical approach a way of acting or a research product is called methodologically objective when it is regimented (i.e., standardized, instrumented, algo-rithmitized, automatized, formalized), intersubjective (i.e., consensual, intra- and interobserver consistent, intersubjectively testable, intersubjectively controllable, intersubjectively criticizable), unbiased (i.e., free from prejudices and independent of racial, cultural, educational and sexual differences between researchers), or value free (i.e., free from value judgments with regard to the observational or theoretical object-language). (See e.g., Brunswik, 1955; de Groot, 1969; Guthrie, 1959; Hull, 1943; Kerlinger, 1970; Midgaard, 1977; Myrdal, 1970; Popper, 1934, 1962, 1966; Stevens, 1935a & b; Straus, 1958; Weber, 1922; Zener & Gaffron, 1962.)

Some possible objections against these conceptions of methodological objectivity are:

- what is being accentuated is avoidance of distortions and independence of individual or subjective differences of the

researchers; hence, there is little attention for 'letting the object speak';[1]

- the subjectivity of the researcher is seen, mainly, as a possible distorting factor;

- most of the time objectivity is regarded as a characteristic of ways of acting (i.e., research methods) and products of these ways of acting and seldom as a characteristic of a personal attitude or mental activity of the researcher;

- if one is talking about an objective attitude of mind it is only in the sense of aloofness, detachment and distanciation; the ideal seems to be to replace the researcher by a robot;

- both the subject (the scientist as a person) and the object of investigation tend to be of merely marginal importance;

- the use of the term objective in these senses is misleading as well as superfluous; misleading, because this term suggests a surplus-meaning which is not intended; superfluous, because one can use more accurate terms like standardized, consensual, intersubjectively controllable, value free, and so forth.

Alternative Approaches

There are different meanings or conceptions of methodological objectivity in other approaches in human sciences, such as (existential-) phenomenological, humanistic, hermeneutical, and critical-dialectical approaches. Some of these conceptions are: phenomenological objectivity (fidelity to the phenomenon of which the world of experience of the investigated subject forms the main part; see Colaizzi, 1978), experiential objectivity or caring objectivity (characterized as non-interfering, indwelling, and Taoïstic; see Maslow, 1966), value objectivity (this is realizable by a researcher when his value-system is well developed so that it can function as an antenna-system for understanding another person's behavior; see Krimerman, 1969), dialogical or hermeneutical objectivity (in this kind of objectivity the interaction between researcher and respondent is essential; see

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[1] The expression 'letting the object speak' sometimes means 'emphasizing the importance of the actor's point of view', but in cases where the object is not conceived or pre-understood as a person it has a metaphorical meaning. This is, for instance, the case in Keller's conception of 'dynamic objectivity' (see Keller, 1985). The biophycisist Keller is talking about Barbara McClintock, a Nobel prize winner for physiology and medicine, when she (Keller) says that objectivity means a listening to the object, a full turning towards the object, a complete focusing of all the perceiver's perceptual and experiential faculties on the object, so that it is experienced in the fullest possible way. Notice that, although we are talking about natural science here, Keller's conception of objectivity is not a typical empirical-analytical one.

Bleicher, 1982), objectivity of love (this existential-phenomenological kind of objectivity is based on a loving relationship between researcher and subject; it is 'der Objektivität der Liebe' [see Binswanger, 1942; see also Buytendijk, 1947]), and emancipatory objectivity (the attitude whereby the research recognizes that striving towards autonomy is essential for the human being; see e.g., Spiecker, 1973; Coenen, 1987; see for a marxist variety of this emancipatory objectivity Moser, 1975; Schneider, 1980).

Some features of these conceptions (which may be criticized from an empirical-analytical point of view) are:

- 'letting the object speak' is accentuated; in the social sciences this may mean recognizing the actor's point of view (see note 1);

- personal experience of the researcher is not just seen as a possible threat to objectivity, but as an instrument: objectivity is an intelligent learned use of our subjectivity, not an escape from it;

- objectivity is mainly seen as a characteristic of a style or attitude of mind (or a mental activity) and is, as such, not so much seen as a detached and impartial attitude, but as an attitude that rests upon involvement and purity of interest;

- the use of the term objective in these alternative meanings is not customary.

A Descriptive Mapping Sentence

The first step towards a solution of the sketched conceptual problem regarding objectivity is an all-embracing descriptive analysis of all methodological meanings of objectivity (cf. Smaling, 1987). Within the methodological context objectivity seems to be an attribute of something. Hence, our purpose will be met by analyzing the expression 'X is objective'. X refers to an element of the domain D, the set of all phenomena which are said to be objective or not. D appears to consist of three subdomains. Subdomain D_1 concerns personal psychic attitudes (or individual mental activities) of the researcher, such as an objective attitude of mind. Subdomain D_2 concerns ways of acting, such as methods and techniques which are called objective because of their regimentation, objective procedures (intersubjectively controllable or based on a rational consensus), and objective acts of research (based on dialogical relationships between the researcher, his colleagues, and the investigated actors). Finally, subdomain D_3 concerns results or products of the activities of the researcher, such as objective data,

objective observations, objective concepts, objective propositions, objective reports, and objective interpretations.[2]

It appears that 'X is objective' has two core meanings. First, a positive core meaning (R_1): X lets the object speak. This positive dimension of 'X is objective', this object-relationship, may be called the epistemic dimension.[3] Secondly, there is a negative core meaning (R_2): X does not distort the object. Thus, objectivity is not an attribute of an element of the domain but rather a relation (R) between an element of the domain and an object of study.[4]

We shall not elaborate on the nature of the object of psychology. It will suffice here to state that the object of study (O) has three aspects: outwardly observable, non-purposeful behavior (O_1), outwardly observable acts (O_2), and internal psychic states and mental processes or activities (O_3).

It appears, moreover, that on the methodological level certain conditions are, explicitly or implicitly, assumed to exist. We differentiate between research problems, research questions, and research goals on the one hand (G) and frames of reference (F) on the other hand. These frames of reference concern background-knowledge and apriori-beliefs about the object of study and ways of studying it. Examples are philosophical, ideological, meta-theoretical, paradigmatic, substantively theoretical, traditional, or common sense frames of reference. The conditions G and F are not to be taken in an absolute sense. Sometimes it will be necessary for the researcher to criticize (aspects of) G or F. Such criticism may occur when the scientist takes on the role of the philosopher of science.

To summarize we construct the following mapping sentence[5]:

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[2] A researcher, an investigator, an observer is called objective because of his or her objective attitude of mind (D_1), ways of acting (D_2) or products (D_3). Hence, there is no need to introduce a fourth subdomain for persons.

[3] The expression 'epistemic' is used rather than 'epistemological' because of the methodological context. Epistemological issues are disregarded because they are of a more philosophical nature. As our discussion proceeds the epistemological points of view will be considered.

[4] A possible objection against our differentiation between the two mentioned core meanings could be that they are logically dependent. Is it possible to let the object speak and yet distort it? Is it possible not to distort the object without letting the object speak? This objection seems logically correct, but does not hold in the present context. On the methodological level accentuating the negative core meaning leads to conceptions of objectivity which do not necessarily imply that the object can speak at all, for instance a machine- or robot-ideal of objectivity. Conversely, accentuating the positive core meaning, for instance, caring objectivity and value-objectivity, does not necessarily prevent all sorts of distortion. Moreover, a distanciating attitude of mind (negative core meaning) does not easily combine with an involving attitude of mind (positive core meaning). Therefore, our differentiation between the two core meanings stands.

[5] For the idea of a mapping sentence see Guttman's facet-analytical approach to conceptual problems (Guttman, 1957).

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(an attitude (D₁)   )                          (behavior (O₁)            )
(                   )  (lets speak (R₁)    )   (                         )
(a way of acting (D₂))  (                    ) (acting (O₂)              )
(                   )  (does not distort (R₂))  (                        )
(a product (D₃)     )                          (psychic phenomena (O₃))
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                  (a question         )   (a frame of   )
                  (                   )   (             )
with regard to    (a problem or goal )  and  (reference    )
                  (                   )   (             )
                  (G₁ through Gₙ      )   (F₁ through Fₘ)
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A short notation for this mapping sentence is: 'DRO|G.F' (D has the relation R with O given the conditions G and F; the vertical line indicates that the conditions are not absolute: (aspects of) G and F can, occasionally, be questioned).

It has to be pointed out that the five facets (D,R,O,G and F) of the mapping sentence can be read disjunctively. For instance, an author may use the term 'objectivity' as follows: objectivity is an attribute of a special product, namely a proposition (D); a proposition is called objective if it is free from value judgments (R); the psychological object is outwardly observable behavior (O); the research question is which of two behavioral therapies is the better one (G), and the frame of reference is a positivistic philosophy of science (F). Another author may use the term 'objectivity' in a different way: objectivity is an attribute of scientific procedures (D); a procedure is said to be objective if it is intersubjectively controllable as a criterion for freedom of bias (R); the object of study concerns mental processes (O); the research problem is how to evaluate an educational program (G), and the frame of reference is the cognitivistic approach in psychology (F). A third author may conceive objectivity as an attribute of a mental activity (D); a mental activity is said to be objective if it lets the object speak by role-taking (R); the object of study is the perspective of the acting other (O); the research goal is to build a grounded theory (G), and the frame of reference is the symbolic interactionistic approach in social science (F).

The Münchhausen-Conception

The next step is the conceptualization of objectivity as a methodological norm (cf. Smaling, 1987, 1988). For this purpose I propose the Münchhausen-conception (or, if you wish, the bootstrap-conception). The Baron Von Münchhausen lived in the eighteenth century and was famous because of his fantastic stories and his incredible adventures all over the world. One of his adventures is especially relevant in this case: the story goes

that when he was trapped in a swamp he rescued himself by pulling at his wig with his hand and thus extracting himself from the swamp. Two aspects of the Münchhausen-metaphor are important: the network-character (think of the several adventures all over the world) and the contrafactual regulative aspect (think of the paradoxical salvation).

The network-character implies that the researcher in his striving for objectivity combines as many aspects as possible, both between the facets D, R, O, G, F, and within these facets. In other words, the facets must not so much be read disjunctively as conjunctively. For instance, one has to avoid mistakes as well as to let the object speak; we need both in a dialectic and dynamic balance. We call this combination of the two core meanings 'doing justice to the object'[6]. According to the Münchhausen-conception one can only do justice to the object if one pays attention to each of the three subdomains: attitudes, ways of acting, and products. The Münchhausen-conception of objectivity as a methodological norm does not belong to a particular approach. From a methodological point of view one is free to choose a specific frame of reference F. Furthermore, Münchhausen-objectivity offers ample opportunity for an integration of various meta-theoretical points of view. For instance: different meta-theoretical standpoints correspond with different preconceptions of the object, with different images of man. Such images are: human beings as machines, robots or organisms, human beings as texts, documents, or works of art, human beings as puppets on the string of historical, societal, or economic forces, but also human beings as symbolic interactors with others within a particular form of life or as creators of machines, robots, books, societies. Each of these images has its strength but also its weakness, its one-sidedness. To neutralize the weaknesses and to fortify the strengths we have to combine the mentioned images in a dialectic and dynamic way. Moreover, Münchhausen-objectivity is relatively neutral with respect to diverse epistemological positions such as realism, idealism, constructivism, relativism, and so forth. For instance: if a clinical psychologist wants to determine whether a 50 year old man with coronary heart disease shows the so-called Type-A behavior pattern or not, he wants to do justice to this man as a clinical psychologist, but that is not to say that he is a realist in an epistemological or ontological sense; possibly he is even, as a philosopher, a relativist.

The Münchhausen-conception implies that objectivity is a contrafactual regulative principle. Regulative, because objectivity is a goal to be aimed at and because this principle has a rough regulating function. Contrafactual, because objectivity is never reached in an absolute sense and because it is necessary to act, against the facts, as if objectivity is realized to some degree

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[6] The essence of the meaning of objectivity within a methodological context is doing justice to the object of study. This applies to all sciences. Even for the natural scientist it is true that he must not distort the object as well as letting the object speak. See especially note 1.

in order to approximate it. Objectivity as a contrafactual regulative principle has as an important consequence that every attempt to reach objectivity may be questioned: is this, after all, objective?

The Münchhausen-conception is a safeguard against degeneration of the quest for objectivity. The Münchhausen-conception operates by counter-balancing one-sided conceptions of objectivity such as the machine- or robot-ideal of objectivity, and, experiential or caring objectivity.

The Münchhausen-Tetralemma

The conditions of research questions, problems, goals, and frames of reference must, as has already been said, not be taken in an absolute sense. Occasionally the scientist may examine these conditions. He may question the preconception or fore-understanding of the object of study which pertains to a particular frame of reference. What really is the object of study? But then the question arises when has he done justice to this object? How can the scientist know when he has done justice to the object? There are four possible fallacies: a logical circle (e.g., when you say a method is objective because it does justice to the object and you say that this method does justice to the object because it is objective), or an infinite regress (e.g., when you say a method is objective because of the objectivity of something else that is somehow linked with that method, etc.), or an arbitrary stop or dogmatism (e.g., when you say results or data are objective arbitrarily or purely by convention), or objectivity without an object (e.g., when you try to avoid the three mentioned fallacies by defining objectivity only in terms of stand-ardization, intersubjective agreement, or internal psychic states).

The Münchhausen-way of handling this tetralemma is not to choose one of the four fallacies but to strive for a dynamic and dialectic balance between all of them. This Münchhausen—tetralemma (i.e., this tetralemma with its Münchhausen-solution) forms an inherent part of the Münchhausen-conception of objectivity as a methodological norm.[7]

[7] The Münchhausen-solution of the tetralemma is, in a sense, Popperian. Popper (1934) presented a new solution for the so-called Fries-trilemma. The philosopher Fries (1773-1843) faced the problem of how to justify statements of science. He taught that, if the statements of science are not just to be accepted dogmatically, we must be able to justify them. A logical justification would imply justification of statements by statements and thus an infinite regress. Fries wanted to avoid the danger of dogmatism as well as an infinite regress and taught that statements could be justified by perceptual experience. Popper, however, rejected this position as a psychologistic one. Popper's solution of the Fries-trilemma, the choice between dogmatism, an infinite regress and psychologism, was not to choose one of them but to accept all three possibilities to a minimal degree by conceptualizing basic statements as empirical statements which are, by convention, not justified further for the time being. Albert (1968) reformulated this Fries-trilemma as follows: the choice is between dogmatism (including psychologism), an infinite regress and a logical circle. Albert renamed this trilemma the Münchhausen-trilemma and maintained the Popperian solution. Notice that our Münchhausen-tetralemma is not just a simple extension of Albert's Münchhausen-trilemma: our tetralemma does not concern basic statements but how to do justice to the object of study.

Reliability and Validity

The reliability-concept is used in a diversity of meanings. Reliability in the sense of freedom from random errors (see e.g., Aiken, 1985; APA, 1985; Walsh & Betz, 1985) is an aspect of the negative dimension of doing justice to the object, namely not distorting the object. Reliability in the sense of freedom from all sorts of errors (see Denzin, 1978) is almost equivalent to the negative dimension if we disregard the subdomain of mental attitudes and activities. Reliability in the sense of (virtual) repeatability, consistency, or stability (see e.g., Carmines & Zeller, 1979; Giorgi, 1988; Kirk & Miller, 1986; LeCompte & Goetz, 1982) is only a possible indicator for Münchhausen-objectivity because reliability in these meanings is neither a sufficient nor a necessary condition for doing justice to the object.

The validity-concept is also used in different meanings. Validity in the sense of freedom from systematic, non-random errors (cf. Carmines & Zeller, 1979) is an aspect of the negative dimension. Validity in the sense of freedom from both systematic and non-systematic errors see (e.g., Aiken, 1985; APA, 1985) is almost equivalent to the negative dimension if we disregard mental attitudes and activities. Other meanings of validity are the degree to which the (essence of the) intended phenomenon is studied and not something else, the degree to which the intended concept is instrumented and not another one, the degree to which results of investigation are defensible or generalizable (see e.g., APA, 1985; Cook & Campbell, 1979; Giorgi, 1988; Kirk & Miller, 1986; LeCompte & Goetz, 1982; Meerling, 1989; Walsh & Betz, 1985). Validity in these meanings is partly an aspect of the positive dimension and partly only a possible indicator for Münchhausen-objectivity. For instance, instrumentation of concepts or generalizability of results to other situations are neither sufficient nor necessary conditions for doing justice to the object.[8]

Reliability and validity concern the subdomains of ways of acting (D_2) and products (D_3), but Münchhausen-objectivity also includes the subdomain of the researcher's attitudes of mind and psychic activities (D_1). Moreover, Münchhausen-objectivity can be aimed at by other ways of acting (D_2) than (un)reliable and (un)valid procedures, methods or techniques. For instance: dialogical relationships with colleagues, key-informants and investigated subjects. Besides, Münchhausen-objectivity as a contrafactual regulative principle transcends norms as reliability and validity because one may, in principle, always ask: is this type of reliability or validity, with regard to a

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[8] In this short paper we do not elaborate reliability, validity and other issues at length, but see Smaling (1987).

certain research goal and a certain frame of reference, a real or important contribution to the quest for doing justice to the object of study at all?[9]

References

Aiken, L. R. (1985). *Psychological testing and assessment*. Boston: Allyn & Bacon.

Albert, H. (1968). *Trakät über kritische Unvernunft*. Tübingen: Mohr.

APA (1985). *Standards for educational and psychological testing*. Washington, D.C.: American Psychological Association.

Binswanger, L. (1942). *Grundformen und Erkenntnis Menschlichen Daseins*. Zürich: Neihaus.

Bleicher, J. (1982). *The hermeneutic imagination. Outline of a positive critique of scientism and sociology*. London: Routledge & Kegan Paul.

Brunswik, E. (1955). The conceptual framework of psychology. In O. Neurath, R. Carnap, & Ch. Morris (Eds.), *Foundations of the unity of science. Vol. I* (pp. 655-760). Chicago/London: The University of Chicago Press.

Buytendijk, F. J. J. (1947). *Het kennen van de innerlijkheid*. Nijmegen: Dekker & Van de Vegt.

Carmines, E. G., & Zeller, R. A. (1979). *Reliability and validity assessment*. Beverly Hills/London: Sage.

Coenen, H. (1987). *Handelingsonderzoek als exemplarisch leren*. Groningen: Konstapel.

Colaizzi, P. F. (1978). Psychological research as the phenomenologist views it. In R. S. Valk & M. King (Eds.), *Existential-phenomenological alternatives for psychology* (pp. 48-71). New York: Oxford University Press.

Cook, T. D., & Campbell, D. T. (1979). *Quasi-experimental design: design and analysis issues for field settings*. Chicago, IL: Rand McNally.

Denzin, N. K. (1978). *The research act: a theoretical introduction to sociological methods* (2nd ed.). New York: McGraw-Hill

Giorgi, A. (1988). Validity and reliability from a phenomenological perspective. In Wm J. Baker, L. P. Mos, H. V. Rappard, & H. J. Stam (Eds.), *Recent Trends in Theoretical Psychology* (pp. 167-176). New York: Springer-Verlag.

Groot, A. D. de (1969). *Methodology. Foundations of inference and research in the behavioral sciences*. Amsterdam: Mouton.

Guthrie, E. R. (1959). Association by contiguity. In S. Koch (Ed.), *Psychology: a study of a science, Vol. 2* (pp. 158-195). New York: McGraw-Hill.

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[9] Münchhausen-objectivity could be conceived as a meta-methodological norm rather than simply a methodological norm because it is a norm for methodological norms as reliability and validity.

Guttman, L. (1957). Introduction to facet design and analysis. *Proceedings of the Fifteenth International Congress of Psychology, Brussels* (pp. 130-132). Amsterdam: North-Holland Publishing Co.

Hull, C. L. (1943). *Principles of behavior.* New York: Appleton-Century-Crofts.

Keller, E. F. (1985). *Reflections on gender and science.* New Haven: Yale University Press.

Kerlinger, F. N. (1970). *Foundations of Behavioral Research.* London: Holt, Rinehart & Winston.

Kirk, J., & Miller, M. L. (1986). *Reliability and Validity in Qualitative Research.* Beverly Hills: Sage.

Krimerman, L. I. (Ed.) (1969). *The nature and scope of social science: a critical anthology.* New York: Appleton-Century-Crofts.

LeCompte, M. D., & Goetz, J. P. (1982). Problems of reliability and validity in ethnographic research. *Review of Educational Research, 52,* 31-60.

Maslow, A. H. (1966). *The Psychology of science. A reconnaissance.* Chicago: Henry Regnery Co.

Meerling, (1989). *Methoden en technieken van psychologisch onderzoek. Deel 1: Model, observatie en beslissing* (2nd ed.). Meppel/Amsterdam: Boom.

Midgaard, K. (1977). On the problem of objectivity in the social sciences with a particular view to the significance of situational logic. *Danish Yearbook of Philosophy, 14,* 127-139.

Moser, H. (1975). *Aktionsforschung als kritische Theorie der Sozialwissenschaften.* München: Kösel-Verlag.

Myrdal, G. (1970). *Objectivity in social research.* London: Gerald Duckworth.

Popper, K. R. (1934). *Logik der Forschung.* Wien: Springer (with the imprint '1935'). (First English translation: The logic of scientific discovery, 1959.)

Popper, K. R. (1962). Die Logik der Sozialwissenschaften. *Kölner Zeitschrift für Soziologie und Sozialpsychologie, 2,* 233-248. (Also in: Adorno et al., Der Positivismusstreit in der Deutschen Soziologie. Neuwied: Luchterhand, 1969.)

Popper, K. R. (1966). *The open society and its enemies. Vol. II: The high tide of prophecy: Hegel, Marx and the aftermath.* London: Routledge & Kegan Paul (First edition: 1945).

Schneider, U. (1980). *Sozialwissenschaftliche Methodenkrise und Handlungsforschung. Methodische Grundlagen der kritischen Psychologie 2.* Frankfurt: Campus Verlag.

Smaling, A. (1987). *Methodologische objectiviteit en kwalitatief onderzoek.* Lisse: Swets & Zeitlinger.

Smaling, A. (1988). Münchhausen-objectiviteit. Een nieuwe conceptie van objectiviteit als methodologische norm. *Psychologie en Maatschappij, 12*, 272-288.

Spiecker, B. (1973). Waardegebondenheid en objectiviteit van de opvoedingswetenschap. *Pedagogische Studiën, 50*, 329-338.

Stevens, S. S. (1935a). The operational basis of psychology. *American Journal of Psychology, 47*, 323-330.

Stevens, S. S. (1935b). The operational definition of psychological concepts. *Psychological Review, 42*, 517-527.

Straus, E. (1958). Objektivität. *Jahrbuch für Psychologie und Psychotherapie, 6*, Heft 1/3. (Also in: E. Straus (1960). Psychologie der menschlichen Welt. Gesammelte Schriften. Berlin: Springer Verlag, pp. 409-426.)

Walsh, W. B., & Betz, N. E. (1985). *Tests and Assessment.* Englewood Cliffs, NJ: Prentice Hall.

Weber, M. (1922). Die Objektivität sozialwissenschaftlicher und sozialpolitischer Erkenntniss (1904). In *Gesammelte Aufsätze zur Wissenschaftslehre* (pp.146-214). Tübingen: Mohr. (English translation in F. R. Dallmayr & T. A. McCarthy (Eds.) (1977). *Understanding and Social Inquiry* (pp. 24-37). University of Notre Dame Press.)

Zener, K., & Gaffron, M. (1962). Perceptual experience: an analysis of its relations to the external world through internal processings. In S. Koch (Ed.), *Psychology: A Study of a Science. Vol. 4* (pp. 515-618). New York: McGraw-Hill.

PSYCHOLOGY AND THE PROBLEM OF VERISIMILITUDE

Morris L. Shames

SUMMARY: Psychology — in the propaedeutic literature as well as in its more technical and theoretical tracts — has implicitly and, oftimes, explicitly embraced the empiricist and positivist perspectives. At the same time it has laid claim to scientific realism and, in consequence, developed a profound commitment — in the service of hypotheticalism — to operationism. Notwithstanding psychology's more recent movement away from logical positivism to a more liberalized logical empiricism it has nonetheless been recognized more recently that such a philosophy of science has become largely enervated. Even scientific realism has come under considerable criticism (Laudan, 1981). However, it has been argued that transcendental realism (Bhaskar, 1978) has yielded a virtual Copernican resolution in philosophy of science by providing a non-Kantian alternative to empiricism and rationalism both. This course of events has, for the most part, escaped psychology's notice. In addition, it has remained impervious to paradigms other than its own, in particular, constructionism, hermeneutic and critical theory, to name the most obvious. This neglect coupled with the ambiguous understanding of itself outlined above has led psychology to the embracement of an arthritized methodology which disqualifies other competing epistemologies out of hand and, in consequence, has generated very little coherent theory. More generally, its failure to disambiguate its fundamental postulates coupled with its methodological exclusivity has rendered it somewhat blind to its past and fairly aimless as to its future.

Introduction

Among all the sciences, psychology's commitment to what is the most arthritized form of hypotheticalism is not only the most steadfast (Doyle, 1965) but is thought to be owing to ideological practice to a large degree (Shames, 1987). However, blame for this perception must also be laid at the feet of historians of the discipline whose historiographical practices seem suspect, at best, and whose work seems derivative, at worst. Historiography always carries a good deal of risk such as 'whiggishness' and the difficulty of resisting revisionist tendencies in general, and psychological histories, feeding off themselves as they tend to have done, seem to have yielded to both temptations. Thus, it is from sources such as these that one gleans the impression that psychology is — and must continue to be, if its scientific status is to be assured — a 'positivist' or 'empiricist' discipline. But how, in actual fact, does psychology measure up to such epistemological claims?

The principal questions confronting psychology are: Is psychology — either by dint of praxis or pronouncement — a positivist, empiricist, or

operationist discipline as is variously claimed? Even more to the point, does psychology have a clear understanding of these terms, their history and their implications? More particularly, Watson's (1913) promulgation of his be-haviorist manifesto marks, for some, the coming of age of scientific psychol-ogy in light of his explicit embracement of an objectivist approach seemingly founded on the solid base of scientific realism. In light of this it is both fair and even compelling to ask whether psychology is indeed driven — *and understands this to be the case* — by the impulse imparted by scientific realism?

It is a virtual truism that a significant part of the armamentary of scientific realism is scientific method, most often interpreted as hypothetico-deductivism in psychology. This suggests that the method by means of which one investigates problems adumbrates the theoretical perspective one holds in respect of those problems. Watson's (1913) famous document, for example, adds considerable testimony to the view that theory — broadly considered — and method are inextricably linked. In fact, it is from works such as this and others (e.g. Hull, 1952) that the impression has been gleaned that the nature of psychology is such that it *must* be cast in a hypotheticalist mold. Not all, however, are of that view. More contemporaneously, for instance, it has been suggested that:

> ... to bring theoretical conclusions within the scope of operational amplia-
> tion theories no less articulate and forceful than what we have already
> attained for statistical reasoning, we need only to shake off the beguilement
> of hypothetico-deductivism and look with care at what, inferentially, prac-
> ticing scientists actually do. (Rozeboom, 1982, p. 646)

These sentiments, it should be noted, stem from a realist — not a construc-tionist or hermeneutic — theoretician.

All of this serves to point up the necessity to disambiguate these con-structs and postulates so critical to the psychological research project. It is, quite simply, intolerable that theoretical and metatheoretical considerations lag far behind praxis in psychology's pursuit of its scientific enterprise; else it may render itself scientistic (Koch, 1981) rather than scientific. It is for reasons such as this that realism, in particular, — the substrate of psychologi-cal epistemology — must be rendered lucid.

Realism and the Conduct of Research

It is to psychology's considerable discredit — epistemologically speaking — that it has failed to discern the lack of affinity between positivitism and realism and appears to have been driven either explicitly or implicitly by both (Mackenzie, 1977). However, psychology's alleged positivist roots and realist predilections merit some consideration in this critique. To begin with, the provenance of hypotheticalism in psychology can be traced meaningfully to a number of sources, primary among which are the logical positivists whose

central doctrine turned on the verification theory of meaning. This confirmational requirement, however, linking scientific laws to observation statements, cannot be satisfied apodeictically and, in consequence, logical empiricism — a more moderate version of logical positivism whose concerns rest with a more liberalized treatment of the problem of confirmation and the meaning imputed to scientific terms — arose. Related to this tradition was the emergence of operationism which, like all the foregoing, suffers from a seemingly critical defect to the effect that:

> ... not only would operationism drastically limit the possibility of extending concepts into new areas, it would entail a great proliferation of the number of distinct theoretical concepts in contemporary science and the surrender of the goal of systematizing large bodies of experience by means of a few fundamental concepts" (Brown, 1979, p. 40).

Moreover, "to present psychological data in operational terms may increase the risk of bias of the observer and experimenter. Operationism comes close to Kant's and Berkeley's subjectivism and even Schopenhauer's solipsism" (Wolman, 1973, p. 45). The long and the short of this criticism, however, is that the research which has emerged from this tradition might stand in need of serious revision because it is suspected that "the standard form of logical empiricist philosophy of science has lost a good deal of its vitality as a research program and that a new approach may be in order" (Brown, 1979, p. 36). In general, it seems that:

> ... social scientists faced with difficult experimental problems, took up hypothesis testing *faute de mieux* to make progress rapidly and to lend their endeavours the appearance if not the reality of scientific respectability. It is in fact irrelevant to science which is concerned with making sense, not conserving resources. (Ross, 1985, p. 514).

Psychology, however, seems to have continued methodologically unperturbed as if its original foundations have remained unaltered; methodology decoupled from epistemology always runs such risks.

On the understanding that fundamental postulates cannot be decoupled from methodology — despite extant attitudes —psychology appears committed, at least, implicitly, to scientific realism. However, it is not always clear what is meant by that term. Firstly, it should be noted that modern realism seems to have grown out of the critique of logical positivism. At its simplest, the term realism — at least as concerns the philosophy of science — is polysemous and despite most of its variations it is, nonetheless, centered on the understanding that the objects of scientific inquiry, that is, the world of material things, exist in space and time and act independently of scientists and their attempts at understanding them. This definition is, perhaps, a blend of what is often called direct or naive realism — often referred to as perceptual realism as well — and scientific realism and psychology seems driven by such a theory of science. This, in fact, is reflected in psychology's hypothetico-

deductivism which presumes to study mind-independent phenomena and is driven by the 'discovery of nature', as is so often claimed.

This view has been propagated not only in noteworthy psychological tracts but in the propaedeutic literature as well. Skinner's viewpoint, for instance, is not only foundational in this respect, but amazingly persistent at the same time. He has argued that:

> ... science ... is an attempt to discover order, to show that certain events stand in lawful relations to other events .... But order is not only a possible end product; it is a working assumption which must be adopted at the very start. We cannot apply the methods of science to a subject matter which we assume to move about capriciously. (Skinner, 1953, p. 6)

Even the philosophically well-grounded are not immune from proffering this view. Wolman, for instance, has suggested that

> ... explanation in psychology reads as follows: A psychological datum is *explained* when the datum is known and the *causes* of this datum are discovered. A psychological datum is *predicted* when the causes of an unknown datum are known and the effects are discovered (Wolman, 1973, p. 42)

— in contradistinction to physics which was beginning to forsake the notion of a direct representation of physical reality.

The propaedeutic literature, it seems, makes the case even clearer. In a classic text the following view of psychological practice is proffered: "Scientific research is systematic, controlled, empirical, and critical investigation of hypothetical propositions about the presumed relations among *natural* [italics mine] phenomena" (Kerlinger, 1964, p. 13). There is an echoing realist refrain in virtually all of the literature which exults in the view that "over the years we have come to depend upon the observational procedures of science as the basis for determining 'reality' because these procedures produce the highest interobserver and intraobserver consistency in reporting 'what is there'" (Arnoult, 1972, p. 4). This is, in brief, the psychological view of science which suggests that "science emerges ... when facts are assembled in a way that: (1) matches what we think we know of the world, and (2) leads us to further discovery and invention" (Agnew & Pyke, 1978, p. 11). This methodological thrust, unfortunately, is so entrenched that it led Royce (1971) to rail against the preoccupation with "pebble picking", that is to say, empirical overload, and to exhort psychology to "boulder building". This empirical preoccupation, he concluded, has had the decidedly unfortunate effect where "the psychological Zeitgeist for the past 24 or 30 years, due primarily to the previously mentioned concern for being identified as 'scientific', has been essentially antirational" (p.225).

This approach is all the more unfortunate, not only for its misbegotten yield, but also for the process which inspired it, that is to say, for its anachronistic view of science. Straightforward scientific realism, likely, is not the most commensurable underpinning for the scientific project. In this

respect, Laudan (1981) has argued that realism is quite multiform and, in consequence, difficult to evaluate. He focusses his attention, however, specifically upon epistemological realism whose tenets comprise notions of reference, approximate truth, and success which, according to some (Putnam, 1978), play a causal, explanatory role in epistemology. However, Laudan's thoroughgoing analysis of this epistemic theory suggests that "nowhere has the realist established — except by fiat — that non-realist epistemologists lack the resources to explain the success of science" (p. 47). Moreover, "given the *present* state of the art, it can only be wish fulfilment that gives rise to the claim that realism, and realism alone, explains why science works" (p. 48). To add to this problematic state of affairs, there is the problem of reflexiveness where "the latter-day realist often calls realism a 'scientific' or 'well-tested' hypothesis, but seems curiously reluctant to subject it to those controls which he otherwise takes to be a *sine qua non* for empirical well-foundedness" (p. 46). Although he does not rule out the possibility of a realistic epistemology of science, *in principle,* Laudan nonetheless concludes that "the history of science, far from confirming scientific realism, decisively confutes several extant versions of avowedly 'naturalistic' forms of scientific realism" (p. 19).

Transcendental Realism and Psychological Science

"There has been a recent subsidence of empiricism in the theory of knowledge" (Will, 1981, p. 1) but psychology does not seem to have noticed. Notwithstanding this obliviousness, this subsidence has had considerable impact, especially on the issue of scientific realism. On that score, Bhaskar (1978) has undertaken a project whose primary aim "is the development of a systematic realist account of science. In this way [he hopes] to provide a comprehensive alternative to the positivism that has usurped the title of science" (p. 8). To that end he has argued eloquently, if not wholly convincingly, for transcendental realism, heir to the two previous, broad positions in the history of philosophy of science — classical empiricism and transcendental idealism. This account, it is argued, is virtually the only one which does justice to the rationality of scientific practice and which renders theory-construction and experimentation intelligible and, as such, represents a virtual Copernican revolution in the philosophy of science. It seeks to synthesize the two main anti-positivist criticisms, that is, those who focus on the social character of science and the phenomena of scientific development and change — for example, Lakatos, Popper, et al. — and those who focus on the stratification of science, stress the importance of models and are highly critical of the deductivistic view — e.g. Hanson, Harré, Hesse, Scriven, et al. Thus, — more by dint of pronouncement than of apodeixis — Bhaskar's transcendental realism provides a non-Kantian alternative to empiricism and rationalism.

The appropriation of this realist theory of science by psychology has been vigorously urged (Manicas & Secord, 1983) but, like the precursive criticisms

of the past thirty years, this exhortation has thus far been without 'paradigmatic' effect. Nonetheless, Manicas and Secord argue "that once they understand it, scientists would happily adopt a realist theory of science" (p. 412). They base this sanguinity upon their interpretation of transcendental — or fallibilist — realism. On this view, a foundationist epistemology — which leans heavily upon 'data', the 'facts', as it were — and the paradigm view, which argues against the logical empiricist assertion of a theory-neutral data base — the bricks and mortar of empiricist epistemology — are rejected in favor of the view that "science aims at discovering lawful processes, but such laws are not about events, but about the causal powers of those structures which exist and operate in the world" (p. 406). Moreover, they invert the logical empiricist credo suggesting, instead, that "theoretical entities are not hypothetical but real; observations are not the rock bottom of science, but are tenuous and always subject to reinterpretation" (p. 406). Their view is based on the acceptance, more properly, their *promotion* of Bhaskar's transcendental realism which:

> ... regards the objects of knowledge as the structures and mechanisms that generate phenomena; and the knowledge as produced in the social activity of science. These objects are neither phenomena (empiricism) nor human constructs imposed upon the phenomena (idealism), but real structures which endure and operate independently of our knowledge, our experience and the conditions which allow us access to them. Against empiricism the objects of knowledge are structures, not events; against idealism, they are intransitive (in the sense defined). On this conception, a constant conjunction of events is no more necessary than it is a sufficient condition for the assumption of the operation of a causal law. According to this view, both knowledge and the world are structured, both are differentiated and changing; the latter exists independently of the former (though not our knowledge of this fact); and experiences and the things and causal laws to which it affords us access are normally out of phase with one another. On this view, science is not an epiphenomenon of nature, nor is nature a product of man. (Bhaskar, 1978, p. 25)

Furthermore, this realist view "holds naturalism to be nonreductive or emergent; both the world and science are stratified" (Manicas & Secord, 1983, p. 401) and, in consequence of these assumptions, it rejects outright the notion of 'brute data'. As far as psychology is concerned, it argues that "to understand persons we need to adopt a hermeneutical approach" (p. 409). In fact, it is averred that this realist dogma is a capacious heuristic which derives from sources as diverse as "continental hermeneutics, post-Wittgensteinian action theory and philosophy of mind, phenomenology, structuralism and neo-Marxism" (p. 399). In addition, it is suggested that experimental psychology must perforce take into account the closure it operates with in the laboratory — unlike the radical openness of the world outside the laboratory. According to this view, "social psychology, then, is ideally a mediating discipline between general psychology, on the one hand, and the social sciences, on the other" (p. 408) inasmuch as it should delineate the articulation between individual behavior and social structure. However, in fact, social

psychology has failed to grasp its role and instead has continued "looking in the wrong place at the wrong phenomena" (p. 409), thus rendering itself otiose, for the most part.

Conclusion

It seems clear from the foregoing that psychology has largely been left behind by events, especially those in philosophy of science. Its overzealous methodological preoccupation has kept it estranged from other epistemological impulses and disciplinary matrices and, as such, it has forfeited its right to the mediating role delineated for it by the transcendental realist project. It has, instead, consigned itself to a sterile hypothetico-deductivism — grounded in the null hypothesis procedure — which is wholly intent upon continued 'pebble picking'. Nothing on the horizon, it seems, bids fair to dislodge this ideology and instigate a 'scientific revolution' in psychology.

More particularly, the tocsin sounded for multidisciplinarism by the transcendental realists — their suggestion that "the explanation of behavior ... is properly a multidisciplinary effort and, though based on the behavioral sciences, necessarily transcends them to involve both biology and the social sciences" (Manicas & Secord, 1983, p. 405) — has gone almost completely unheard. Similarly, the constructionists (Gergen, 1985; Gergen & Davis, 1985; Sampson, 1987) — with their insistence on the recognition of knowledge as a sociohistorical construction and, in consequence, their delineation of psychology's transformative rather than empirical role — have also gone largely unheeded. Critical theory, (Habermas, 1971, 1973), too, arguing as it does for the emancipation of epistemology from pseudonatural constraints in an effort to radicalize it, has played virtually no role whatever in psychology. The hermeneutic project (Gadamer, 1975, 1983; Ricoeur, 1977) as well — despite psychology's recognition of an ontologically grounded, hermeneutic substrate in man (Royce, Coward, Egan, Kessel, & Mos, 1978) — has failed to penetrate psychology's epistemological consciousness. This remissness has not been without effect for psychology has become the poorer — virtually theoretically sterile — for all of this neglect.

References

Agnew, N. M., & Pyke, S. W. (1978) (2nd ed.). *The science game: An introduction to research in the behavioral sciences.* Englewood Cliffs, NJ: Prentice-Hall, Incorporated.

Arnoult, M. D. (1972). *Fundamentals of scientific method in psychology.* Dubuque, IA: Wm. C. Brown Company Publishers.

Bhaskar, R. (1978). *A realist theory of science.* Atlantic Highlands, NJ: Humanities Press Incorporated.

Brown, H. I. (1979). *Perception, theory and commitment: The new philosophy of science.* Chicago: University of Chicago Press.

Doyle, C. L. (1965). *Psychology, science and the western democratic tradition.* Unpublished doctoral dissertation, University of Michigan.

Gadamer, H-G. (1975). *Truth and method.* New York: Continuum Publishing.

Gadamer, H-G. (1983). *Reason in the age of science.* Cambridge, MA: The MIT Press.

Gergen, K. J. (1985). The social constructionist movement in modern psychology. *American Psychologist, 40,* 266-273.

Gergen, K. J., & Davis, K. E. (1985). *The social construction of the person.* New York: Springer-Verlag.

Habermas, J. (1971). *Knowledge and human interests.* Boston: Beacon Press.

Habermas, J. (1973). *Theory and Practice.* Boston: Beacon Press.

Hull, C. L. (1952). *A behavior system: An introduction to behavior theory concerning the individual organism.* New Haven: Yale University Press.

Kerlinger, F. N. (1964). *Foundations of behavioral research: Educational and psychological inquiry.* New York: Holt, Rinehart, & Winston, Incorporated.

Koch, S. (1981). The nature and limits of psychological knowledge: Lessons of a century qua "science". *American Psychologist, 36,* 257-269.

Laudan, L. (1981). A confutation of convergent realism. *Philosophy of Science, 48,* 19-49.

Mackenzie, B. D. (1977). *Behaviourism and the limits of scientific method.* Atlantic Highlands, NJ: Humanities Press.

Manicas, P. T., & Secord, P. F. (1983). Implications for psychology of the new philosophy of science. *American Psychologist. April,* 399-413.

Putnam, H. (1978). *Meaning and the moral sciences.* London: Routledge & Kegan Paul.

Ricoeur, P. (1977). *Freud and philosophy: An essay on interpretation.* New Haven: Yale University Press.

Ross, J. (1985). Misuse of statistics in social sciences. *Nature, 318,* 514.

Royce, J. R. (1971). Pebble picking vs. boulder building. In V. S. Sexton & H. Misiak (Eds.), *Historical perspective in psychology: Readings* (pp. 223-228). Belmont, CA: Brooks/Cole Publishing Company.

Royce, J. R., Coward, H., Egan, E., Kessel, F., & Mos, L. (1978). Psychological epistemology: A critical review of the empirical literature and the theoretical issues. *Genetic Psychology Monographs, 97,* 265-353.

Rozeboom, W. W. (1982). Let's dump hypothetico-deductivism for the right reasons. *Philosophy of Science, 49,* 637-647.

Sampson, E. E. (1987). A critical constructionist view of psychology and personhood. In H. J. Stam, T. B. Rogers, & K. J. Gergen (Eds.), *The analysis of psychological theory: Metapsychological perspectives* (pp. 41-59). New York: Hemisphere Publishing Corporation.

Shames, M. L. (1987). Lagging behind the papacy: Whither psychology's aggiornamento? In H. J. Stam, T. B. Rogers, & K. J. Gergen (Eds.), *The analysis of psychological theory: Metapsychological perspectives* (pp. 25-40). New York: Hemisphere Publishing Corporation.

Skinner, B. F. (1953). *Science and human behavior.* New York: The Macmillan Company.

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20,* 158-177.

Will, F. L. (1981). Reason, social practice, and scientific realism. *Philosophy of Science, 48,* 1-18.

Wolman, B. B. (1973). Concerning psychology and the philosophy of science. In B. B. Wolman (Ed.), *Handbook of general psychology* (pp. 22-48). Englewood Cliffs, NJ: Prentice-Hall, Incorporated.

IDENTIFYING THE PROPERTIES OF LINGUISTICALLY EXPRESSED EXPERIENCE: EMPIRICAL INDUCTION OR INTUITION OF ESSENCES?

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SUMMARY: The identification of properties of linguistically expressed experience depends on (1) prior epistemological goals and (2) prior theoretical concepts. In the *empirical-inductivist* mode of protocol analysis, the goal is to provide a causal-explanatory account of mental processes. Newell and Simon (1972) thus interpret protocols with respect to a highly constrained domain of properties (knowledge and knowledge operations) and ascribe causal status to sequences of experiential properties. In the *phenomenological-intuitionist* mode of protocol analysis, the goal is to provide an essential description of experience as it is phenomenally 'given' to the experiencer. Giorgi (1985) thus interprets protocols with respect to their psychological significance, and does not ascribe causal status to sequences of experiential propeties. Despite these differences, both modes of analysis provide general descriptions of types of experience, based upon a review of the properties of instances.

After a period of consistent opposition to linguistic descriptions in the methods used by academic psychologists, the study of experience through the use of verbal reports has again become legitimate research practise.[1] Among the problems raised by this practise are: (1) how to interpret linguistic accounts as expressive of the properties of another's experience, and (2) how to review a set of such accounts in order to identify their general — or even essential — properties.

Two epistemological traditions have attempted to provide a foundation for addressing these problems. From the perspective of *empirical-inductivism*, the observed properties of a set of actual entities are reviewed in order to determine those that are held in common. From the perspective of *phenomenological-intuitionism*, free imaginative variation is used to discern those properties that are essential for an entity to be of the kind that it is. We will review two different protocol analyses[2] and indicate how they are influenced by these epistemological positions. We will consider Newell and Simon's (1972) empirical-inductivist approach to problem-solving protocols, and Giorgi's (1985) phenomenological-intuitionist approach to protocols

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[1] The term experience, as used here, refers to explicit awareness or *conscious* experience. See Singer and Kolligan (1987, pp. 542-548) for a summary of recent research which views conscious experience as a legitimate domain of psychological inquiry.

[2] Throughout this paper, we will restrict our use of the term 'protocol' to refer to a verbal or written account of conscious experience solicited by an investigator. See also Marcel (1988).

describing an experience of learning. Articulation of the similarities and differences between these modes of protocol analysis may help to clarify the analytic procedures available to investigators who interpret verbal accounts as in some sense indicators of conscious experience.

Two Approaches to Protocol Analysis

In their empirical-inductivist analysis, Newell and Simon (1972, ch. 6) collected "think-aloud" protocols from participants who were asked to solve a crypt-arithmetic problem. These problems require participants to assign digits to alphabet characters in an addition or subtraction problem (e.g., DONALD+GERALD=ROBERT, where D=5). Newell and Simon regard each protocol as a sequential record of a participant's knowledge and operations on that knowledge. Crucial to this view is Simon's (1978, p. 4) contention that utterances are "informationally equivalent" to internal representations of knowledge and operations on knowledge. Below are selected segments from one protocol, along with the knowledge states and operations represented by them. The first statement (a) indicates the original utterance by the subject, while the second statement (b) indicates the experiential properties identified by the researchers:

1) (a) Each letter has one and only one numerical value [E: one numerical value]; there are ten different letters and each of them has one numerical value (b) Ask E about rules defining problem.

2) (a) Therefore I can, looking at the two D's — each D is 5; therefore T is zero (b) Knowledge state: 5 assigned to D; Find column D; Process column; Knowledge state: T equals 0, new.

3) (a) Two L's equal an R; of course I'm carrying a 1, which will mean that R has to be an odd number (b) Get R; Find column R; Process column; Knowledge state: R is odd, new.

Excerpt one is regarded as 'outside the problem space' because it is not a distinct operation resulting in a transformation of the participant's knowledge state. In contrast, excerpts two and three are typical of the operations and resulting knowledge states described by Newell and Simon. The protocol analysis provides a complete record of the sequence of knowledge states and operations used by the participant during solution of the crypt-arithmetic problem. An individual record is used to derive a Problem Behavior Graph (PBG) which represents knowledge as nodes, and operations as arrows between the nodes (Newell, 1977, p. 49). First individually and then as a set, PBG's are reviewed in order to identify (a) the conditions (i.e., knowledge states) that typically elicit particular operations, and (b) typical

sequences of such knowledge states and operations (i.e., episodes). These typicalities, in turn, guide the development of a computer program that, with minor modifications, can simulate each participant's problem solutions.

In contrast, Giorgi (1985) has outlined a phenomenological-intuitionist approach to protocol analysis. He reported the analysis of protocols provided when participants were encouraged to concretely describe prior experiences in which learning had occurred. After reading an entire protocol to get a sense of the whole, 'meaning units' are identified. Each meaning unit is a segment of the protocol that delineates "a change of meaning for the subject" (Giorgi, 1985, p. 11). Then, meaning units are transformed by describing their psychological import, that is, their meaning as phenomenally given to the experiencer and as colored by the particular perspective taken by the experiencer (Giorgi, 1986). The following excerpts demonstrate the progression from (a) the subject's original statement to (b) the transformed meaning units:

1) (a) In a health food store in downtown Pittsburgh a friend and I asked the clerk if she knew how to make yogurt (b) S gets instructions she desires from 'expert other'.

2) (a) Because of its simplicity, I did not write down the recipe but assumed that I could remember it. I tried the recipe about 10 days later (b) Instructions seem simple to S; therefore she committed them to memory and attempted to execute procedure 10 days later.

3) (a) Then I decided that something had gone wrong (b) Third step of process seems not to help. Thus S continues to have doubts about correctness of procedure and decides that there is an 'error somewhere'.

4) (a) I described my mistake in keeping the mixture in the oven for five hours where the instructions called for keeping the mixture at the high temperature for 5 hours (b) S communicates error and the clarified meaning that resulted in success to a friend.

Giorgi then synthesizes the complete set of transformed meaning units into an essential description for this subject, known as a "General Description of Situated Structure of Learning":

> Learning [for this participant] is the acquisition of knowledge concerning, and the actual execution of, as well as the belief in one's ability to execute on one's own, on demand, a progressive step-like procedure which initially involved the clarification through the mediation of others, of ambiguously lived-through moments on account of lack of knowledge, or wrongly posited assumptions (Giorgi, 1985, p. 56).

Finally, several such descriptions are reviewed and compared in order to create a description of a general type of learning, which was labelled "Discovery of Discrepancy Between Assumptions and Situation":

> The subject either posits or is existing within a posited project and then comes across a hard fact that is discordant with the assumptions and aims of the project. This makes the subject pause and reflect on what could be the matter, and he or she becomes aware that another perspective in the situation is not only possible, but sometimes actually operating. The awareness of the other perspective brings more precise clarity to a situation which, from hindsight, is now recognized as having been more ambiguous than it seemed prior to the discovery of the discrepancy (Giorgi, 1985, p. 65).

Comparison of the Analyses

The protocol analyses presented above are similar in two fundamental respects. First, both rely on verbal accounts as indicators of experience. Second, both construct general statements about the experience indicated by the protocol(s). However, there are equally fundamental differences between these two approaches in the roles of (a) *a priori* theoretical concepts and (b) the role of causal explanation.

The role of a priori theoretical concepts. Both approaches to protocol analysis sketched above make use of *a priori* theoretical considerations that circumscribe the *domain* of experiential properties to be identified in the protocol. Since Newell and Simon's overall goal is to construct a computer program capable of solving crypt-arithmetic problems, they explicitly limit the domain of relevant properties prior to any protocol analyses. Specifically, they interpret the protocol by identifying categories of knowledge states and categories of knowledge operations. Although the specific categories are contingent on the particular problem being solved (e.g., crypt-arithmetic *vs.* chess), these theoretically *a priori* concepts constrain the range of what Goodman would call the projectible predicates of this inductive effort (see Goodman, 1978).

In contrast, Giorgi is much less restrictive in the application of *a priori* theoretical concepts. The goal of Giorgi's analysis is to provide an essential description of subject-dependent, or psychologically-situated, meaning. Each step in his analytic procedures — the delineation of meaning units, the interpretation of 'psychologically significant' meanings, and the synthesis of transformed meanings into an essential description — takes place within a certain psychological perspective. As Giorgi indicates, to adopt such a perspective means "to set some limits ... on the analysis and to thematize only a particular aspect of a more complex reality" (1985, p. 12). However, Giorgi's *a priori* categories are much more inclusive than Newell and Simon's. As Giorgi acknowledges, "While there are presuppositions and a general precomprehension, these are not specific enough to delineate the relevant categories in an exclusionary way" (1985, p. 13).

In summary, although both approaches to protocol analysis acknowledge the role of prior theoretical considerations in the identification of experiential properties, these *a priori* categories play sharply contrasting roles. For Newell and Simon, no fundamental property of experience is interpreted beyond knowledge states or operations on knowledge states. In their case, prior theory *narrows* the domain of projectible properties to those that describe variations in programmable knowledge states or transformations thereof (e.g., process column). For Giorgi, the psychological set adopted by the researcher allows for differentiation of a wider range of meanings, nearly as many as are expressible in our conventional language (e.g., subject gets instructions from 'expert other'). In this case, prior theory minimally constrains the range of projectible properties, providing considerable *flexibility* in their identification and articulation.

Temporality and causality. The epistemological bases of empirical-inductivist and phenomenological-intuitionist protocol analyses also influence how each deals with the temporal and causal relations among experiential properties. Newell and Simon regard the goal of their procedures as a search for a *causal-explanatory* account of mental processes. In this framework, knowledge and knowledge operations are graphed in the PBG in a specific temporal sequence (see Newell and Simon, 1972, pp. 174-181). Further, this temporal sequence has explicit causal import, that is certain knowledge state conditions result in the selection of a particular operation, even though other operations might be legitimate. Thus, Newell and Simon search for temporally-ordered experiential properties with specific causal consequences for solution of the problem.

Giorgi's analyses similarly involve temporal relations among experiential properties. Meaning units are explicitly ordered with respect to their sequence in the original transcript, and the transformed meaning units preserve this temporal sequence. For instance, several of Giorgi's (1985) General Descriptions of Situated Structure of learning experience make implicit reference to temporal order (e.g., "a progressive, step-like procedure", "... initially involved clarification through the mediation of others, etc.). However, Giorgi identifies temporal relations among properties of experience without ascribing *causal* status to them. This is consistent with the fundamental epistemic goal underlying his approach: *description* as opposed to causal explanation of mental acts. Although phenomenological description fundamentally constrains any causal account of the phenomenon in question (see Husserl, 1970/1900), such description does not in itself entail causal explanation.

In summary, although both approaches to protocol analysis allow for temporal sequences of experiential properties, prior epistemic goals determine whether temporal relations imply causal relations. In order to provide a causal-explanatory account of problem solving, Newell and Simon link

temporal relations among experiential properties to explicit causal relations. In order to provide an essential description, Giorgi describes temporality without reference to causality.

The role of induction. There is an important sense in which Newell and Simon's empirical-inductive and Giorgi's phenomenological-intuitive analyses converge when comparisons are made *among* subjects within a set of protocols. Newell and Simon compare protocols in order to identify (a) the conditions (i.e., knowledge states) that result in particular operations and (b) typical sequences of knowledge state transformations. Similarly, Giorgi compares several descriptions of learning experiences in order to identify types of learning. In both cases, actual protocols within a set are reviewed to (a) differentiate protocols within the set according to their similarity to each other, and (b) identify properties of experience that are common to subsets of individuals in the sample under study. In this sense, both modes of protocol analysis utilize a procedure akin to induction.

This convergence in reliance upon empirical variation may only be superficial, since Giorgi emphasizes free imaginative variation in his procedures. However, Giorgi's analyses appear to limit the use of imaginal variation to derivation of the psychological sense of the meaning units *within* protocols. Variations among actual protocols are apparently used to develop generalizations about a set of research participants. Perhaps imaginative variation establishes the possible (i.e., projectible, see Goodman, 1979) psychological sense of the meaning units within a protocol, but actual variation establishes the common properties of similar protocols within a set. Regarding the latter possibility, Merleau-Ponty suggested that:

> If eidetic psychology is a reading of the invariable structure of our experience
> based on examples, the empirical psychology which uses induction is also a
> reading of the essential structure of a multiplicity of cases. But the cases
> here are real and not imaginary (Merleau-Ponty, 1964, p. 70).

In conclusion, both the empirical-inductive and phenomenological-intuitive methods outlined above perform an operation akin to induction when comparisons are made among protocols provided by a sample of actual research participants. Despite this convergence, the precise relations between empirical and imaginative variations on a kind of entity remain unclear. If imaginal variation does function to establish the projectible sense of meaning units, there may be unacknowledged overlap between these modes of protocol analysis. However, both the concept of projectibility and the process of free imaginative variation will require clarification before this overlap can be precisely discussed. This clarification would be an important step toward articulation of how interpretations of linguistic expressions can contribute to psychological research.

References

Giorgi, A. (1985). *Phenomenology and psychological research*. Pittsburgh: Duquesne University Press.

Giorgi, A. (1986). The meaning of psychology from a scientific phenomenological perspective. *Etudes Phenomenologiques*, *4*, 47-73.

Goodman, N. (1978). *Ways of worldmaking*. Indianapolis: Hackett Publishing Company.

Goodman, N. (1979). *Fact, fiction, and forecast*. Indianapolis: Hackett Publishing Company.

Husserl, E. (1970/1900). *Logical investigations*. (Trans. J. N. Findlay) New York: Humanities Press.

Marcel, A. J. (1988). Phenomenal experience and functionalism. In A. J. Marcel & E. Bisiach (Eds.), *Consciousness in contemporary science*. Oxford: Clarendon Press.

Merleau-Ponty, M. (1964). Phenomenology and the sciences of man. In J. M. Edie (Ed.), *The primacy of perception*. Evanston: Northwestern University Press.

Newell, A. (1977). On the analysis of human problem-solving protocols. In P. N. Johnson-Laird & P. C. Wason (Eds.), *Thinking: Readings in cognitive science* (pp. 46-61). London: Cambridge University Press.

Newell, A., & Simon, H. A. (1972). *Human problem solving*. Englewood Cliffs, N. J.: Prentice-Hall.

Simon, H. A. (1978). On the forms of mental representation. In C. W. Savage (Ed.), *Minnesota Studies in the Philosophy of Science*, (Vol IX, pp. 3-18). Minneapolis: University of Minnesota Press.

Singer, J. L. & Kolligan, J. (1987). Personality: Developments in the study of private experience. *Annual Review of Psychology*, *38*, 533-574.

THE SCIENTIST WHO MISTOOK HIS OBJECT FOR A METHOD, OR: CAN WE MAKE A NON-CLASSICAL PSYCHOLOGY?

Arno L. Goudsmit

SUMMARY: Two research traditions in psychology, called the 'outside tradition' and the 'inside tradition', are compared in respect of their weak sides. These weaknesses can be seen to converge precisely toward one central issue that usually evades attention: the difficulties a researcher may have to distinguish object and method of investigation. By including these difficulties as a substantial topic of study, a non-classical psychology is considered possible. It is claimed that an empirically founded theory of psychotherapeutic practice, that intends to be more than a conglomeration of facts and feelings, must be non-classical.

Introduction

There is a basic split between two research traditions in psychology. We may call them the 'outside tradition' and the 'inside tradition'. The following terms connote these two traditions:

'Outside Tradition':	'Inside Tradition':
positivistic	phenomenological
experimental	descriptive
reductionistic	holistic
explaining	understanding
nomothetic	idiographic
-etic	-emic
quantitative	qualitative
objective	subjective
'hard'	'soft'

These two research traditions, based on much older philosophical traditions, have developed during this century into more and more mutually opposing movements in psychology, each having its own strong and weak sides. Usually in debates between the two movements the strong sides of one are contrasted with the weak ones of the other. Also some 'reconciliating' styles of research have been developed, in which it is tried to combine the strong sides of both, for example, by having a large quantitative survey study be preceded by a small qualitative exploratory 'pilot' study. No attempts seem to have been made to relate the weak sides of both traditions. Why should one?

The aim of this contribution is to do just this: to relate the weak sides of both, in order to show that these weaknesses can be seen to converge precisely

toward one central issue, that usually evades attention. This central issue will be described in terms of the relations between the object of investigation and the method.

A Metaphor From Quantum Physics and From Phenomenology

It usually goes unnoticed that two so remote authors as Niels Bohr and Maurice Merleau-Ponty both make use of a particular metaphor, in discussing aspects of human perception. Bohr (1934, p. 99), being concerned with the boundaries between the observer and the physical object he is investigating, had a need to express the impossibility of observing one's measuring instrument while using it as such. This was particularly relevant in his description of the role played by the measuring device while observing quantum phenomena. Merleau-Ponty (1945, p. 167) was also concerned with the boundary between a sensing individual and his environment, with regard to how an individual organizes his environment in respect of his own body. Both authors use, in order to make their point, a metaphor which is quite interesting for our present purposes. It is the metaphor of a stick that can be used as an instrument of touch.

Bohr speaks of a stick that, if grasped firmly, can be used as an instrument for touching objects in the environment. The tactile sensations in the hand then escape from attention, and instead the distal edge of the stick takes the quality of a tactile organ. It is there, at this distal site, where the person observes the object he is touching with his stick; no longer is the palm of the hand the boundary of the person as a sensing unity, but the edge of the stick. Conversely, if the stick is held loosely, it cannot be used as an instrument of touch, and it appears to the observer as a stick, that is, as an independent object, sensed in the hand. Likewise, Merleau-Ponty gives the example of a blind man using a stick as an elongation of his own body. Thus, the blind man has his stick 'participate' in his own body. Then the way in which he perceives his environment by means of the stick necessarily evades his attention. The stick has become incorporated.

Both for phenomenologists and for quantum theorists the metaphor illustrates that we cannot simultaneously pay attention both to an object and to the method by which it is perceived. Bohr is considered to have been inspired in this respect by William James (cf. Holton, 1973).

According to this basic complementarity between method and object of perception, there seem to be two possible courses of action for a researcher: either to focus upon the object, and allow the instrument to become part of his 'tacit knowledge' (e.g., Polanyi, 1966), or to keep his mind clear about his method, irrespective of the objects that we will face. This is a matter of *priority*. It is the (implicit) assignment of this priority by the researcher which deter-

mines in which tradition he operates. Priority of object over method has been assumed by the 'inside' tradition, and the reverse priority has been assumed by the 'outside' tradition.

I will describe the object-method complementarity in terms of a relation between this priority and the weaknesses of both the 'inside' and the 'outside' tradition. First, let me give a sketch of these weaknesses.

The Weakness of the 'Inside' Tradition: The Method Fades Out

The inside tradition has been defined in various ways. Shotter's description of 'practical knowledge' is the most proper to our present purposes. He describes it as: "... knowing from within a situation, which takes into account, in what is known, the situation within which it is known." (Shotter, 1985, p. 448). Adherents of the inside tradition may or may not differ in respect of whether the object they are interested in exists independently of their knowing acts. What matters is that the object is given priority to the method. Here we encounter a problem. The knowing person is considered to influence, if not to constitute, his object of study by his knowing acts. However, a specification by the researcher of the context in which the object exists would also comprise his act of knowing the object.

The inside tradition has thus as a logical limit the researcher's attempt to specify fully the method itself as a 'contextual aspect' of the object of study. This is why Polanyi's term 'tacit knowledge' is felicitous: the observer will never end in making the context explicit. His inside knowledge of the object seems to consist of an infinite hierarchy of implicit abilities, each of which is concerned with putting into practice the knowledge that has already been made explicit.

Consider the following examples:

a) A practicing psychotherapist tries to be explicit about what he is doing when he is empathic with a patient. As soon as he starts to regard his empathy as a technique that can be performed, he finds himself in need of pointing to his own private experiences, from which he was able to utilize this 'technique' appropriately. The crux of empathy, and of authenticity in general, seems to escape whenever one tries to formulate it as an executable technique.

b) A participant observer tries to be explicit about his performance, for example, in an anthropological field study. Since his behavior is part of the situation studied (e.g., a party), description of his research behavior is necessarily embedded in a description of the situation. The method is absorbed by the object of study. Extricating it from the object can only

be done at the cost of no longer understanding why this 'procedure' has been followed.

The weakness of this tradition, thus, resides in the researcher's incapacity to account fully for the method by which he obtained his 'inside knowledge'. Rather, he feels compelled to declare his knowledge to be of too much a 'contextual' nature as to formulate it as a fully explicit and reproducible procedure. The method is absorbed by the object of study, and cannot be extricated from the object. The object has priority over the method. The 'pure' method can only be formulated in abstract terms, as an in itself impracticable procedure. Wouldn't the availability of a clear and context-free method free us from many impediments?

Weakness of the 'Outside' Tradition: The Object Fades Out

The 'outside' tradition is the mainstream scientific culture. It assigns a predominant role to the development and elaboration of scientific methods. Only by being critical about the way one conducts one's observations and argumentations, it is thought possible for a researcher to arrive at knowledge that is of a justifiable degree of certainty. If the method cannot be accounted for, then the insights thus obtained are considered gratuitous.

Adherents of the outside tradition may or may not differ in respect of whether the object they are interested in exists independently from their acts of measuring it. What matters is that the method is given priority to the object. It is thought virtually possible to describe this world by means of an axiomatized system ('more geometrico'), that is, by means of a method we are fully aware of, as we apply it. The method can also be conceived apart from that to which it is applied.

Since the method is kept here continuously under critical control, the weakness of this tradition does not reside, as above, in a lack of explicit procedural knowledge. To the contrary, the research techniques are clear and accessible. This time, however, the problem is in the object of investigation.

Many aspects of the 'outside' techniques and methods, therefore, are aimed at establishing a clear image of the object, distinct from the method itself. The bulk of statistical techniques for example is designed to do just this. It allows the researcher to 'subtract' the assessed features of the measuring procedure from the 'raw data', so that what remains may validly be interpreted as features of the object of investigation. This kind of 'subtraction thought' is beautifully illustrated by the language used in the following fragment:

> If, after determining that neither concept redefinition nor scale recalibration
> has occurred, a researcher observes a difference in subject responses from
> time-1 to time-2, behavioral change can be said to have been detected.
> (Armenakis, 1988, p. 165)

For adherents of this tradition it is of the foremost importance that the object can be distinguished from the measuring instrument. Statistical criteria are often available to calculate measurement errors, and to decide whether or not measurements validly and reliably represent features of the object of study.

The outside tradition has thus as a logical limit the researcher's attempt to specify fully the object itself as the outcome of his method. Then the researcher becomes more and more entangled in technical issues of analysis, instead of being able to perceive his object by means of his method. The 'error terms' become too high, the 'signal-noise ratio' too low, or the sample too small. As a result, the focal object fades out.

The less the object can be distinguished from the method, the more the latter is given priority over the former. In a sense, then, the object is absorbed by the method and can only be formulated as an abstract concept.

Object-Method Complementarity and the Relation Between Both Traditions

The validity and reliability issue is for the outside tradition the Achilles heel, as is the context issue for the inside tradition. The weakness of the inside tradition resided in that the method was absorbed by the object; the object was no longer a background against which the method could be delineated. Likewise, the weakness of the outside tradition resided in that the object was absorbed by the method.

What the weaknesses of both traditions have in common is their convergence to indistinguishability[1] between object and method. We say that the method cannot be distinguished from the object, if the object absorbs the method. In this way we are able to formulate the weaknesses of both traditions in terms of the amount of difficulties it takes the researcher to distinguish object and method from one another.

We may put our two traditions thus on a horizontal 'distinguishability' dimension (see Figure 1), linking the two traditions together at their weakest spots. Here 'weak inside' means that many difficulties arise for the researcher in distinguishing method from object. 'Weak outside' means the same for the reverse distinction: object from method.

The more difficulties arise in making either distinction, the more the 'priority' that is assumed by a tradition, becomes relevant. This 'priority' is given by the researcher in accordance with the tradition he is in. 'Priority of object over method', for example, is the degree to which the researcher attempts to keep sight of his object of investigation. The priority value,

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[1] Notice that such a distinction means that object and method are not distinguished as entities 'in themselves', but always as a figure against a background. Either may take the role of figure or background.
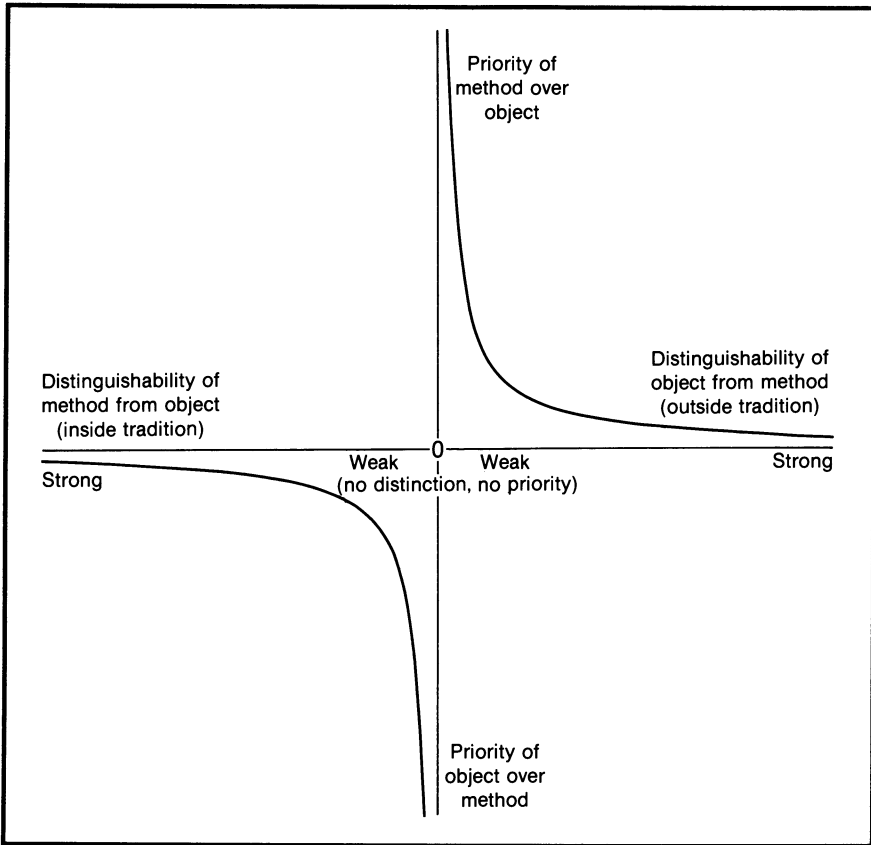
Figure 1.

therefore, denotes that to which the researcher clasps as he looses the ability to make distinctions.

Figure 1 shows 'priority' as a function of 'distinguishability'. Then at the 'weakest' value of both traditions the 'priority' values (i.e., the priority of object over method, as well as the priority of method over object) asymptotically tend to infinite values. That is, at the value of 'no distinction' between object and method, 'priority' is undefined. For the present purposes of conceptual definition, the relation between 'distinguishability' and 'priority' may be sketched as a hyperbola of the type: $y = 1/x$. Clearly, at extremely 'strong' values of 'distinguishability', the 'priority' values asymptotically tend to the 'no priority' value: object and method are clearly distinguishable.

The Domain of a Non-Classical Psychology

Notice that both dimensions 'distinguishability' and 'priority' are defined in relation to the researcher who attempts to distinguish method and object.

They are not defined as ontological 'an sich' qualities of method-object relations, that exist irrespective of an observer.

Regular psychology[2] in fact does make such an ontological assumption, namely, that object and method belong to distinct categories. Instead, a non-classical psychology takes into account the full variety of relations that may hold between object and method in terms of the researcher's capacities to distinguish the two. Predominant in this arrangement is the central asymptotic 'distinguishability' value, at which 'priority' is undefined. As a limit case of the outside tradition, the method has become here its own object of investigation. Likewise, as a limit case of the inside tradition, the object has become here fully its own method of investigation. Thus, at this point a fully self-referential relation occurs between object and method, that we may consider as a merge between the two.

By including the vertical asymptote we admit a basic incapacity for a researcher to maintain *always* a clear object-method distinction. The two 'regular' traditions, then, can be considered as unwarranted generalizations of a perfect object-method distinguishability. They both leave out the undefined point, at which the self-referential merge of object and method takes place.

Rather than denoting some 'temporary imperfection' in his method, a researcher's failures and inabilities to make object-method distinctions are now to be understood of *substantial* interest to his field of study. Thus, a whole universe of new empirical phenomena opens up, that is concerned with:

a) properties of objects of investigation that do not allow a clear object-method distinction; and

b) the ways a researcher may become entangled in keeping his mind clear about object-method distinctions.

In other words: a universe of phenomena in which the researcher himself also enters as a participant (cf. von Foerster, 1981).

An Empirical Theory of Psychotherapeutic Practice

It is precisely this universe that is also the work area of practical psychology, in particular psychotherapy. For if anywhere, it is here that

a) clear distinctions between the topics that are discussed and the ways in which these topics can be known, often disappear;

b) practitioners favor a culture of self-observation and the use of oneself as a supreme instrument in the study of their therapeutic interactions (e.g., Reik, 1948).

[2] The reader may wonder why we do not speak more generally of 'regular science'. It is because modern physics, par excellence, has been already non-classical since the twenties of this century, whereas current mainstream psychology still mirrors itself to 19th century 'classical' physics.

I, therefore, claim that an empirically founded approach to psycho-therapeutic practice, that accounts for this self-inclusion by the therapist and not merely dismisses it as 'non-scientific subjectivity', must be non-classical.

In such an approach to psychotherapeutic practice the various ways a person (whether patient, psychotherapist, or therapy researcher) may deal with object-method distinctions, are phenomena of substantial (and not merely of methodological) interest. Research questions then are about the ways people get confused about object-method distinctions, and how they may 'infect' one another with these confusions (cf. Goudsmit & Mowitz, 1987).

Curiously, themes like 'incapacity' and 'confusion', 'indistinguishability' are highly disliked by academic researchers as respectable phenomena of study, especially when their own confusions become the topic of interest. However, if psychology is to account for psychotherapy as a scientific activity, it must be in a framework of non-classical psychology. If not, then either psychotherapy will eventually degenerate into a pseudo-medical technology, or it will be disqualified as an ('unscientific') art.

References

Armenakis, A.A. (1988). A review of research on the change typology. *Res. in Organ. Change and Development, 2*, 163-194.

Bohr, N. (1934). The quantum of action and the description of nature In N. Bohr, *Atomic theory and the description of nature* (pp. 92-101). Cambridge: Cambridge University Press.

Foerster, H. von (1981). On cybernetics of cybernetics and social theory. In G. Roth & H. Schwegler (Eds.), *Self-organizing systems. An inter-disciplinary approach* (pp. 102-105). Frankfurt M./New York: Campus.

Goudsmit, A.L., & Mowitz, J. H. (1987). Over inhoud en proces in psychotherapieonderzoek. Een methode van onderzoek naar therapeutisch handelen in ontdekkende psychotherapieën. *Handelingen, 1*, 48-63.

Holton, G. (1973). *Thematic origins of scientific thought.* Cambridge, MA: Harvard University Press.

Merleau-Ponty, M. (1945). *Phénoménologie de la perception.* Paris: Gallimard.

Polanyi, M. (1966). *The tacit dimension.* New York: Doubleday.

Reik, Th. (1948). *Listening with the third ear. The inner experience of a psychoanalyst.* New York: Grove Press.

Shotter, J. (1985). Accounting for place and space. *Environment & Planning D: Society & Space, 3*, 447-460.

ESSENTIAL UNPREDICTABILITY

Paul van Geert

SUMMARY: Nomothetical theoretical models, including those in psychology are judged according to general criteria among which that of predictability is one of the most important. That is a nomothetic model should be capable of predicting future events and paths of processes successfully. It is claimed that such models base their predictions upon empirical generalizations of normal courses of events, and not upon models of developmental mechanisms. In the present article the question is raised whether or not predictability can in principle be achieved. A simple model of longitudinal cognitive growth is presented based on the logistic growth equation. It is argued that this model produces good theoretical reconstructions of empirical cognitive growth sequences. It is shown that the behavior of growth processes based on the logistic assumption is nevertheless very complicated, and sensitive to differences in parameters that are well beyond our possibilities of psychological measurement. Some examples of unpredictability are presented. It is argued that a model of psychological development should be a model of a dynamics describing ranges of individual processes. Such processes are characterized by intrinsic prediction horizons which are not reducable to influences of free will or major random factors.

Prediction is one of the aims of science (Kerlinger, 1965; de Groot, 1960). If two theories claiming to explain the same set of empirical phenomena differ in predictive power, then the theory yielding the best predictions is the better of both. However, the question is: how do these theories generate predictions? This question reduces in its turn to the question of what kind of theories these theories actually are. For instance, one theory might just be a dressed up description of empirical phenomena, and so its predictions are just empirical generalizations of observations. Clearly, a theory should provide a model of a process or phenomenon, and it should be easily transferable into a calculus generating predictions of future events in a recursive way. That is, by automatic application of the calculus implicit in the theory to a set of data describing an initial or starting state, it should generate a description of a future state of affairs. This description should specify a measurable variable, a time of occurrence, and a probability describing the proportion of the specified outcome relative to an infinite number of potential tests. The problem with most psychological theories dealing with psychosocial phenomena, however, is that they hardly succeed in providing a calculus necessary for generating a prediction. Part of their trouble lies in the occurrence of essentially unpredictable events that might interfere with the predicted properties or behaviors (e.g., a prediction of brilliant school results depends on the student not catching a serious illness, or getting addicted to drugs). In principle, such a difficulty could be solved if a reasonable estima-

tion of the probability of such events is available. But even then, most, if not all, of our available models of long-term processes fail in specifying some sort of calculus. More precisely, the models that do indeed present a sort of calculus do so in the form of a disguised set of empirical generalizations. The disguise takes the form of a factor model. Take, for instance, the following fake factor model of cognitive achievement over the years (Figure 1).

The model states that the variance due to cognitive achievement in a population is explained by variance in three factors (social background, intelligence, and temperament). Each factor explains a specified part of the variance of cognitive achievement. In principle, that ratio of explained variance may change over the years (see Figure 1). In addition, the value on each factor has a specified stability. Thus, given an initial state description consisting of specified values over the three factors, one might easily compute the probability that a subject will fall within a specified domain of cognitive achievement. The problem with a factor model, however, is that it actually represents an empirical generalization. It is based on observations of stability of the factors over the years, and of the contribution each of the factors makes to the predicted variance. Thus the theory does not provide a description of a mechanism in the real world, of real world inputs to that mechanism, and of real world outcomes. There is no mechanism in the world out there that takes as its input a value on three factors, and that produces a specific cognitive achievement level as its output. There is a mechanism, though, as far as cognitive achievement is concerned. It is an information processing mechanism, processing input information on the basis of production rules and representations stored in memory. Any explanatory model of cognitive achievement should consist of a model of this information processing mechanism. It is this explanatory model — and not the hidden descriptive factor model — that should generate our predictions of future cognitive achievement, skills, and knowledge. It is clear, however, that the present models, however sophisticated they are, will not do the job.

In general one can tell from one's experience in psychological research that reliable long-term predictions of psychological achievements are extremely hard to make. Of course we have a considerable knowledge of the distributions of psychological properties over populations (e.g., academic education, unschooled labor, IQ distributions, etc.). We could then 'predict' one's chances of becoming a psychology professor, a priest, or the sovereign of a small monarchy, but such predictions, it has already been said, are not based on explanatory models, that is, models of the mechanism that lead one to become a professor, a priest, or a king. Now one could blame psychology, because it has not yet provided any interesting explanatory model. The latter is hardly true, however. One might put the blame on nature, because it interferes with the smooth course of psychological change in the form of unpredictable events. But if that is true our models should be capable of
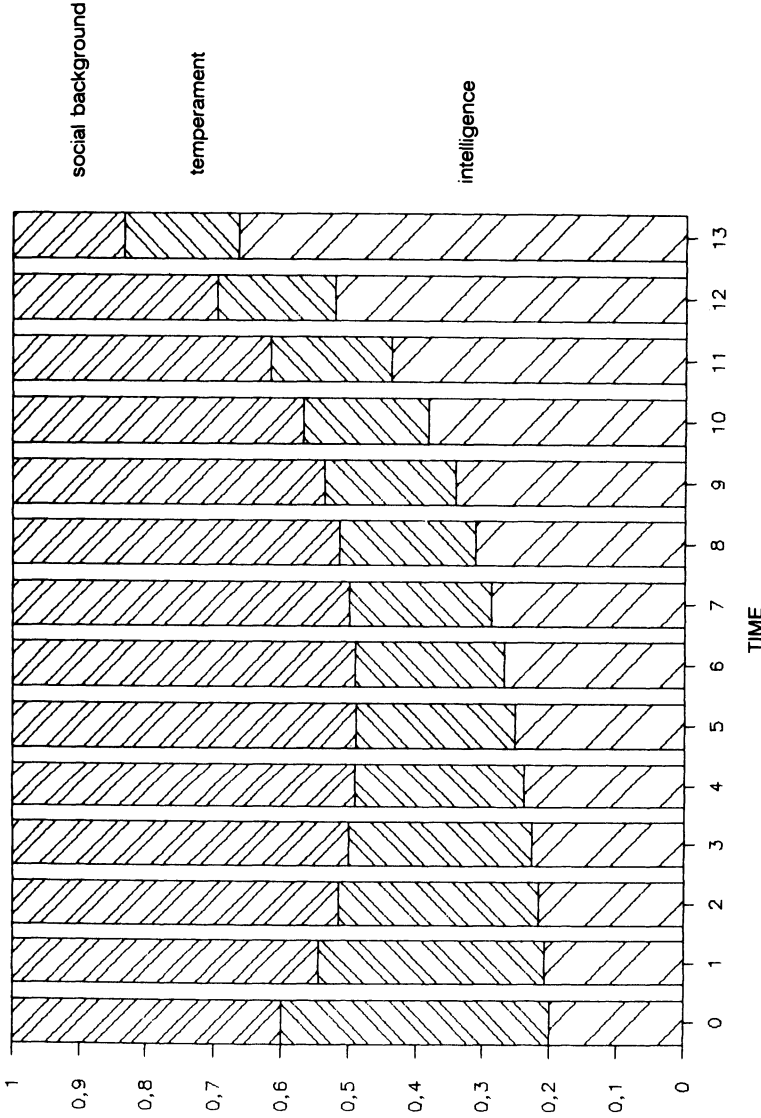
Figure 1. A fake 'Factor Theory of Cognitive Achievement'. From bottom to top, zones represent the factors 'social background', 'temperament', and 'intelligence'. As children grow older (x-axis) the percentage explained variance of the factors changes. Predictions based on this 'theory' are mere empirical generalizations.

explaining why our psychological development and change is actually
influenced so much by those events. Furthermore, they should be able to
predict the future, given one or other major life event (i.e., when such major
accidents are held constant, prediction should be possible). However, it
seems that even under stable circumstances the course of psychological
development and change is rather difficult to predict (again provided that our
predictions are based on explanatory models, not on empirical generaliza-
tions). In this paper I shall try to show that the reason for this difficulty lies
in the inherent unpredictability of dynamic processes. We shall see that, by
their very nature, prediction is possible only for a trivially limited set of
psychological processes or for limited time spans. There is a definite predic-
tion horizon inherent in the natural processes under which psychological
processes are also subsumed. Those processes are dynamic growth processes.
I shall first explain their nature and form, and argue why they are explanatory
in the sense of providing a model of an underlying mechanism. I shall then
give some examples of psychological long-term phenomena to illustrate that
these dynamic growth models provide good formal descriptions of the empiri-
cal course of events. I shall then show why and how these processes are
characterized by a definite prediction horizon. Finally, I shall present some
methodological and research consequences of the prediction model discussed
in this paper. I shall confine the discussion to cognitive growth processes.
However, the reasoning holds for all psychological processes possessing the
properties relevant to the application of the growth model discussed below.

A Model of Cognitive Growth

The cognitive system is assumed to consist of semi-independent com-
pon-ents, describable in the form of quantitative variables. One example is
the lexicon, another one's knowledge of color terms. Further examples are
one's proficiency in solving mathematical equations, one's understanding of
the concept of temperature, and so forth. An example of the latter sort of
component can be expressed quantitatively in the form of the set of problems
taken from a test that a subject may solve. These components grow, that is,
they increase (or decrease) quantitatively over time. Semi-independent as
they are, though, the components entertain various types of mutual relation-
ships, which are either supportive (the growth in one supports the growth in
another), competitive (the growth in one relates to the decline in another), or
(virtually) neutral. The components show strongly dissimilar growth rates
and growth-onset-times. The components compete for limited spatio-tem-
poral, informational, and energetic resources (e.g., time to exercise a cognitive
skill competes for the time available to exercise any other skill). The sum of
resources results in a semi-stable carrying capacity; that is, the cognitive
environment is able to maintain a limited (though vast) number of

components each characterized by a specific and limited growth level. The growth level of a component at time t is written as L_t.

The model presented here deals with the evolution of L_t in time, that is, longitudinal cognitive growth. The model is expressed qualitatively as follows:

$$L_{(t+f)} = f \{ L_t, r, K, (K - L_t), C (c, ...), t, f, p, w \}$$

L_t is an initial *growth level*. The *growth rate* r expresses the intrinsic productivity of a grower. The *carrying capacity* K refers to the principle of competition for limited resources in ecological systems. Cognitive growth feeds upon spatio-temporal, informational and energetic resources. Complex as the structure of these resources may be, they can be expressed in the form of a single variable K, the carrying capacity for L of all the resources normally available. K corresponds with the maximal stable level a growing variable may achieve, given the sum of resources presently available to the grower. The *unutilized opportunity for growth* ($K - L_t$) is that portion of the resource reserve waiting to be utilized for further growth. In principle, changes in K are (very) slow in comparison with changes in its supported growers. The *competitive grower* C (if any exists) competes for the same resources as the one whose growth we try to explain. The *competition factor* c expresses the strength of this competitive relationship. In view of the complexity of the supportive and competitive relationships in systems behaving according to the ecological principles discussed earlier, we reckon with competitive growers separately only if the competition takes place at about the same time level as L-growth, such that it cannot be accounted for in the form of the resource level or carrying capacity K. The *time of growth* t is the time elapsing between the initial and the final state of a growth process (in principle any couple of states can be taken as the initial and the final state, but it is more natural to take an early state where the growth level is minimal as the initial state, and a late state where the growth level has either stabilized or set into cyclic motion as the final state). In fact, t specifies the temporal interval for which a specific growth equation holds. The longer the interval a growth equation covers adequately, the more powerful its underlying growth model. The *feedback delay* f is the average time needed for the variables (K, L, C, ...) to exert their influence upon the grower. The factor f implies that a developmental growth state L_i is not necessarily determined by its direct predecessor state L_{i-1}, but eventually by a distant predecessor L_{i-n}. It is claimed that f is about constant for any non-trivially long growth interval t. Finally, the *intrinsic random fluctuation factor* p is a small positive or negative random factor adding to the growth level. Since the model is a model of growth, and not of its measurement, p should refer to real perturbations in the grower, not to measurement or observation errors. In principle, p should be rather small, in view of the fact that growth depends on a growth mechanism which is not expected to change drastically because of intrinsic perturbations in its structure (compare this to

single genetic mutations, which are either very small or lethal). Observed perturbations are either due to external accidents, for example, affecting the resource factor K, or to deterministic (near)-chaotic growth processes (see further). The *weight factor* w is a complex factor determining the relative weight of K, L_t, and (K - L_t) in specific growth equations (for w either 1 or 0). Although several variables in the model are clearly not independent, they are treated as separate variables because they have a distinct role in describing specific families of growers.

The quantitative basis of the model is the so-called 'logistic growth equation'. It states that the growth level at a later stage, for example, L_t is explained, first, by the growth level at an earlier stage, L_{t-f} (remember that f is the feedback delay, which is a measure of the system's inertia or resistance to change), and, second, by a growth rate R. The growth rate R, however, depends on an intrinsic growth rate r, on the one hand, and the relative unutilized opportunity for growth on the other hand. Combining these assumptions we find the equation for logistic growth under delayed feedback

$$L_t = (1 + r) \cdot L_{(t-f)} - r \cdot L_{(t-f)}^2 / K$$

In principle, the general growth model and growth equation are partially recursive. That is, they apply not only to the growth level, but also to growth rate itself (which too may be subject to growth), to carrying capacity (which may grow as a consequence of growth in supporting variables), and to eventual competitive growers.

Empirical Evidence for the Growth Model

Although the present article is definitely not the place where the empirical evidence for the model should be discussed, I shall give a hint as to the sort of data supporting the model (for a more complete account, see van Geert, 1989). Forms of exponential growth have been found in the first stages of lexical development, for instance. The increase of words in the lexicon during the first weeks of the one-word stage follows an exponential course (Dromi, 1986; Nelson, 1985). In view of the explosive nature of such growth, however, exponential growth should level off soon towards an optimum level (Dromi, 1986). The resulting growth curve can be modelled accurately in the form of a logistic growth curve with delayed feedback (Figure 2; van Geert, 1989).

Approximating growth has been found in early syntax acquisition (Brown, 1973, Labov & Labov, 1978). The growth of the correct form of a syntactic construction such as the present progressive or inversion in interrogative sentences shows an oscillating course. The amplitude of the oscillations decreases as the growth level approaches the 100 percent correct level. The model of cognitive growth, based on the logistic growth equation,
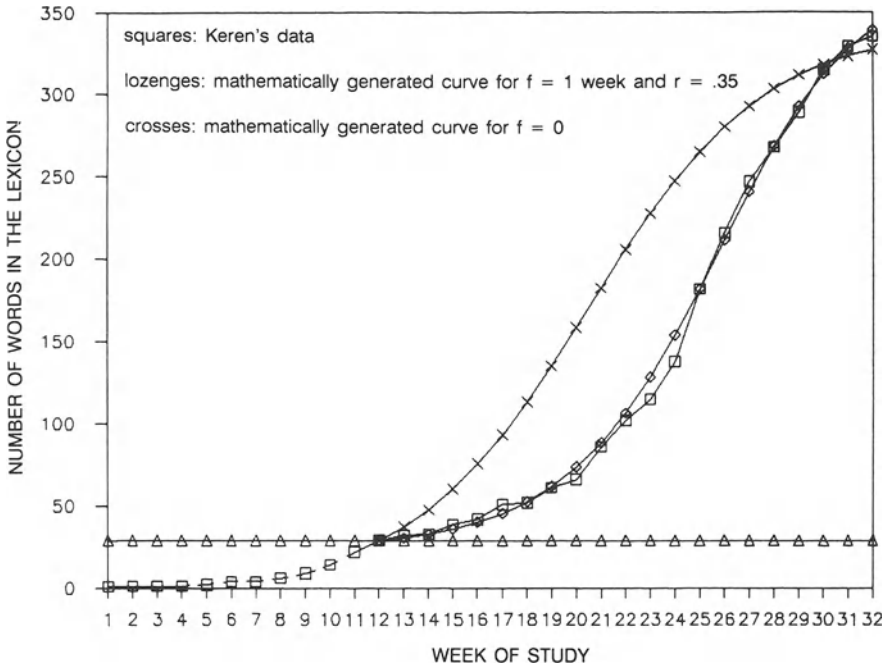
Figure 2. Reconstruction of the second substage of the one-word stage
in vocabulary learning (after Dromi, 1986). A feedback delay of one
week, and a growth rate of .35 per week give an almost perfect fit. If
feedback delay is set to zero, the resulting curve is much steeper than
the empirical one.

enables us to make rather accurate mathematical simulations of these curves,
however irregular they are (Figure 3).

Restrictive growth has been demonstrated in many different sorts and
levels of concept development during the school years (Klausmeier & Allen,
1978). Several authors reported S-shaped, that is, logistic growth in long-
term development of formal problem solving (Fischer & Pipp, 1984; Fischer
& Canfield, 1986). U- and M-shaped growth have been observed in many
developmental fields, such as conservation, face recognition, artistic develop-
ment, and so forth (Figure 4; overviews are presented in Strauss, 1982; Strauss
& Stavy, 1982; Bever, 1982). Such forms of growth can be modelled mathe-
matically by treating them as forms of competitive growth, for example, an
older strategy competing with a newer strategy for a limited domain of correct
application (van Geert, 1989).

There are two problems with the available empirical evidence, though.
One is that, since development is thought to be a regular process, data on
irregular developmental changes are rare. Such changes are considered
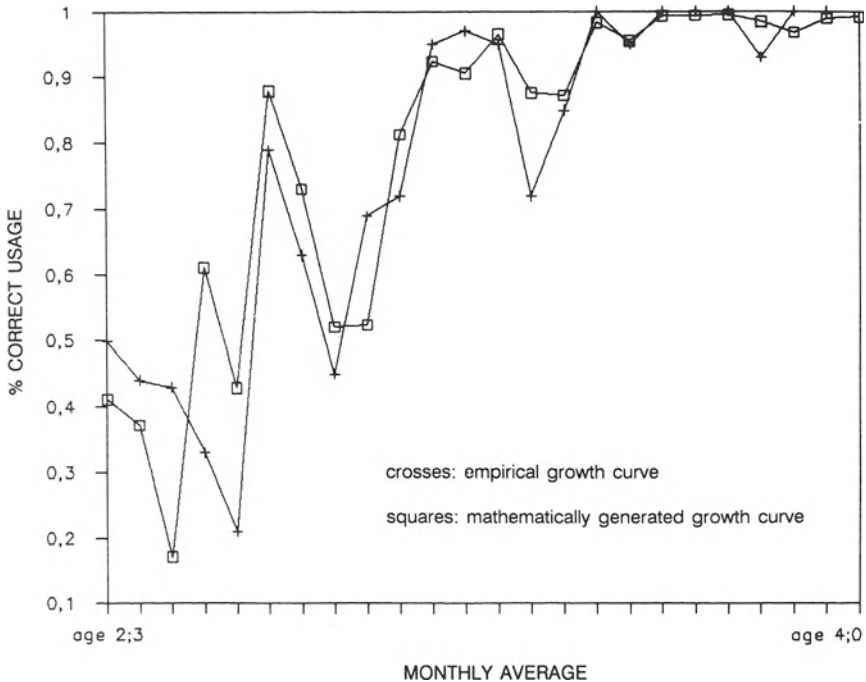
Figure 3. The growth of the present progressive construction in one child shows a very irregular pattern (after Brown, 1973). Nevertheless it is possible to make a fairly good mathematical simulation of the growth curve based on a dynamic systems model of logistic growth.

random, and thus not interesting to developmental inquiry (studies on regressions or U-shaped growth, and on early language development are partial exceptions). The other is that, since irregularity is eschewed and since individual growth data are pretty irregular, the vast majority of the data are group data, aimed at sifting out the random variation in individuals. However, growth models actually model individual growth processes. In view of the potential irregularities of the outcomes of such processes, group data may thus seriously obscure what actually happens.

The Prediction Horizon in Longitudinal Cognitive Growth

The logistic growth model, which is a powerful tool in many scientific disciplines, yields perfectly regular growth curves under two conditions. The first is that feedback from the main parameters (growth level and resource level or carrying capacity level) is undelayed. If some delay occurs — which is highly likely in most cognitive processes — regular growth happens only if the growth rate and the feedback delay are relatively low. Beyond a certain
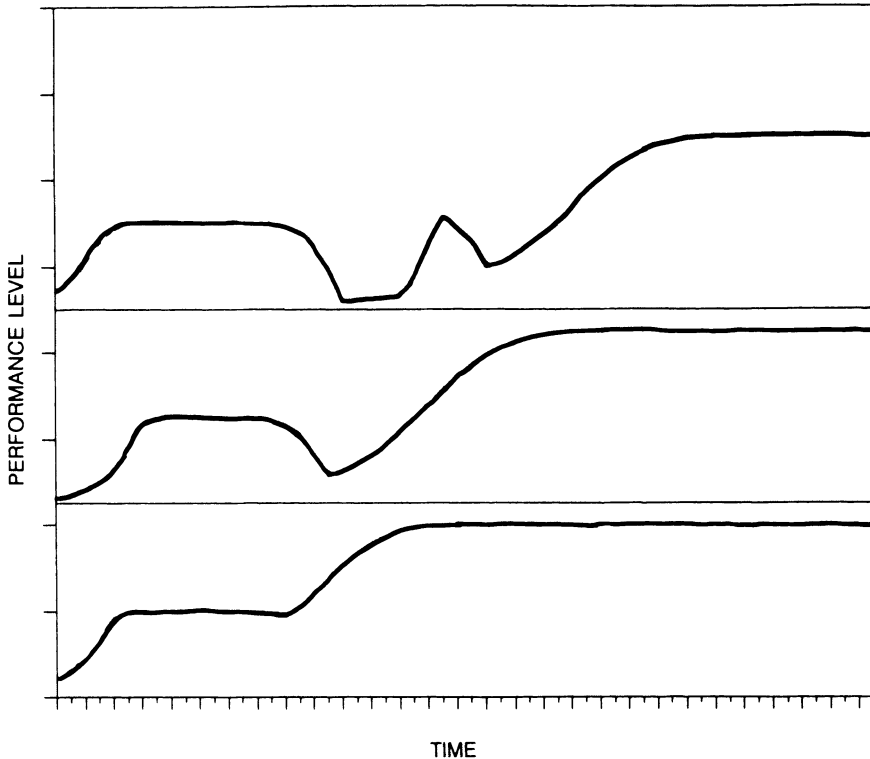
Figure 4. Three possible performance curves resulting from competitive growth among a 'strong' and a 'weak' cognitive strategy (e.g., a quantity-oriented versus a quality-oriented strategy to solve simple physics problems, after Strauss & Stavy, 1982). The curves have been generated by a mathematical model of two competitive cognitive growers.

growth rate and or feedback delay, growth first suddenly becomes approximate, beyond a further point it suddenly becomes oscillatory, and as growth rate and feedback delay are increasing, growth oscillates over a number of points which quickly achieve a purely chaotic appearance. Thus given two complete accounts of processes of change, one random, the other deterministic but with high growth rate and feedback delay, it is impossible to tell which one is random, and which one is not (Figure 5).

Unless one knows the trick, one would think both were completely random variations over time. If the growth process exceeds specific magnitudes of growth rate and feedback delay, prediction of a future growth level on the basis of linear extrapolation of state- or initial sequence-properties is simply impossible. It may seem rather irrelevant to be able to make a distinction between real and apparently random sequences of points, since for all
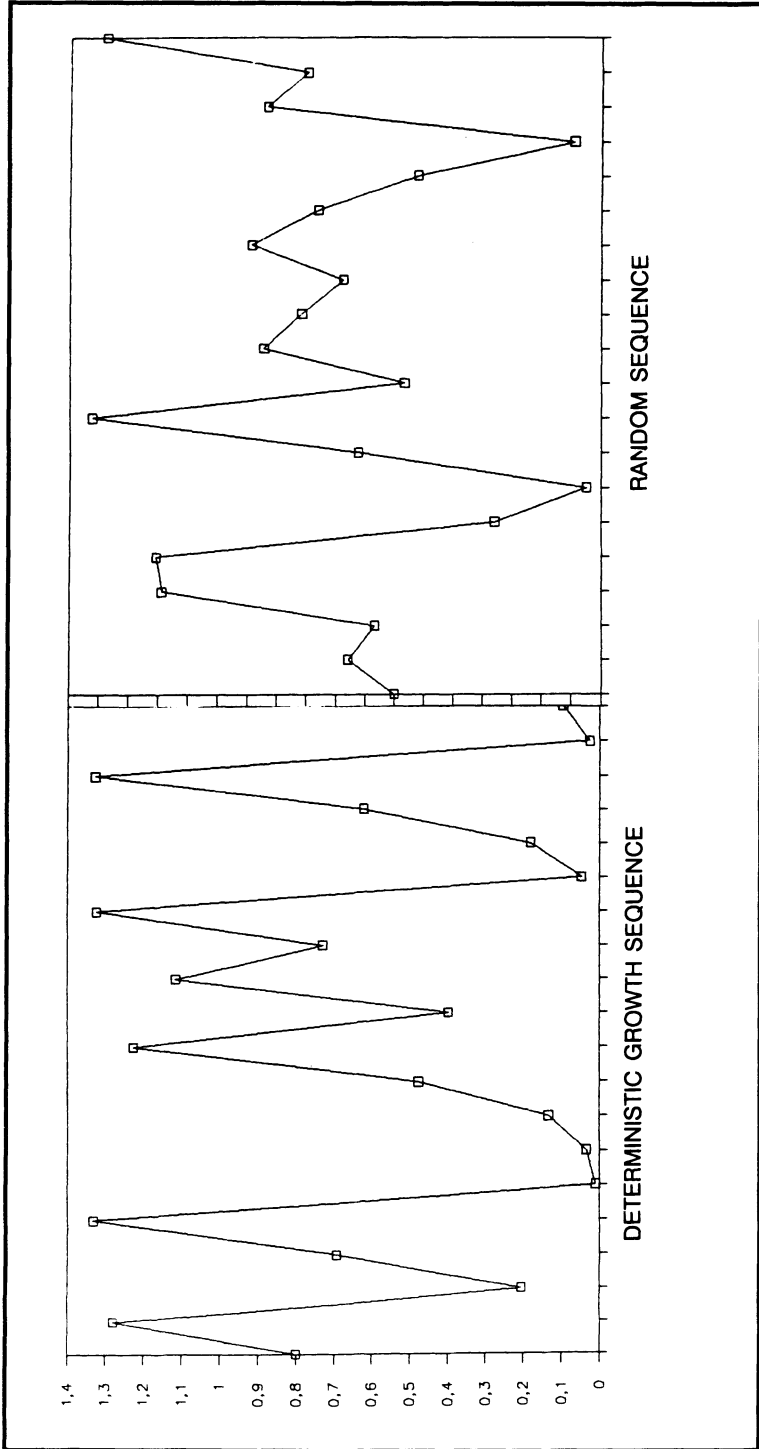
Figure 5. A logistic growth process with a very high growth rate (about 3.0) shows a sequence that cannot practically be distinguished from a real random sequence (right), although it is completely deterministic and initial-state-dependent.

practical purposes both sequences behave randomly. However, the apparently random sequence based on a very high growth rate differs from the actually random sequence in that the former will evolve towards a stable point if its carrying capacity is permitted to adapt, while the latter will never evolve towards a stable state. Since cognitive development is full of occasions where the support given (the carrying capacity) tends to adapt to the level achieved by a grower, the distinction between the real and the apparent random sequence is relevant enough.

If one is given a complete account of all relevant points in the sequence, the parameters can be computed rather easily, and thus a reliable mathematical reconstruction of the process can be given. The calculus generating the prediction is then actually the calculus modelling the growth process itself. Only by following growth mathematically, step by step, can predictions be generated. Now comes another considerable difficulty, namely, that predictions are possible only if the measurement of each state has been infinitely accurate. Very small errors, even far beyond the reach of the best measurement procedures in psychology, will result in very large differences in patterns after a sufficient number of iterations. Such magnifying effect of small inaccuracies — known as the Butterfly effect (Gleick, 1987) — makes all long-term prediction simply impossible (Figure 6).

Although this 'butterfly-effect' does not occur in all forms of developmental growth, it occurs in a sufficient number of cases to be taken seriously. For instance, the forms of regressive growth discussed further in this section show a strong sensitivity to very small perturbation factors in the vicinity of sensitive zones. Another example is the long-term adaptation of the carrying capacity of a cognitive environment to each of its individual growers. This adaptation, which is shown in Figure 6, is extremely sensitive to very small differences in initial state conditions. Both quasi-random curves from Figure 6 are fully deterministic growth curves differing from each other only in one control factor. The difference is only $1/10^{-10}$ of this factor, which is very small.

As long as growth rates and feedback delays stay within definite boundaries, growth processes are simple and quasi-linear. Characteristic for growth processes is the S-shape of so-called logistic or sigmoid growth, which has also been found in cognitive growth. Theorists who try to integrate structural with growth models consider the S-shaped growth function a proof for the fact that growth on the one hand and stepwise changes from structuralist models on the other hand can be reconciled (Fischer, 1980; van Geert, 1989). Any sufficiently dense measurement schedule will reveal enough of the properties of the growth sequence to make an adequate reconstruction possible. (By measurement schedule I understand the number and frequency of measurements of a variable relative to its growth. The denser a schedule, that is, the higher the measurement frequency, the more reliable is the resulting representation of the measured process.) However, if the growth and feed-
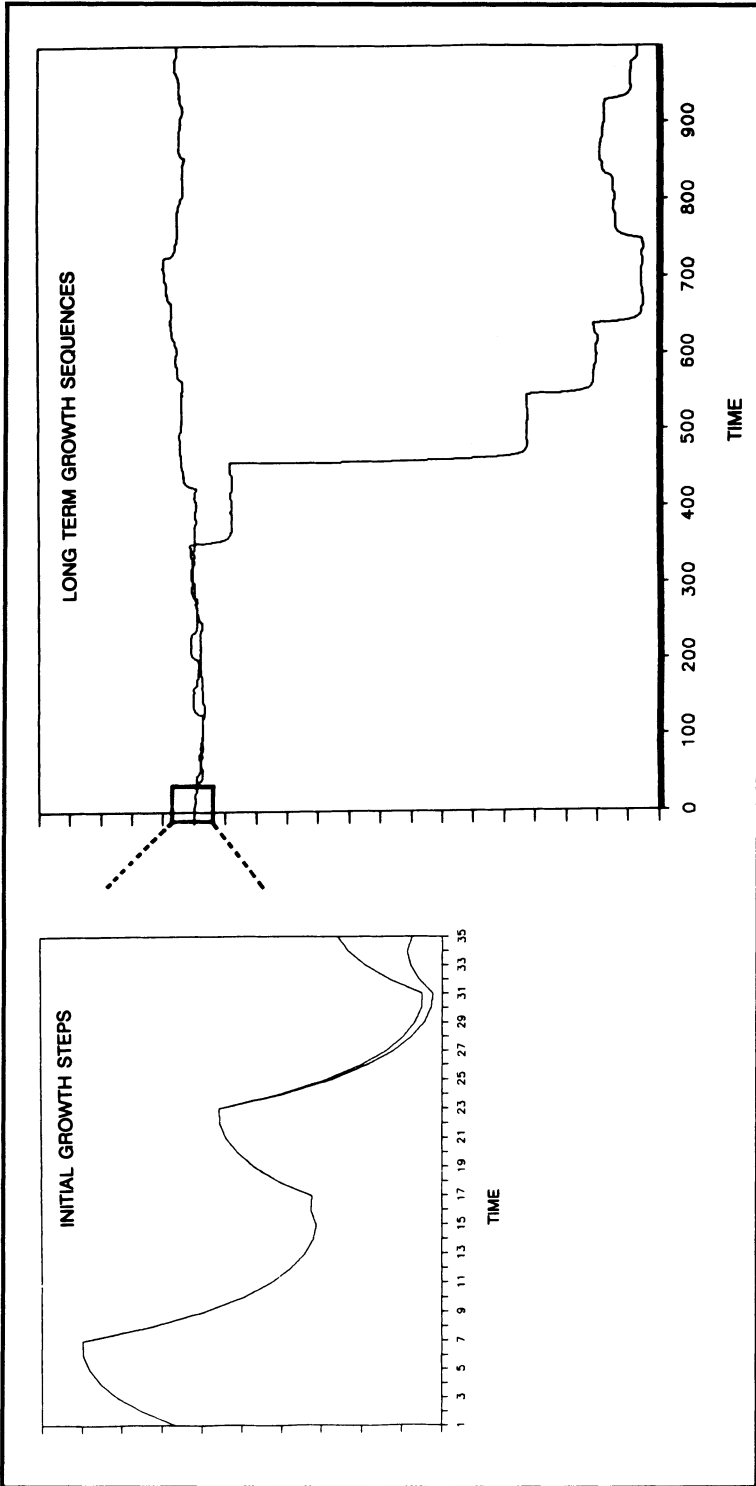
Figure 6. A very small initial state difference which is well within normal measurement error, evolves into dramatically different long-term growth curves. The first 30 steps in the growth sequences are practically identical, and could not be distinguished with normal measurement procedures (left).

back parameters cross the boundary where growth becomes approximate or oscillatory, the measurement schedule will become extremely important. Only if the frequency of measurement is about as high as the number of oscillations a reliable reconstruction of the process is possible. Where this frequency is considerably lower — which will be the standard case — the observed growth curve will be highly dependent on arbitrary measurement points (see Figure 7).

Neither adequate model building nor acceptable prediction will then be possible. Low measurement schedules are the rule rather than the exception in psychological research on long-term change. It is very time consuming, for instance, to make a reliable measurement of the size of the lexicon of child, unless one observed this child on a daily basis from the beginning of the word acquisition stage on. Next, psychological measurement interferes more strongly and fundamentally with the measured variable than in most other sciences. Administering a weekly test of mathematical problem solving ability will amount to providing extra time of exercising, and thus to changing the growth rate of the measured ability considerably. Thus for all fields in which a low to moderate measurement schedule is the obvious way of investigation, prediction is practically impossible for all growth processes which fall outside the confinements of regular logistic or restricted growth. It is highly likely that psychological growth is full of such phenomena, but we have not looked at them for want of theories explaining the regularity, if not determinacy, of such seemingly chaotic processes.

Thus far the unpredictable processes were pretty irregular. Although irregularity is certainly not always — or maybe generally not — the effect of random 'noise' added upon a process, it still seems rather uninteresting from a developmental point of view. Developmentalists have traditionally been rather teleological thinkers, and processes ending in a 'steady' state that is actually a sort of random oscillation, are not what should be expected from an interesting developmental end state. Several authors have studied processes of cognitive growth which seem to head for a respectable sort of end state but which do so via regression, that is, via temporary fallbacks that may eventually amount to temporary extinction of an earlier achievement (the so-called U-shaped growth phenomena mentioned earlier). A mathematical model of such U-shaped processes has been presented elsewhere (van Geert, 1989) which is based on the theoretical assumption that these U-shaped phenomena are actually the effect of competition among cognitive growers (e.g., alternative strategies for solving a problem, Strauss & Stavy, 1982). Such processes are very sensitive to initial conditions. Small initial state differences end in massive differences after some time, and this is so for deterministic and noise-fed cases alike (see Figure 8).

It should be noted that the noise added to each state is actually very small and well beyond the sort of measurement error that normally occurs in
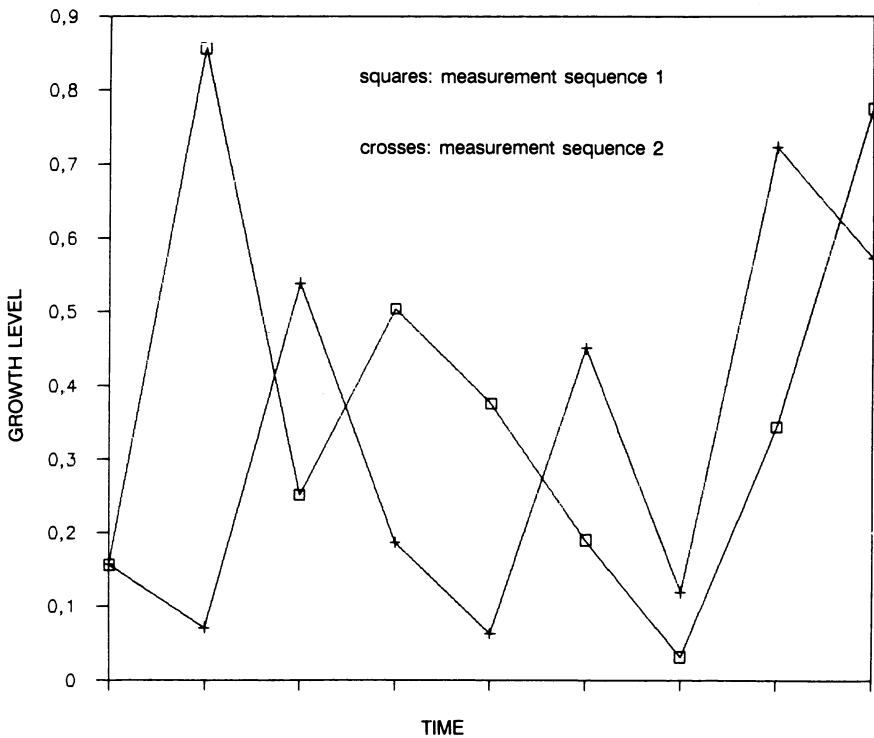
Figure 7. Strongly oscillating and irregular growth processes, which occur, for instance, in early syntactic growth, yield completely different growth curves if measurements are sparse and measurement sequences not completely in phase.

psychological data. So it follows that the fate of such competitive growers is simply unpredictable over longer periods of time. This would not be much of a problem if the period over which one may predict reliably were developmentally relevant. However, the problem is that only the first steps of the transitory phase of growth are predictable, while the steady state, that is, the end state is not predictable. The potential end states are, in general, rather strongly different, for example, a number are about zero, others about the maximum, with only very few cases in between. Thus the likelihood that a child will reacquire a strategy proficiently used earlier is about .5, or .3; and the probability that the child will never use this strategy again is about .5, or .7. Note that this prediction is based on an almost divine knowledge of the process parameters and the process mechanism (that is, it is the effect of simulated processes all of whose details we know). What may be expected then from prediction based on a less than optimal knowledge of the mechanism, and a more than extremely small measurement error?
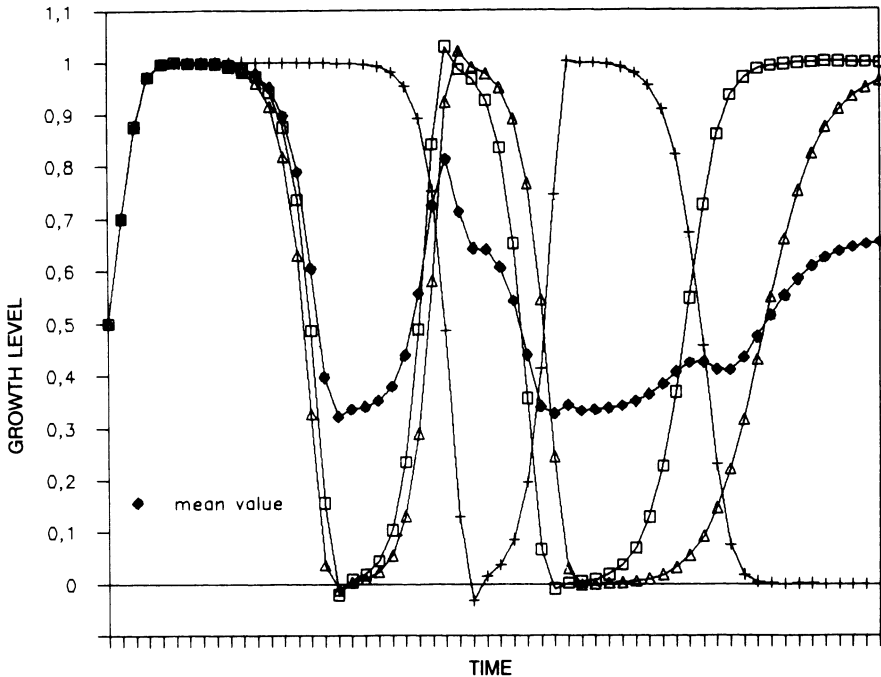
Figure 8. Very small random fluctuations in a competitive growth
dynamics result in completely different end states (either minimum
or maximum end level); the curve of the mean values is completely
uninformative; the increase in variance of the end states is not due
to large intrinsic differences between the growers, but rather to the
additive effect of small perturbations , which are effective only in the
vicinity of the maximum and minimum zones.

Some Consequences for Psychological Research

For a long time past psychologists interested in long-term change have
eschewed irregularity. They considered it an effect of random noise and
measurement error. Nature was supposed to reveal its truths in its
regularities rather than its irregularities. Thus they sought for simple curves
and distributions. Besides, psychologists like most other scientists believed in
the essential linearity of causation: small causes, small effects, and large
causes, large effects. Finally they believed in isotropy, just as most other
scientists did: a small or large factor here (or now) is a small or large factor
anywhere (or anytime). The canons of psychological research follow from
these assumptions: look for the smooth processes, and do so by studying
populations, such that individual random noise will be filtered out. However,
there seems to be a very significant subclass of psychological growth

phenomena that do not obey the general rules upon which methodological canonicity has been built. What methodological implications should the discovery of these processes have?

The first consequence is that psychologists should put more of their efforts into single-case longitudinal studies (taking as many single cases as possible). Only those studies reveal the dynamics of the process, and the eventual irregularity which is an essential aspect of change rather than an ephemeral disruption. One should, of course, select variables that can be measured as reliably as possible, that is, that are not too dependent on additional factors (fatigue, motivation, etc.). The second is that the measurement schedule should be as dense as the measured phenomena permit. Certainly not all psychological growth phenomena are insensitive to high-frequency-measurement. That means that we should concentrate on those phenomena that are rather robust in the face of observation. Third, beware of statistics if they are meant to flatten the individual differences in order to find the 'real and pure underlying process', because the processes revealed after the methodological ironing are, in general, only the artifacts of a Newtonian worldview. Finally, in building our models we should try to capture the general dynamic properties of the phenomena, not the actual events, since those are only the arbitrary expressions of the underlying mechanisms, and highly sensitive to small variations in the parameters. We have seen that even a 'divine' understanding of a process does not warrant prediction over sufficiently long intervals. These problems with predicting long-term effects of processes, however, do not argue against predictability on the population level. Difficult as it is to predict the individual effect of some measurement taken (such as information campaigns against smoking), we know that the population effect is rather robust. It is, of course, the population effect that makes the efforts of psychologists and social interventionists, in general, worthwhile.

References

Bever, T. G. (1982). *Regressions in mental development: basic phenomena and theories*. Hillsdale, NJ: Lawrence Erlbaum.

Brown, R. (1973). *A first Language. The early stages*. London: Allen & Unwin.

Dromi, E. (1986). The one-word period as a stage in language development: quantitative and qualitative accounts. In I. Levin (Ed.), *Stage and structure: reopening the debate* (pp. 220-245). Norwood, NJ: Ablex.

Fischer, K. W. (1980). A theory of cognitive development: The control and construction of hierarchies of skills. *Psychological Review, 87*, 477-531.

Fischer, K. W., & Canfield, R. L. (1986). The ambiguity of stage and structure in behavior: person and environment in the development of psychological structures. In I. Levin (Ed.), *Stage and Structure. Reopening the debate* (pp. 246-267). Norwood, NJ: Ablex.

Fischer, K. E., & Pipp, S. L. (1984). Processes of cognitive development: Optimal level and skill acquisition. In R. J. Sternberg (Ed.), *Mechanisms of cognitive development* (pp. 45-80). New York: Freeman.

Geert, P. van (1989). A dynamic systems model of cognitive and language growth. University of Groningen: unpublished paper (submitted for publication).

Gleick, J. (1987). *Chaos.* New York: Penguin Books.

Groot, A. D. de (1960). *Methodologie.* Den Haag: Mouton.

Kerlinger, F. N. (1965). *Foundations of behavioral research.* New York: Holt, Rinehart & Winston.

Klausmeier, H. J., & Allen, P. S. (1978). *Cognitive development of children and youth: a longitudinal study.* New York: Academic Press.

Labov, W., & Labov, T. (1978). Learning the syntax of questions. In R. N. Campbell & P. T. Smith (Eds.), *Recent advances in the psychology of language: language development and mother-child interaction* (Vol. 2) (pp. 1-44). London: Plenum Press.

Nelson, K. (1985). *Making sense. The acquisition of shared meaning.* New York: Academic Press.

Strauss, S. (1982). *U-shaped behavioral growth.* New York: Academic Press.

Strauss, S., & Stavy, R. (1982). U-Shaped behavioral growth: Implications for theories of development. In W. W. Hartup (Ed.), *Review of Child Development Research* (pp. 547-599). Chicago: University of Chicago Press.

JOHN DEWEY'S RECONSTRUCTION OF THE REFLEX-ARC CONCEPT AND ITS RELEVANCE FOR BOWLBY'S ATTACHMENT THEORY

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and Marinus H. van IJzendoorn

SUMMARY: In "The Reflex Arc Concept in Psychology" (1986), John Dewey gives a reconstruction of the reflex arc concept which forms the starting point of a 'transactional paradigm' which is fundamental for all aspects of Dewey's work. In this article we start with a reconstruction of Dewey's paradigm. Next we show that Bowlby's attachment theory fits very well within such a transactional paradigm because it is based on ethology. A closer analysis, however, shows that the place of the mental in attachment theory is not completely consistent with the evolutionary assumptions of the transactional paradigm, and it is suggested that this problem can be solved from a Deweyan perspective.

Introduction

In 1896 John Dewey's "The Reflex Arc Concept in Psychology" was published (*Psychological Review*, 3, July 1896, 357-370; EW5:96-109[2]). This article has not only been considered a crucial step in the development of Dewey's thinking (cf. Bernstein, 1966, p. 15; Hahn, 1969, p. 27; Smith, 1973, p. 122; Sleeper, 1986, p. 21; Alexander, 1987, p. 41), but also an important step in the development of psychology (cf. Langfeld, 1943; Murphy, 1961, p. 27, 29; McKenzie, 1975, p. xiv-xv). In this article Dewey criticizes the way the physiological idea of the reflex arc is used in psychology. The alternative view of behavior and the explanation for behavior that Dewey develops, is almost unanimously considered to be the starting point for functionalism in psychology (cf. Titchener, 1898, p. 451; Langfeld, 1943, p. 148; Flugel, 1964, p. 194; Bernstein, 1966, p. 15; Phillips, 1971, p. 566; Eisenga, 1973, p. 102; Verbeek, 1977, p. 141; Leahey, 1987, p. 270-271).

We shall draw on this article to introduce Dewey's position and give an evaluation of its topicality. We shall then confront Dewey's ideas with some aspects of John Bowlby's attachment theory. It should become apparent that Dewey has formulated the paradigm within which attachment theory can be placed. Looking at attachment theory from a Deweyan perspective enables

one to indicate which elements in the theoretical framework of attachment theory need further elaboration and how this might best be approached.

The Reflex Arc Concept in Psychology and Dewey's Critique

By the end of the nineteenth century psychologists were convinced that it was possible to overcome the dualistic presuppostions of associationist psychology. Physiology had provided the *structural* unit of afferent nerves, central nervous system and efferent nerves. Psychologists transposed this unit of *structure* into a unit of *function* of peripheral stimulus, central processing and motor response. This 'reflex arc' model was thought sufficient to explain human behavior (cf. Smith, 1973). The model resolved both the matter-form dualism ('sensation' and 'idea') of associationist psychology (by locating sensations and ideas in *one* process), and the body-mind dualism (by taking the mind to be the processes in the brain). Human behavior could now be explained from the physical, anatomical structure which meant that no essential distinction between animal and human behavior was to be made. The model was, therefore, in agreement with the theory of evolution.

In his article Dewey expresses the opinion that by using the idea of the reflex arc concept in psychology "the principles of explanation and classification which the reflex arc idea has replaced (...) are not sufficiently displaced" (EW5:96). According to Dewey "(t)he older dualism between sensation and idea is repeated in the current dualism of peripheral and central structures and functions; the older dualism of body and soul finds a distinct echo in the current dualism between stimulus and response." (EW5:96). This is caused by the fact that the sensori-motor circuit or system is interpreted from "preconceived and preformulated ideas of rigid distinctions between sensations, thoughts and acts." (EW5:97). As a result psychological theory-building starts with "disjointed parts" and must then attempt to explain how the separate parts work together or interact. In Dewey's opinion attempts to explain the interaction either by introducing an "extra-experimental soul" or by giving an explanation in terms of "mechanical push and pull" (EW5:100), must be refuted because they contradict the biological-evolutionary points of departure which form the basic assumptions both of physiological psychology and of Dewey himself.[3]

Because the theory of evolution "include(s) man under the same generalization with other facts of nature" (EW1:210), an ontological dualism that presupposes two substances (mind and body) is impossible. This means

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[3] Although functionalism can better be viewed as a line of reasoning than a real school of thought (see, e.g., Leahey, 1987, p. 278; Verbeek, 1977, p. 126; Eisenga, 1973, p. 101), one of the essential characteristics of this line of reasoning is that it bases psychology on evolutionary principles (both Darwininan and Lamarckian). An elaborate account of functionalism can be found in Eisenga, 1973, especially Chapter 5.

that an "extra-experimental soul" does not fit into a physiologically based psychological theory.

Dewey also rejects the idea of "mechanical push and pull," because it presupposes an organism at rest that can only be activated by stimuli from the environment. This notion of an isolated, autonomous organism is also contradictory to the theory of evolution. "The idea of environment is a necessity to the idea of organisms, and with the conception of environment comes the impossibility of considering psychical life as an individual, isolated thing developing in a vacuum." (EW1:56). Organism and environment are not from the outset two autonomous entities. "Only by analysis and selective abstraction can we differentiate the actual occurrence into two factors, one called organism and the other, environment." (LW5:220). This implies that the organism cannot be considered as an organism-at-rest, having a completely rested, neutral and unpreoccupied status. "(T)he *state* of the organism is one of *action* which is continuous" (LW5:223). It is against this background that Dewey reconstructs the reflex arc concept.

Co-ordination

An adequate psychological use of the reflex arc idea should not start with disjointed parts, but has to begin with an organic unity. "What is wanted is that sensory stimulus, central connections and motor responses shall be viewed ... as divisions of labor, functioning factors, within the single concrete whole, now designated the reflex arc" (EW5:97). "The process *all the way around* is assumed to be the unit" Dewey writes (in a letter to Angell; see Coughlan, 1975, p. 139). According to Dewey "this reality may most conveniently be termed co-ordination." (EW5:97).

Dewey gives a reinterpretation of "the familiar child-candle instance" from James' *Principles of Psychology* to demonstrate how he is using the term co-ordination. "The ordinary interpretation would say the sensation of light is the stimulus to the grasping as a response, the burn resulting is a stimulus to withdrawing the hand as response and so on." (EW5:97). But, from a psychological point of view, this analysis is inadequate. "Upon analysis, we find that we begin not with a sensory stimulus, but with a sensori-motor co-ordination, the optical-ocular, and that in a certain sense it is the movement of body, head and eye muscles determining the quality of what is experienced. In other words, the real beginning is with the act of seeing; it is looking, and not a sensation of light." (EW5:97). On the basis of this analysis Dewey concludes that "both sensation and movement lie inside, not outside the act" (EW5:98). Dewey is also using the term co-ordination in a more encompassing sense. With the stimulus-response analysis the whole process of seeing light — reaching for it — burning the hand — withdrawing the hand, can only be characterized as "the replacing of one sort of experience by another" (EW5:99). Only a fragmentary account of the state of affairs can be

given. Dewey, however, is able to show the connection between these sequential acts using the concept of co-ordination (see also EW5:99).

The Status of the Mental

It may seem that the concept of co-ordination must be considered a mental capacity because of the way in which Dewey uses the term. Such an assumption would imply that the mental retains a central position in Dewey's philosophy. This is, however, not the case. Dewey wants to do two things. On the one hand he wants to formulate a monistic psychology, because in a psychology consistent with the principles of evolutionary theory there can be no place for an ontological dualism between mind and matter. On the other hand Dewey wants to incorporate the mental into his psychology. The only way to solve this is to show the continuity between (primary) physiological processes and (secondary) mental functions. Dewey provides two arguments which sustain this conclusion. Firstly, he shows that the principle of co-ordination can be applied both to the most fundamental physiological level and to the more complex mental level. Secondly, he shows how the development from the fundamental (i.e., physiological) processes of co-ordination to the more differentiated (i.e., mental) processes of co-ordination should be conceived.

Co-ordination is primarily a physiological process. Dewey typifies the 'primary quality' of the organism as "movement as self sustaining through selection and assimilation of environment" (EW5:304). Within the organism a "specialization of labor" takes place with a "consequent need of interrelating or co-ordinating structure". That co-ordinating structure is the nervous system. "(T)he nervous system has one primary inclusive function: co-ordination of specialized activities to a common end." (EW5:308). In the process of evolutionary development Dewey distinguishes between an "increasing range of co-ordination," "co-ordination of movements with each other" and "co-ordination of movements with sensory activities." Within this latter modus of co-ordination, Dewey distinguishes between "(a) reflex action, (b) instinct, (c) formed habit, (d) deliberative activity" (EW5:307)[4]. Where Dewey uses the term "deliberative," mental capacities (i.e., thinking) are first introduced. Thought plays no role at the level of 'reflex action' and 'instinct' and this is also the case with 'formed habits'. "Thought arises in conflict of activities, through the need of striking a balance, that is, of discovering the course of action which will reconcile a number of conflicting minor activities"

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[4] Dewey uses the principle of co-ordination also in an even more extensive sense. In his course on Educational Psychology (1896) he distinguishes stages in child development; each stage is characterized by its own 'co-ordinatory task' (EW5:310-311). At the level of adolescence Dewey speaks of "The Co-ordination of One's Activity as a Whole with That of Others in Society" (EW5:311).

(EW5:314). So Dewey gives thought a function in situations in which the continuity of action is at risk, in those situations in which it is not clear "what kind of responses the organism shall make" (Dewey, 1938, p. 107).

Dewey's Transactional Paradigm

Dewey typifies human behavior as a biological-physiological process of co-ordination, a continuing attunement of organism and environment to each other as a result of which both the organism and the environment change in order to realise a situation of optimal adaptation. The word 'optimal' might suggest that Dewey holds that it is possible to state which adaptation is absolutely optimal. Dewey, however, keeps telling us that both organism and environment change. So there cannot be one state of the organism that will always guarantee an optimal 'fit'. The activities of the organism bring about changes in the environment which in turn can disturb the state of equilibrium reached. 'Optimal' must be seen as strictly situational. In attachment theory a similar idea can be found in Hinde, who suggests that there is a range of behavioral strategies for adapting to different niches (cf. Hinde, 1982b).

According to Dewey, the process of co-ordination becomes increasingly complex and encompassing. Mental functions originate *within* this process and fulfil a function in the increasing refinement of the ever more complex and encompassing processes of co-ordination. Dewey does not place the mental alongside the physical (as had been the case in psychology before Dewey) but neither does he deny the mental (as would become the case in psychology after Dewey). Dewey's functionalism does not stop at just indicating the function of thinking in the process of (evolutionary) adaptation (a sort of functionalism that, e.g., can be found in Titchener, 1898). Dewey shows that thinking is one of the functions *of* the organism; a function that has developed in the ongoing process of 'interaction' just as, for example, can be said of the digestive system.

While psychology (and philosophy) before Dewey had drawn heavily on concepts such as 'mind' and 'consciousness', the introduction of evolutionary ideas leads to a shift of attention from 'mind' to the interaction of organism and environment. "The old center was mind ... The new center is indefinite interactions ..." is Dewey's summary of this 'Copernican Revolution' (cf. LW4:232).

As we have cited above: "Only by analysis and selective abstraction can we differentiate the actual occurrence into two factors, one called the organism and the other, environment." (LW5:220). Because of the fact that the term 'interaction' implies two separate entitites, Dewey, in his later works, prefers to use the term 'transaction' to denote the initial totality within which, from a certain perspective and for certain reasons, organism and environment can be distinguished (cf. Dewey & Bentley, 1960, p. 122-124).

Bowlby's Attachment Theory: 'Behavior Systems' and 'Control Theory'

Bowlby's attachment theory is an ethological theory (Bowlby, 1988, p. 1) based upon "analytical biology and control theory, which together have elucidated the basic principles that underlie adaptive, goal-directed behavior" (Bowlby, 1982, p. 37). Ethology departs from the principles of evolutionary theory. Bowlby praises ethology for not only studying the morphological and physiological characteristics of animals from the Darwinian framework of adaptation-for-survival, but also their 'behavioral equipment' (cf. Bowlby, 1982, p. 54-55).

One of the central concepts in Bowlby's theory is the ethological concept 'behavior system'. A behavior system is a system postulated to explain behavior by thinking of more complex behavior as a compilation of, and cooperation between, more simple behaviors. The concept "is used in an explanatory sense to refer to systems postulated as controlling behavior" (Hinde, 1982b, p. 62). With the help of this concept, explanations of behavior are given in which there are "lower-level systems controlling behavioral acts," "control systems postulated to explain interrelations between the several types of behavior" and "higher-order control systems" (Hinde, 1982b, p. 66). Bowlby elaborates on this (hierarchically ordered) classification of behavior and behavior systems by differentiating (in increasing order of complexity) "reflexes," "fixed action patterns" and behavior in which "a fixed action pattern is combined with a simple sequence that is dependent on feedback from the environment" (Bowlby, 1982, p. 67). Bowlby notes that "just as there are many different types of behavioral systems so there are a number of different ways in which their activities can be co-ordinated" (Bowlby, 1982, p. 74). Various more or less 'elementary' behavioral acts are observed and are then placed in a coherent system. This systematization is based on the function that more elementary behavior performs in reaching a certain *goal*, and provides the means by which ethology tries to *explain* behavior.

While in the more elementary 'behavior systems', patterns of behavior are such that they go 'straight for the goal', in the case of more complex behaviors (and the corresponding more complex behavior systems) principles of 'control theory' are introduced. The main concepts of control theory are 'setting', 'set-goal' and 'feedback' (Bowlby, 1982, p. 43). In the case of, for example, a thermostat the set-goal is keeping the temperature at a certain level and the setting is the actual temperature that is wanted. While in the case of a thermostat the 'setting' has to be instituted by a human being, it is also possible to envisage a system which receives its setting from another system. Bowlby gives the example of automatic anti-aircraft guns which receive their information from a radar-screen (cf. Bowlby, 1982, p. 44). It is Bowlby's opinion that "this type of system is replicated in living organisms" (Bowlby, 1982, p. 44). Which behavior system must be activated in what way,

depends on the comparison between the current situation and the situation that is wanted; a comparison in which 'feedback' plays an important part. Bretherton (1985) pointed out that the attachment behavior system must not be seen as a system-at-rest that is switched on to reach the set-goal and is switched off when the set-goal is reached. According to Bretherton the attachment behavior system is always active in coordinating different sub-systems to reach and maintain the set-goal.

When control theory starts to play a role in the ethological explanation of behavior, cognitive capacities make their entry. It is evident that "ultimately 'goal-directed' behaviour must imply that the animal has some model or correlate of the goal situation before that situation is achieved, and the behavior is governed by the discrepancy between current and goal situations. (...) An internal correlate of the goal can, but need not, imply cognitive abilities on the part of the animal" (Hinde, 1982a, p. 76). According to Hinde the hesitation of ethologists to introduce cognitive powers in explaining "complex social behaviour of higher mammals" is understandable but inappropriate. This hesitation has led, unfortunately, to "the neglect of many interesting phenomena" (Hinde, 1982a, p. 76-78).

Bowlby assumes that behavior systems are adapted to the environment in which they originated because they are the product of the evolutionary process of variation and selection. This means that nowadays behavior systems can only function adequately in a situation that does not differ too much from the original 'environment of evolutionary adaptation' (cf. Bowlby, 1982, p. 47; Bowlby does not make clear what he means exactly by 'too much'). This implies that the organism must be able to recognize certain characteristics of (or characteristic patterns in) the environment. So, according to Bowlby, we must presuppose that "the individual organism has a copy of that pattern in its CNS and is structured to react in special kinds of ways when it perceives a matching pattern in the environment and in other kinds of ways when it perceives no such pattern" (Bowlby, 1982, p. 48). "(A)s well as having equipment that enables them to recognise special parts of their environment, members of all but the most primitive phyla are possessed of equipment that enables them to organise such information as they have about the world into schemata or maps" (Bowlby, 1982, p. 48). Bowlby prefers to speak of a 'working model' instead of a 'cognitive map', because the latter is a too static concept (cf. Bowlby, 1982, p. 80). The individual can, as it were, look at, change and adjust the model and can use the model as a reference to plan his actions. The notion of "a model in the brain is that it constitutes a toy that is yet a tool, an imitation world, which we can manipulate in the way that will suit us best, and so find out how to manipulate the real world, which it is supposed to represent" (Bowlby, 1982, p. 80). By the use of language we can exchange our working models so that we need not re-invent the wheel over and over again.

So in Bowlby's elaboration of attachment theory two cognitive abilities are postulated: "(a) a means of receiving and storing instructions regarding the set-goal, and (b) a means of comparing the effects of performance with instruction and changing performance of it" (Bowlby, 1982, p. 70).

Discussion

A comparison of Dewey's ideas with attachment theory shows that both depart from evolutionary theory. In both cases we also find a similar strategy for the explanation of behavior: complex behavior is explained by seeing it as a co-ordinated composition of more elementary behavior, and elementary behavior is in the end viewed as biological-physiological. From this we can conclude that the 'paradigm' for explaining behavior Dewey formulated around the turn of the century has, in the last two or three decades, led to a flourishing research-program, especially since the introduction of ethology into psychology.[5]

Dewey's significance lies in the fact that he has shown that an elaboration of (monistic) evolutionary principles need not imply that there is no place for mental 'faculties' in the explanation of human behavior. On the contrary: the mental must be considered *as real as* the material (the physiological). There is no argument for viewing certain 'results' of the process of evolutionary development (as, e.g., the mental, but also language and culture) as having a lower status or as being less relevant to the process of adaptation and change than other 'results' (as, e.g., reflexes or the digestive system).

When we apply Dewey's elaboration of the evolutionary principles to attachment theory, we are led to the conclusion that the latter is unclear on the issue of the status of the mental. Whilst attachment theory considers the physiological-biological level to be real, it is often the case that as soon as mental functions are introduced in explaining behavior, recourse is made to the level of *explanation*. Ethology wants to *explain* behavior by *postulating* behavior systems. Hinde describes an attachment behavior system as "a system postulated as controlling the several types of attachment behavior" (Hinde, 1982b, p. 64). To be able to explain more 'complex' behavior, the principles of control theory are applied. Exactly at the point at which those principles are introduced, mental capacities are *presupposed* in order to arrive at an adequate explanation.

In attachment theory, then, there is a difference between the ways in which the physiological and the mental level are discussed. This difference can be partly traced to a difference between description and explanation. But

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[5] In theoretical psychology there is also a reassessment of Dewey's ideas. See, for example, Natsoulas (1983) who discusses the way in which the concepts 'conscious' and 'consciousness' should be used and elaborates explicitly on ideas Dewey formulated as early as 1906 (see Dewey, 1906).

besides the fact that, from a philosophical point of view, the validity of such a rigid distinction between description and (theoretical) explanation can be questioned, Dewey has shown us that in this case there is no need to make any such distinction. Moreover, any such distinction is contrary to the evolutionary principles subscribed to by attachment theory.

We conclude that further research needs to be done into the ways in which the mental is used in attachment theory. We think that a Deweyan perspective might be helpful in realizing a more adequate conceptualization of the mental. A reconceptualization is not only necessary for theoretical reasons, that is, to get a theory that is consistent with its basic evolutionary assumptions. But also, to the extent to which attachment theory ascribes to mental capacities a direct influence on behavior, a more adequate and consistent way of viewing these capacities can contribute to the practical relevance of attachment theory.

References

Alexander, Th. M. (1987). *John Dewey's Theory of Art, Experience and Nature: The Horizons of Feeling*. Albany: State University of New York Press.

Bernstein, R. J. (1966). *John Dewey*. New York: Washington Square Press.

Bowlby, J. (1982). *Attachment and Loss. Volume I: Attachment*, (2nd ed.). New York: Basic Books.

Bowlby, J. (1988). *A Secure Base. Clinical Applications of Attachment Theory*. London: Routledge.

Bretherton, I. (1985). Attachment Theory: Retrospect and Prospect. In I. Bretherton & E. Waters (Eds.), Growing points of attachment theory and research. *Monographs of the Society for the Research of Child Development, 50*, (1-2, Serial No. 209), 3-37.

Coughlan, N. (1973). *Young John Dewey. An Essay in American Intellectual History*. Chicago and London: The University of Chicago Press.

Dewey, J. (1884). The New Psychology. EW1:48-60.

Dewey, J. (1887). Ethics and Physical Science. EW1:205-226.

Dewey, J. (1896). The Reflex Arc Concept in Psychology. EW5:96-110.

Dewey, J. (1896). Educational Psychology: Syllabus of a Course of Twelve Lecture-Studies. EW5:304-327.

Dewey, J. (1906). The Terms 'Conscious' and 'Consciousness'. MW3:79-82.

Dewey, J. (1929). *The Quest for Certainty*. LW4.

Dewey, J. (1930). Conduct and Experience. LW5:218-235.

Dewey, J. (1938). *Logic. The Theory of Inquiry*. New York: Henry Holt.

Dewey, J. & Bentley, A. F. (1960/1949). *Knowing and the Known*. Boston: Beacon Press.

Eisenga, L. K. A. (1973). *Structuralisme, functionalisme en behaviorisme. De gedachte van een behavioristische revolutie in de psychologie.* (Structuralism, functionalism, and behaviorism. The idea of a behavioristic revolution in psychology). Amsterdam: Vrije Universiteit (diss.).

Flugel, J. C. (1964). *A Hundred Years of Psychology, 1833-1933.* New York: Basic Books.

Hahn, L. E. (1969). Introduction. From Intuitionalism to Absolutism. In J. A. Boydston (Ed.), *John Dewey. The Early Works, 1882-1898. Volume 1: 1882-1888*, (pp. vii-xxi). Carbondale & Edwardsville: Southern Illinois University Press.

Hinde, R.A. (1982a). *Ethology. Its nature and relations with other sciences.* London: Fontana.

Hinde, R. A. (1982b). Attachment: Some Conceptual and Biological Issues. In C. M. Parkes & J. Stevenson-Hinde (Eds.), *The Place of Attachment in Human Behavior*, (pp. 60-76). New York: Basic Books.

Langfeld, H. S. (1943). Jubilee of the Psychological Review: Fifty Volumes of the Psychological Review. *Psychological Review, 50*, 143-155.

Leahey, Th. H. (1987). *A History of Psychology. Main currents in psychological thought.* (2nd ed.). Englewood Cliffs, NJ: Prentice-Hall.

McKenzie, W. R. (1975). Introduction. Toward Unity of Thought and Action. In J. A. Boydston (Ed.), *John Dewey. The Early Works, 1882-1989. Volume 5: 1895-1898*, (pp. ix-xvi). Carbondale & Edwardsville: Southern Illinois University Press.

Murphy, G. (1961). "Some Reflections on John Dewey's Psychology". In *University of Colorado Studies.* Series in Philosophy, No. 2, pp. 26-34. Boulder: University of Colorado Press.

Natsoulas, Th. (1983). Concepts of Consciousness. *The Journal of Mind and Behaviour, 4(1)*, 13-59.

Phillips, D. C. (1971). James, Dewey, and the Reflex Arc. *Journal of the History of Ideas, 32*, 555-568.

Sleeper, R.W. (1986). *The Necessity of Pragmatism. John Dewey's Conception of Philosophy.* New Haven and London: Yale University Press.

Smith, A. K. (1973). Dewey's Transition Piece: The Reflex Arc Paper. *Tulane Studies in Philosophy, XXII*, 122-141.

Titchener, E. B. (1898). "The Postulates of a Structural Psychology. *The Psychological Review, 5*, 449-465.

Verbeek, Th. (1977). *Inleiding tot de geschiedenis van de psychologie.* (Introduction to the history of psychology.) Utrecht/Antwerpen: Het Spectrum.

TWO CONCEPTIONS OF STAGE STRUCTURE AND THE PROBLEM OF NOVELTY IN DEVELOPMENT

Jan Boom

SUMMARY: Cognitive development can be construed in terms of conceptual or operational structures. The notion of 'operational structures' is used to refer to Piaget's dynamic form of structuralism and implies an autonomous, universal development through self-regulative processes. This position was criticized because the social aspects of development were underestimated. If development is construed in terms of 'conceptual structures', then 'structure' stands for coherence in the organization of meanings and development need not be autonomous nor self-regulative: the cultural environment has an important role to play. Although the second position thus seems promising, the worth of a conceptualization in terms of operational structures must be stressed because in this way the learning paradox can be avoided. This paradox states that it is impossible for a subject to formulate a hypothesis that does contain structures of the higher stage while being in the lower stage. Therefore, a relevant hypothesis about a new way of thinking cannot be tested and consequently never be 'learned'. With the conceptualization of development in terms of 'operational structures' these objections can be neutralized because a new structure can, to a certain extent, be available for the subject in his or her actions.

Genetic structuralism is a still influential paradigm inside and even outside developmental psychology. Within this larger framework cognitive development is sometimes construed as the development of conceptual structures and sometimes as the development of operational structures. Both designations can be found, sometimes even within one theory (e.g., Kohlberg's [1984] theory of moral development). Although there is a growing tendency to view development as the development of conceptual structures (and for good reasons) I will argue that in order to avoid the constructivist fallacy the other interpretation should not be given up all too easily.

Two Conceptions

The notion of 'operational structures' is used here to refer to Piaget's dynamic form of structuralism. 'Dynamic' because the structures themselves can and do change in development. This possibility of change is dependent upon the fact that structures are structures of transformations. According to Piaget a structure "is the system of what a subject 'can do' and not of what he says of it or thinks about" (Piaget in Piattelli-Palmarini, 1980, p. 282). Operations are internalized actions and are, therefore, also to be understood as

active transformations (which a subject can perform mentally). Variations that can occur in the actual use of operations can give rise to change in the structure. Hence for Piaget, the dynamic character of structuralism and the fact that the entities which are structured are operations are two sides of the same coin (e.g., Piaget 1970). Notice that there are also two levels of speaking about transformations.

Another key assumption in Piaget's structuralism, and of structuralism in general, is that certain properties of a system as a whole (the structures) are preserved when transformations are applied on the elements. For example, when using the addition-operation on the class of integers the outcome also belongs to that class. In fact, most of the possible operations on the class of integers together have such distinctive features and these features retain their validity, irrespective of the combination of integers involved. So there are endless possibilities for applying a certain set of operations while still retaining specific structural properties. This is why we can speak of stages which unify a relatively large range of cognitive functioning. For moral development this conceptualization could lead to the following: in all moral judgments given by a subject in a certain age period, only a few operations can be used and only in a specific constellation. Such an operation could be taking the perspective of another person involved but not being able to co-ordinate it with one's own perspective. These possibilities then result in a specific kind of moral judgment which we conveniently refer to as a certain stage in moral development.

These two characteristics of structures, put forward by Piaget, seem to involve a tension: on the one hand, structures can change because they are structures of transformations, but on the other hand structures remain the same through operations (transformations). However, if we see development as the development of conceptual structures then this tension seems absent. I will go further into this problem but let me first introduce the other interpretation of structures.

Due to a growing awareness of the importance of socially constituted, meaningful content in psychology, genetic structuralism is more and more taken to be concerned with the development of 'conceptual structures'. Central in this form of structuralism are the ways of seeing a cognitive domain under consideration. A moral stage could for example, be defined by the way in which morality is conceptualized and by the criteria a subject holds for valid in moral judgments (cf. van Haaften, Korthals, Widdershoven, Mul, & Snik, 1986). Structure now stands for relations between certain concepts and specifically for coherence in the organization of meanings. For example; in the (pre-conventional) stage 2 in moral development, moral norms are seen as psychological expectations with no fixed value. Structure here refers to the basic ideas (a sort of foundation, or framework) which furnishes consistency and coherence for the thinking of a subject.

Although it is often assumed (either explicitly or implicitly) that both designations of stage development are two sides of the same coin, I think both conceptualizations should be distinguished more carefully. They have different implications for a theory of development and imply different presuppositions (especially in the case of moral development).

Piaget's notion of operational structures implies a strictly autonomous, universal development through self-regulative processes (provided some environmental minimum conditions are met). The notion of self-regulation is used by Piaget in order to avoid the problems that would arise if structures were merely contingent or, in the other extreme case, if they were completely preformed. He tries to steer his way between empiricism and rationalism (in the nativistic variety). This position, which Kohlberg also subscribed to, has, however, given rise to severe criticisms. In the first place, because the social aspects of development are not adequately represented in Piaget's (latter) work (e.g., Miller, 1986) and in the second place, because the influence of cultural transmission of ideas is disregarded. This second kind of objection can be found among philosophers of education (e.g., Peters, 1974) but also among developmental psychologists like Vygotsky (1986).

The notion of the development of conceptual-structures is less prone to these objections because here 'structures' imply radically different assumptions. 'Concepts' presuppose a language community and conceptual structures refer to coherence and consistency in meaningful content. The ideas of a subject are structured by a foundational idea. In this view on stage-structure there is an interplay between structures that are available in the culture a subject is part of, and his or her own structuring of experience. Development need not be completely autonomous nor self-regulative. On the contrary, the cultural environment (including pedagogic interventions) has to play an important role in development, but not necessarily the only role.

For the study of moral development, therefore, the second position in which structure is conceived of as 'conceptual structures' seems to be more promising. And it is also consistent with a more general trend in developmental theory which tries to correct Piaget in accounting for the role of grown-ups in the development of children (e.g., Elbers, 1988). I would like, however, to point out the worth of a conceptualization of stage development in terms of operational structures.

The Novelty Problem

All theories using the concept 'developmental stages' have to face two problems. One is giving an account of novelty in development and the second is giving a justification for the claim that the later stage is better in some respect than the earlier stage (cf. van Haaften, in press). I will not address the second problem here and will limit myself to the question of novelty.

Piaget's account of novelty is the strongest we have in developmental theories. Any suggestion that all novelty in the child's thinking is induced by introducing it to new ideas only shifts the problem to the question how novelty can arise on the socio-cultural level. Therefore, we have a stronger position if the problem can be faced on the level of individuals.

Piaget sees development as a successive series of increasingly adequate structures that are acquired by learning activity of the child. This concept of learning has, however, given rise to a learning paradox that is known in two slightly different formulations. If learning is hypothesis formation and confirmation, and the thing to learn is a richer structure, then we have a problem. It is impossible for a subject to formulate a hypothesis that does contain structures of the higher stage while he or she is still in the lower stage, otherwise the higher stage would have been reached already. Therefore, he or she never can put relevant hypothesis about higher stage structures to test nor confirm them and consequently never 'learn' them. Fodor who has put this argument forcefully then pleads for a strict nativism: all structures are innately given (Fodor, in Piattelli-Palmarini, 1980).

Another formulation for the learning paradox was already given by Plato while discussing moral virtues: if we know something, we cannot learn it any more, because we already know it. If we do not know it then we can't learn it either, because we don't know what to look for or whether we have found it or not (cf. Flanagan, 1984; Hamlyn, 1978; Miller, 1986).

These two objections have been leveled against Piaget, but I would like to suggest that this is not in order. Here the confusion between the notions 'conceptual-structures' and 'operational-structures' plays a role. My thesis is that the conceptualization of development as 'development of operational structures' is not prone to these objections, only the other conceptualization is. Because of all the complications, and limited space I will only argue why I think Piaget's basic idea is tenable in at least some instances. My argument requires two steps: first, it must be shown that the tension mentioned before is essential and not fatal for Piaget's position and, in addition, it must be shown that if the tension is eliminated only by acknowledging conceptual structures, then development seems to be impossible.

In the first place, I think the learning paradox can be solved because the new structure is to a certain extent already available for the subject. In his or her actions a richer structure can be available as a possibility (and the same holds for operations). Let me give an example: a child that can add numbers can also do this repeatedly. So, in fact, what we would call multiplication is a possibility for a child that only knows the operation of addition. Even when the concept 'multiplication' is not yet available to the child the operation can be within reach. For this child, learning to multiply is not a question of hypothesis formation about something external, it is the result of a sort of reflection on what he or she already can do. Piaget refers to this process as

'reflective abstraction' (Piaget, 1977). According to Piaget this is a central mechanism for development. Basically this is a combination of a transference of an ability to a higher or more general level and a subsequent reorganization of the ability itself. It is a process in which 'form' (the structure of operations) can become 'content'. This idea of a dialectical relation between form and content is elaborated upon by Eckensberger (1986) for the context of moral development. A clear-cut example in the socio-moral domain is not easy to give. Imagine peers playing a competitive-game in which part of the game is trying to mislead each other. Suppose further that one child attributes to the other child the same abilities he would use himself and that both know that deceiving is possible. Then the first child anticipates the strategy of the other party by, in fact, not deceiving (hoping that the other party, by expecting to be misled, will choose the opposite of what is suggested). Up to here the operations involved are simply switching between the I- and you-perspectives. The next step, however, comes — and presumably only after considerable experience — when the first child realizes that his strategy (his anticipating) might be seen through and that in fact it does not matter what he tries to suggest to the other party. Now, with this insight, a new perspective comes into play: the perspective of an independent observer of the interaction between ego and alter who can now see the structure of the game. Being able to reflect the interaction between ego and alter is an important achievement in socio-moral development.

Conclusions

Although, according to several commentaries on Piaget's later work, the notion 'reflective abstraction' is interesting but not as clear as it should be, the only question that concerns us here is whether the learning paradox is solved or not. The paradox is solved if the notion of operations can bridge the gap between old and new, which I think it does. A relatively simple procedure (such as addition) can reveal patterns that are more complex (like multiplication) and it seems to me that these patterns can be abstracted from experience and reflected upon. In the other example the experience in actually alternating the perspectives (in concrete interactions), when reflected upon, eventually reveals the structure of the game, which implies a generalized, third party, or observer perspective.

The tension is still there but not in a problematic way. The mathematical properties of addition and subtraction are not lost and yet the structure is changed because they are encompassed in a larger structural framework.

Secondly, it should be clear by now why I think that, if we speak about conceptual structures, the learning paradox is not easily solved. If a stage (a few key concepts and their relations) is seen as a foundation of thinking and this foundation is seen as the logical condition for the judgements of an individual then it becomes impossible that some relevant aspects of a higher

stage structure are already contained in the lower stage. The operations are, according to this view dependent upon the concepts: you need the concept of number before you can count, while in Piaget's view repeated simple counting acts engender the scheme for counting and in the end the concept of number.

The distinction of the two conceptualizations of stage-structure is a question of logical and chronological priorities: if we assume that concepts are logically prior to operations, then they are also chronological prior in either the nativistic or the contextualistic sense. If, however, concepts are thought not to be logically prior to operations then operations, can have chronological priority over the concepts and then the constructivist option in which novelty is thought to be constructed by the subject is feasible. And this is precisely the claim of Piaget, if we interpret his notion of 'the whole' as roughly meaning the same as a concept. "In other words, the logical procedures or natural processes by which the whole is formed are primary, not the whole, which is consequent on the system's laws of composition, or the elements." (Piaget, 1970, p. 9).

What is needed is a developmental model in which the strong points of both conceptualizations can be combined. A recent proposal of Bickhard (1988) to do away with all of Piaget's structuralistic strands seems not necessary and, in fact, detrimental where moral development is concerned. Elsewhere I have made steps in the direction of integrating the two conceptualizations that I have contrasted here (Boom, 1989).

References

Bickhard, M. H. (1988). Piaget on variation and selection models: Structuralism, logical necessity, and interaction. *Human Development, 31*, 274-312.

Boom, J. (1989). *Structures and reflection in individual and collective development*. Paper presented at the '9th Tagung fur Entwicklungspsychologie' in Munich, Germany.

Eckensberger, L. (1986). Handlung, Konflikt und Reflexion: Zur Dialektik von Struktur und Inhalt im Moralischen Urteil. In W. Edelstein & G. Nunner-Winkler (Eds.), *Zur Bestimmung der Moral* (pp. 409-442). Frankfurt a/M: Suhrkamp.

Elbers, E. (1988). *Social context and the child's construction of knowledge*. Utrecht.

Flanagan, O. J. (1984). *The science of the mind.* Cambridge, MA: M.I.T. Press.

Haaften, A. W. van, Korthals, M., Widdershoven, G. A. M., Mul, J. de, & Snik, G. L. M. (1986). *Ontwikkelingsfilosofie.* (Developmental philosophy). Muiderberg: Coutinho.

Haaften, A. W. van (in press). The justification of conceptual development claims. *Journal of Philosophy of Education*.

Hamlyn, D. W. (1978). *Experience and the growth of understanding*. London: Routledge & Kegan Paul.

Kohlberg, L. (1984). *Essays in moral development. Vol. II: The psychology of moral development. The nature and validity of moral stages*. San Francisco: Harper & Row.

Miller, M. (1986). *Kollektive Lernprozesse. Studien zur Grundlegung einer soziologischen Lerntheorie*. Frankfurt a/M: Suhrkamp.

Peters, R. S. (1974). *Psychology and ethical development*. London: Allen & Unwin.

Piaget, J. (1970). *Structuralism*. New York: Harper & Row.

Piaget, J. (1977). *Recherches sur l'abstraction réfléchissante. Vol 2: L'abstraction de l'ordre des relations spatiales*. Paris: Presses Universitaires de France.

Piattelli-Palmarini, M. (1980). *Language and learning: The debate between Jean Piaget and Noam Chomsky*. Cambridge, MA: Harvard University Press.

Vygotsky, L. S. (1986). *Thought and language*. Cambridge, MA: M.I.T. Press.

THINKING OF EMOTIONS:
A SOCIO-COGNITIVE VIEW

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SUMMARY: Cognitive theories of emotion reject the classical polarity between rationality and emotionality and stress the cognitive underpinnings of emotions. The cognitive processes *preceding* the emotion, the so-called appraisals, have been studied extensively. In this paper the view is defended that not merely these cognitive antecedents, but the emotional experience as a whole is soaked through with cognitions. This does not imply that emotions can be reduced to cognitions. It is argued that emotions are structured, regulated and understood in a social meaning system. We possess a great deal of (often implicit) knowledge about emotions, for example, how they are caused, how they feel, what effects they have upon oneself or others, how they can be coped with. Thus, we know our emotions and they mean something to us. Two different views on emotion knowledge are discussed.

Introduction

As a rule, both scientific and common-sense views of emotions have been characterized by the antithesis emotion-rationality. According to Averill (1974), such a view is primarily based on cultural norms, attitudes and values and can be described by three different, though closely connected features: irrationality, involuntarity and instinctivity. Together these three characteristics make up what Averill has called psychophysiological symbolism.[2]

One consequence of psychophysiological symbolism is that cognition has been equated with rationality, reflection or deliberation and considered as a counterpart of emotion. An illustration of such confusions considering the conceptualization of cognition can be found in the recent debate between Lazarus (1982, 1984) and Zajonc (1980, 1984). Nevertheless, today many authors have observed that cognitive factors have unjustly been neglected and they stress the cognitive underpinnings of emotions (Calhoun, 1984; Frijda, 1986; Lazarus, Kanner, & Folkman 1980; Mandler, 1975; Solomon, 1984; Weiner, 1986).

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[2] This symbolism can be illustrated in varying degrees in several psychological theories. One example is James' famous theory of emotions: emotions are the perceptions of internal physiological reactions caused by an external event. So, we are afraid because we feel our hearts beating or our muscles stiffening. In recent psychological theories of emotions some aspects of psychophysiological symbolism are also found, for example, in Schachter & Singer (1962), Mandler (1975), and Plutchik (1980).

The question arises, what exactly do psychologists mean with this cognitive basis of emotion? Is an emotion a special form of thinking or must we look for thought processes as the origins of our emotions? In cognitive theories, emotions are often considered processes that consist of different subcomponents, such as appraisals of antecedents, tendencies to act, expression, and regulation (Frijda, 1986; Lazarus, Kanner, & Folkman, 1980). Thus, an emotion is not an all-or-none event; it cannot be seen as a static and lawlike state, characterized by fixed beginnings or ends. In conceptualizing emotions as processes in which appraisal dimensions and regulation processes play an important role, the possible influence of cognitive factors becomes clear.

In general, there are two ways in which the cognitive basis of emotions is conceptualized. In the *first* place most attention is directed at the cognitive dimensions underlying the emotional process. It is assumed that emotional stimuli are appraised in a certain way and that different appraisal structures go with different emotions. Besides these cognitive analyses of antecedents, the emotional experience itself underlies different cognitive dimensions. A strong relationship between emotional experiences and reported states of action readiness, such as the tendency to flee, attack, oppose or avoid has clearly demonstrated.[3] Thus, in this first conceptualization emotional antecedents and experiences are conceived of as *appraisal structures*.

In the *second* place the cognitive basis of emotions can be analyzed by studying the knowledge people have about their emotions. In this conceptualization lay people's views of what emotions are, what they do, and so on, are emphasized. This is called *emotion knowledge*. Until recently this perspective has been given little consideration and some authors even consider commonsense views as outdated. In my view the study of emotion knowledge has important implications for the conceptualization of, and research on, emotions. Emotion knowledge reflects dominant emotion ideologies and conventions and emotions are largely influenced by these knowledge structures. In this paper I will elaborate upon this socio-cognitive view by reviewing and commenting upon the content and function of emotion knowledge from two different perspectives: a prototype view and a rule model.

Prototypical Emotion Knowledge

What do we know about emotions? And how is this knowledge structured and activated? — these are the critical questions in concept formation and categorization studies. So, in the case of emotions, the following question is posed: how do we decide that a specific instance (e.g., jealousy) is a perfect example of what we call the class of emotions? The prototype approach seeks

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[3] For an extended overview of appraisal and action tendency dimensions see Frijda, Kuiper, & ter Schure, (1989). The three most important factors in appraisal dimensions are pleasantness, agency, and importance.

the answer in the principle of *family resemblance*.[4] The various members of the general category emotion are organized around its clearest examples: the prototypes. A prototype can be either a known instance or an abstract image, in any case it is one of the best examples of what we consider an emotion. Thus, the concept of emotion, just as many other concepts, is not defined by sharp boundaries. It appears that we cannot name one feature which can be attributed to all instances that one agrees to call an emotion. As a matter of consequence, emotions are 'fuzzy sets' (Fehr & Russell, 1984).

This means that it is impossible to judge with full interreliability whether a reaction belongs to a specific emotional repertoire. Such a judgment is not a matter of all-or-none. When I know I'm angry, a stranger might label my behavior as cool, or perhaps as slightly irritated. These different interpretations also depend for that reason on what people know and how much they know. The internal structure of the concept may vary with different levels of expertise or subjective meanings attributed to the category.

The *contents* of emotion prototypes were studied by asking subjects to describe an emotional event with as many details as possible (Shaver, Schwartz, Kirson, & O'Connor, 1987). When someone doesn't explain to you the cause of his emotion, and he only mentions his getting furious last week, you immediately ask why. It seems *unnatural* when either the cause or its emotional consequences are absent in the report. The stories were analyzed and coded. Two knowledge categories appeared to be present in all reports on emotions: antecedents and emotional reactions (both physiological, cognitive, experiential and behavioral reactions).[5] So, the conclusion can be drawn that the two prototypical attributes of an emotion form a causal emotion schema.

What *function* can we ascribe to these knowledge structures? In general the prototype perspective views knowledge of prototypes as necessary for the categorization and thus, recognition of instances of a category. The function of emotion knolwedge for emotions, however, is not explicated. From its general function we can infer that emotion knowledge influences the *labeling process*: people who have a distinctive and elaborated emotion network (e.g., because they are focussed upon a particular emotion) are likely to recognize more quickly aspects of an emotional episode and label it accordingly. For example, a man asks his wife what she did the previous evening. She is very wary of signs of jealousy because she is a strong advocate of 'free' relationships. So, she immediately becomes suspicious and labels her husband 'jealous'. The husband, however, considered his question an innocent expression of interest and consequently they end up fighting over whether he is, in fact, jealous. This example illustrates that labeling and interpretation are inextricably linked with knowledge of the emotion.

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[4] See, for example, Rosch & Mervis (1975), Mervis & Rosch (1981).

[5] In the case of the negative emotions the reports also contained self-control procedures.

From the causal emotion-schema (from antecedent to reaction) a second important function of emotion knowledge can be derived, *cognitive elaboration* (Frijda, 1988). Two examples illustrate this phenomenon. First intense guilt feelings have been demonstrated to coincide with an absence of actual guilt (Kroon, 1988) and second, sometimes anger reactions erupt when no other person is to blame (e.g., scolding at a flat tire) (Frijda, 1988). Apparently, sometimes emotions may occur, while correspondent cognitive structures (appraisal components) are initially not present. Nevertheless the emotion-schema presses us to consistency. We search for self-blame or perceive the anger-eliciting object as possessing a kind of agency. Knowledge of prototypical emotional causes, therefore, influences the interpretation of the emotional stimulus. Cognitive elaboration also works the other way around as is shown in the example of the jealous husband. We may deny or argue an assumed emotional state if the adequate antecedents are lacking.

At this point, we may conclude that prototypical emotion knowledge functions as a kind of causal emotion schema. In the first place, it interprets and fills in ambiguous, inconsistent, or incomplete information and this consequently stimulates or inhibits the labeling process. In the second place both the antecedents and the emotional experience are cognitively elaborated in accordance with the emotion schema.

Though we can infer some important aspects and functions of emotion knowledge, the prototype view has serious theoretical limitations. One of the most important is the conceptualization of the knowledge about emotions and the emotions themselves as two separate entitites. I will delve more deeply into this criticism later. First I will present a view which takes the (sometimes implicit) knowledge of emotion rules as the focus of attention.

Emotion Rules

Let us first consider an example. In the film *Under Fire*, which is situated in Nicaragua at the end of the Civil War and the approaching victory of the Sandinistas, an American journalist is publicly weeping because of the loss of her American friend who has been shot. A Nicaraguan woman watches her and says — seemingly emotionless — 'Now one American has died, but fifty thousand Nicaraguans have already died in this war'. The American woman looks at her, stops crying and walks away.

Why does she do this? Apparently not because she suddenly realizes that crying is an atypical expression of sadness. Perhaps she thinks that her crying in this situation is *illegitimate*. The confrontation with the Nicaraguan woman takes her back to another reality and a new frame of reference influences the perception of her own loss and regulates her behavior. Maybe she feels ashamed, guilty, or perhaps just depressed and she thinks that — compared to

the Nicaraguans — she has no right to show her sadness so publicly and vehemently.

The contents of emotion knowledge seem to be more extensive than has been suggested by the prototype view. In this situation a simple causal schema cannot explain her change in behavior. The inhibition of her emotional expression must be accounted for by the context of the situation. This is one example of an emotion rule as described by Hochschild (1983). Her study on the vicissitudes of emotions 'one should or should not feel' has revealed several specific rules concerning experiences and expressions of emotions. These rules refer to the intensity, duration, time, place, or type of the emotional experience or expression in a specific situation. Averill (1986) also proposes a rule model for emotions in which he distinguishes between constitutive, regulative and heuristic rules. The rules as described by Hochschild can be considered as constitutive (certain situations, and especially the appraisal of situations determine the kind of emotion one ought to feel) and regulative (how the emotion is expressed is influenced by different emotion rules).

What is the relationship between emotion knowledge and emotion rules? In general people are not aware of these rules; their behavior is an automatic consequence of the situations they are in and the way they have learned to perceive and cope with them. This seemingly non-reflective nature of rule-following behavior, however, does not implicate that we do not know the rules. Rules are based on tacit knowledge structures: we only become aware of them when one of those rules is broken. Then people seem to know immediately what is right or wrong with what we feel or how we express it. We subsequently evaluate an emotion or expression of an emotion. Thus, emotion rules are based on context-dependent, normative beliefs.

These and other beliefs about emotions have also been found in anthropological studies. It has been described in various ways and labeled with equally varied concepts; "emotion talk" (Heelas, 1986), "emotion ideology" (Gordon, 1981), and so forth. The common idea is that every culture has general practices and norms of how to cope with emotions. Underlying those conventions are beliefs on the nature and function of emotions and the general attitude towards them (see Heelas, 1986; Fischer & Mesquita, 1988). Heelas (1986) shows that ordinary emotion knowledge contains — in addition to prototypical elements such as antecedents and reactions — evaluations and beliefs about management techniques, the aetiology, nature and power of emotions. These cultural beliefs, which are closely connected with emotion rules and conventions, are of a social nature. They develop in interaction between people. Children learn to express and recognize increasingly more distinct emotions, because their social environment emphasizes specific aspects of emotional experience and explicates emotional conventions.

The *functions* of these belief systems are threefold. In the first place they provide guidelines for emotion regulation. We try to feel or express an emotion when we think we should. Hochschild (1983) calls this emotion work, the result of the continuous evaluation of our emotions: we judge them as right or wrong, overdone or silly, depending on the context in which they are elicited. Second, they accentuate and thus partly constitute the emotional experience itself. According to Heelas "differences in representation are actually differences in construction" (Heelas, 1986, p. 258). Emotion knowledge, in the form of emotion talk, can be seen as a kind of spotlight that illuminates raw experiences. The experience of an emotion differs depending on which aspect of the emotion is highlighted or remains in the dark. Third, they form the basis of accounting practices. Emotion-knowledge can often provide a legitimate explanation for rule-violating behavior. In this way beliefs, attitudes, or norms on the nature, workings, and expressions of emotions function as a defense for our positive self-image.

Conclusion

In this paper I have reviewed two different perspectives on emotion knowledge. Both approaches draw different boundaries around the emotion domain and both describe different functions. What they have in common is the evidence they present on the existence and importance of emotion knowledge. It is now time to draw some conclusions about these different perspectives and the consequences for the conceptualization of emotions.

The prototype view has shown that it is probable that people have knowledge of general concepts. But I have already mentioned an objection against the theoretical presuppositions of the prototype view: it does not account for the role of emotion knowledge in emotions. Emotion knowledge is studied from the perspective of concept formation; it is conceived as an internal, individual structure which is detached from its subject of knowledge. So, emotions seem to be quite different entities than the knowledge about them. This also leads to a restriction of the domain of emotion to knowledge of those features that are only necessary for the recognition and categorization of an emotion.

The rule model of emotions gives in to the limitations of the prototype view by stressing the social nature of emotion rules and belief systems. Emotion rules are culture- and situation-specific and based on correspondent, often tacit knowledge structures. This leads to an enlargement of the prototypical knowledge domain with context-dependent, normative and evaluative beliefs.

A socio-cognitive view adopts the assumptions of the rule model, but explicates more clearly the functions of emotion knowledge in emotions. It emphasizes that we cannot experience our emotions, that is to say make them

the subject of consciousness and discussion, without emotion knowledge. Emotions are not clearly defined states, but matters of continuous interpretation. Of course, this is not to say that we constantly reflect upon them. In most instances, general prototypes and context-dependent and normative knowledge structures tacitly accentuate, guide and regulate our emotional experiences and expressions. In this process, prototypes and rule-based beliefs have different, though complementary functions. I have conceived these prototypes as causal schemas, including what emotions are and how they are elicited. They account for the taken-for-granted nature of emotional experiences. We couch the stories about emotions in a prototypical emotion script. Rule-based beliefs regulate, constitute and account for specific emotional experiences. In cognitive terms, they form the basis of 'knowing how'.

References

Averill, J. (1974). An analysis of psychophysiological symbolism and its influence on theories of emotion. *Journal of the Theory of Social Behavior, 4*, 147-190.

Averill, J. (1986). The acquisition of emotions during adulthood. In R. Harré (Ed.), *The social construction of emotions* (pp. 98-119). Oxford: Basil Blackwell.

Calhoun, C. (1984). Cognitive emotions? In C. Calhoun & R. C. Solomon (Eds.), *What is an emotion?* (pp. 327-343). Oxford: Oxford University Press.

Fehr, B., & Russell, J. A. (1984). Concept of emotion viewed from a prototype perspective. *Journal of Experimental Psychology, 113*, 464-486.

Fischer, A., & Mesquita, B. (1988). Emoties zijn niet zuiver. *Psychologie en Maatschappij, 12*, 258-271.

Frijda, N. H. (1986). *The emotions.* Cambridge: Cambridge University Press.

Frijda, N. H. (1988). The role of cognition in emotion. Paper presented at the 28th International Congress of Psychology, Sydney, Australia.

Frijda, N. H., Kuiper, P., & Schure, L. ter (1989). Relations among emotion, appraisal and emotional action readiness. *Journal of Personality and Social Psychology, 57*, 212-229.

Gordon, S. L. (1981). The sociology of sentiments and emotions. In M. Rosenberg & R. H. Turner, (Eds.), *Social Psychology* (pp. 562-593). New York: Basic Books.

Heelas, P. (1986). Emotion talk across cultures. In R. Harré (Ed.), *The social construction of emotions* (pp. 234-267). Oxford: Basil Blackwell.

Hochschild, A. R. (1983). *The managed heart.* Berkeley: University of California Press.

Kroon, R. M. (1988). Aanleiding en structuur van schuldgevoel. Master thesis. Psychological Department, University of Amsterdam, no. psy. 11888225.

Lazarus, R. S. (1982). Thoughts on the relation between emotion and cognition. *American Psychologist, 37,* 1019-1024.

Lazarus, R. S. (1984). On the primacy of cognition. *American Psychologist, 39,* 124-129.

Lazarus, R. S., Kanner, A. D., & Folkman, S. (1980). Emotions: a cognitive-phenomenological analysis. In R. Plutchik & H. Kellerman (Eds.), *Emotion — theory, research and experience, vol. 1* (pp. 189-218). New York: Academic Press.

Mandler, G. (1975). *Mind and body.* New York: Norton.

Mervis, C. B., & Rosch, E. (1981). Categorization of natural objects. Annual Review of Psychology, 32, 89-115.

Plutchik, R. (1980). A general psychoevolutionary theory of emotion. In R. Plutchik & H. Kellerman, (Eds.), *Emotion — theory, research and experience* (pp. 3-34). New York: Academic Press.

Rosch, E., & Mervis, C. B. (1975). Family resemblances: Studies in the internal structures of categories. *Cognitive Psychology, 7,* 573-605.

Schachter, S., & Singer, J. E. (1962). Cognitive, social and physiological determinants of emotional state. *Psychological Review, 69,* 379-399.

Shaver, Ph., Schwartz, J., Kirson, D., & O'Connor, C. (1977). Emotion knowledge: further explorations of a prototype approach. *Journal of Personality and Social Psychology, 52,* 1061-1086.

Solomon, R. C. (1984). Emotions and choice. In C. Calhoun & R. C. Solomon (Eds.), *What is an emotion?* (pp. 305-326). Oxford: Oxford University Press.

Weiner, B. (1986). Attribution, emotion and action. In R. M. Sorrentino & E. T. Higgens (Eds.), *Handbook of Motivation and Cognition* (pp. 281-313). New York: Wiley.

Zajonc, R. B. (1980). Feeling and Thinking. *American Psychologist, 35,* 151-175.

Zajonc, R. B. (1984). On the primacy of affect. *American Psychologist, 39,* 117-123.

THINKING IN SOCIETY

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SUMMARY: The crisis of social psychology in the sixties reflected a dissatis-
faction with the implicit ideas governing methods and theories of rigorous
experimental research and with its neglect of the social nature of human beings.
New research consequently considered the individual as part of a group
positioned in relation with other groups. In Europe the idea of a group in social
psychology has been expanded beyond the physical individuals that constitute
a group to get at what may be called an ideology, a belief system, a
Weltanschauung or a community of thoughts and of deeds. In this kind of
research an attempt is made to study the cognitive strategies used when treating
information. These strategies or perception mechanisms are not comparable
to information processing by computers. They are embedded in a community
of thoughts and of deeds and follow certain observable patterns, but they are
differently pruned by individuals depending on their context and the relevance
of the topic for the individual.

The crisis of social psychology in the sixties reflected a dissatisfaction
with the often unquestioned acceptance of the assumptions — social, scien-
tific and philosophical — underlying many of the theories and methods that
have followed the long established tradition of rigorous experimental re-
search. This crisis was followed by a cry for change, as clearly expressed in
Israel and Tajfel (1972). The authors of this volume called for an examination
of the nature of theory in social psychology and they questioned the adequacy
of the methods used for the analysis of 'natural' social phenomena as well as
the unstated assumptions, values and presuppositions about Man or Woman
and society. An example of this critical work is the sharp criticism about the
classical attribution studies raised by Apfelbaum and Herzlich (1970).

But there was not only a critique of the implicit ideas governing methods
and theories which had become familiar in the fifties and sixties. Social
psychology, it was said, managed neither to study social conduct as an interac-
tion between individuals and society, nor to study individuals in society. A
discipline that ignored the social nature of human beings could not be called
'social psychology'. Indeed, the most differentiating feature of human social
behavior is the human use of symbols in social communication, and —
proceeding from it — the creation and dissemination of ideologies.[1]

The social psychological study of inter-group relations and social
categorization by Henri Tajfel and his group in Bristol was one of the several

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[1] I am well aware that the concept of 'ideology' is a messy one. But I do not really think that
concepts such as 'belief systems, 'system of ideas', or 'Weltanschauung' are far more satisfactory.
Personally, I prefer the notion of 'community of thoughts and of deeds'; Jean-Pierre Deconchy
and some of my colleagues refer to a 'fundamental anthropology'.

answers to that 1972 cry. Serge Moscovici's (1961) study of the social representations of groups was its earlier anticipation. Jean Claude Deschamps'
(1984) work on social attributions evolved from this new approach to human
behavior. For the most part, the new approach considered the individual as
part of a group (e.g., the group of the working class, the group of adolescents,
or of women) positioned in relation with other groups (e.g., the group of
factory owners, of adults, or of men) within a social hierarchy. The dynamic
of the inter-group relation was then studied in order to understand the way an
individual perceives, judges and behaves in relation with another individual
(of the same group or of another group).

In Europe (especially in France) the idea of a group in social psychology
has recently been expanded beyond the physical individuals that constitute a
group to get at what may be called a social representation (Moscovici, 1961),
an ideology (Beauvois, 1984), a fundamental anthropology (Deconchy, 1987),
ideological representations (Deconchy, 1980), or an epistemo-ideology
(Doise, 1982). Instead of banishing ideology as if it were an obstacle to
scientifically true knowledge about the ways the human mind functions, all
these approaches use it as an instrumental tool in the groping for understanding of common sense and of everyday activities. From that point of view,
common sense and everyday activities are not just a chaotic mess; they are
embedded in what might be called a community of thoughts (in German:
Denkkollektiv[2]) and of deeds and henceforth they follow certain observable
patterns.

Thus, research tries to observe or, more often, experimentally manipulates the way everyday people 'read' factual information and filter this information by selecting parts of it and by loading these parts with new information
which is independent of the factual information at hand. This new information is entirely derived and constructed from more or less equally shared and
differentiatingly pruned 'ideologies'. Thus, when confronted with factual
information about the coming birth of a child conceived in vitro versus in the
common way, people would produce new specific information about the
future abilities of that child (on an affective, biological, social level). This new
information has nothing to do with what may be known about the subject or
with what they were told, but depends only on their own ideas about 'nature',
the way 'nature' works, and what it can do to human beings when it is
seemingly diverted from its 'natural' course.

In that sense, 'ideologies' are not entities governed by natural lawlike
principles of operation already existing and to be discovered somewhere.
Their actualizations are ongoing processes and proceed from social communication. In line with Jansz's (this volume, p. 249) "conglomerate of
meaning, which shapes the person's attribution of meaning to his or her

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[2] We are indebted for this term to Ludwik Fleck (1935).

world," they follow certain regularities or patterns without which mutual understanding and conversation would be impossible.

Thus, the very idea of the existence of communities of thoughts and of deeds may or may not be shared by researchers. It may also be only partially shared or differentiatingly pruned and negotiated. Even if the idea of a community of thoughts and of deeds is accepted, for instance, by a number of French speaking researchers, each one may put emphasis on some specific aspect and be less specific about some other. That means that he or she prunes the 'community principle' from a personal perspective where certain elements are given priority over others. One author may examine the systematic nature of certain cognitive processes among his subjects and refer them to an immunization strategy against scientific explanations; for instance, the immunization strategy allowing the subjects to keep intact their adhesion to a religious belief system. Another author perhaps prefers to refer such strategies to the 'material' position the individual holds in society. And still another author may bring to view the underlying values of such processes and refer them to a given society's guiding rules of behavior. Despite such differences, the acceptance of the idea of a community of thoughts and of deeds constitutes, a basis for discussions or even disputes at scientific meetings (see Aebischer, Deconchy & Lipiansky, in press). In fact, they are in themselves an illustration of the way a community of thoughts and of deeds may be pruned, derive meanings and establish relationships between topics. The existence of this specific community of thoughts may, however, make difficult and even preclude dialogue or understanding in the sense of an agreement or disagreement with researchers whose view of the human mind is of a rather behaviorist stance. Understanding in the same sense might also be difficult with scholars who think that people do not know what happens in their mind and that they need help from outside, or with cognitive psychologists who assimilate the human mind to a computer-like processing machine which 'distorts' true information and henceforth reality. Their findings, in order to be compatible and intelligible within the 'community of thoughts' principle must be reinterpreted.

We can use what Ross (1977) called the "fundamental error". Ross claims that we tend to make internal attributions rather than external ones, and that we convey the responsibility of certain outcomes or events to individuals rather than to the situation or to chance. Now, contrary to Ross' and other cognitive psychologists' contention, this perception process may be regarded as only an instance of a more general process which consists in the attribution of control to individuals over events. Instead of a distortion of the mind, it should be regarded as a coherent perception strategy which has as a starting-point the idea or general assumption of people's control over events and situations where in fact no such control is possible.

One colorful instance of the strategy of overestimating people's control over events are Lerner's (1965) studies on the justice motive in social behavior. When confronted with a situation where a fellow student (in fact a confederate of the experimenter) would get unfair treatment and not be paid for his work as agreed upon beforehand, experimental subjects did not react against this unfair treatment, but blame the victim for it, as if it were all his fault. This shows that an event is not necessarily perceived as it has actually happened. Instead, its perception is, up to a point, controlled by the needs and interests of the perceiver. In the case of Lerner's subjects, new information was added which could not be derived from the event itself. This new information helped to fill out the blanks and to establish (apparently missing) links between co-occurent items. It made these fit into an ordered and predictable pattern which on a socially shared level or frame of reference made sense. The good is rewarded and the evil punished, or people get what they deserve and they deserve what they get. In this precise context, the social justice motive actualized the strategy of reading control into the event in such a way that the relation between the different topics could be understood.

Interestingly enough, events in such contexts where control seems to be precluded and not in the hands of human beings but of God (death, life after death) or some other external forces (earthquakes, sickness, etc.), innumerable strategies — 'incantation' or ingratiating strategies — are deployed to get some control over these events. Most religious beliefs, for instance, propose ways to negotiate and to bring oneself into favor with external forces such as God. Depending on contexts, ingratiating may take the form of fasting, prayer, lighting a candle, ritual dancing, and so forth. Now, such 'incantation' strategies may also be used in non-religious contexts; in situations of anticipation of a valued event where the outcome is still uncertain. I can illustrate the point by referring to a study currently in progress at my own university.

Young women, who were the experimental subjects, were presented with the case of a future mother who had deliberately chosen not to undergo a local anesthesia for childbirth. Compared to other young women who were presented with the case of a future mother who chose not to suffer but to undergo local anesthesia for childbirth, or compared to future mothers for whom the obstetrician chose one or the other modalities of childbirth, predictions as to the psychological and physiological development of the yet to be born child were significantly and overwhelmingly more positive. When the future mother chose to suffer, the child, it was thought, would be more sociable and alert, have better appetite and sleep, and less respiration problems than when she chose not to suffer. In other words, whereas it can hardly be said that any reliable data exist on that matter, the young women of our study would nevertheless establish a positive relationship between the deliberate choice by the future mother of inflicting pain on herself and the future prospects of the child to be born. Again, they fit these elements into

an ordered and predictable pattern which on a socially shared level makes sense — that to suffer pain now may have a positive effect on a future, but yet uncertain outcome. This general belief in human control over events and situations gives thus rise to differentiatingly pruned instances of perception and evaluation strategies with different contents.

It is interesting to see that belief systems often not only resist scientific explanation, but that they are often incompatible with it, thus rendering explicable the preference of many educated people for 'natural medicine' practitioners rather than for representatives of scientific medicine. These people do not simply lack appropriate information about sickness, but they have their own understanding of the functioning of the human body, of sickness as well as of health. Therefore, the kind of research developed in recent years in France attempts to study the cognitive strategies used when treating information. These strategies or perception mechanisms are not comparable to information processing by computers as the cognitive psychologists often view it. They are shaped and modelled by communities of thoughts and of deeds which orient them depending on their context, their interests or the relevance of the topic.

From that point of view cognitive strategies are just one instance of a community of thoughts and of deeds. Cognitive activities are one expression of the actualization of an ideology. Thinking, in fact, is a modelling facility (Pask, 1976) as are speech, emotions (see also Fischer, this volume) or bodily activities. They are expressions of an ongoing interaction, of permanent prunings and reconstructions which, despite their creativity, follow certain patterns in order to permit dialogue and understanding. Their creativity might engender conflict in situations where agreement seemed to be the case and put peril in this ongoing dialogue. It is within the framework of that dialogue that new understanding of the situation has to be negotiated, conflict being one of the ingredients of innovation.

References

Aebischer, V., Deconchy, J. P., & Lipiansky, M. (Eds.). (in press). *Représentations sociales et idéologies*. Cousset (Fribourg): DelVal.

Apfelbaum, E., & Herzlich, C. (1970). La théorie de l'attribution en psychologie sociale, *Bulletin de Psychologie, 24*, 961-976.

Beauvois, J. L. (1984). *La psychologie quotidienne*. Paris: Presses Universitaires de France.

Deconchy, J. P. (1980). *Orthodoxie religieuse et sciences humaines, suivi de religious orthodoxy, rationality and scientific knowledge*. Paris/La Haye: Mouton.

Deconchy, J. P. (1987). Conduites sociales, comparaison sociale et représentation du patrimoine comportemental commun à l'homme et à l'animal. In J. L. Beauvois, R. V. Joule, & J-M. Monteil (Eds.), *Perspectives cognitives et conduites sociales*, (pp. 151-185). Cousset (Fribourg): DelVal.

Deschamps, J. C. (1984). The social psychology of intergroup relation and categorical differentiation. In H. Tajfel (Ed.). *The social dimension*, (pp. 245-266). Cambridge: Cambridge University Press.

Doise, W. (1982). *L'explication en psychologie sociale*. Paris: Presses Universitaires de France.

Fleck, L. (1935). *Entstehung und Entwicklung einer wissenschaftlichen Tatsache. Einführung in die Lehre vom Denkstil und Denkkollektiv*. Basel: Benno Schwabe & Co. (Republished in 1980 by Suhrkamp: Frankfurt a.M.).

Israel, J., & Tajfel, H. (1972). *The context of social psychology. A critical assessment*. London: Academic Press.

Lerner, M. J. (1965). Evaluation of performance as a function of performer's reward and attractiveness. *Journal of Personality and Social Psychology*, *4*, 137-146.

Moscovici, S. (1961). *La psychanalyse, son image et son public*. Paris: Presses Universitaires de France.

Pask, G. (1976). *Conversation theory. Applications in education and epistemology*. Amsterdam: Elsevier Scientific Publishing Company.

Ross, L. (1977). The intuitive psychologist and his shortcomings: distortions in one attribution process. In L. Berkowitz (Ed.). *Advances in experimental social psychology. Vol. 10*, (pp. 179-221). New York: Academic Press.

THE MUTUAL CONSTRUCTION OF SOCIAL AND SELF:
A SOCIAL CRITIQUE OF SOCIAL COGNITION

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SUMMARY: In general, modern psychology is characterized by an individualistic view of the person. Theories of social cognition, which are dominant in social psychology nowadays, are no exception to this rule. They conceptualize the self-schema as a homunculus which constructs actively his personal environment. Consequently, the classical dualism between self and social is revitalized, with the attribution of primacy to the individual mind. In this paper, person and selfhood are conceptualized as being constructed in interpersonal communication. Primacy is attributed to public discourse, without, however, lapsing into determinism. As an agent, the individual takes part in and contributes to public communication. On the one hand, he or she uses public language for, for example, self-reflection. On the other hand, language cannot exist without the 'words' of individual speakers. This mutual construction is illustrated with some examples.

Introduction

In academic cognitive psychology, *the individual* is taken for granted as an abstract entity, highly distinct and well apart from others. The individual human being is seen as the proprietor of his or her capacities, owing nothing to society for them (cf. McPherson's, 1962, "possessive individualism"). So, cognitive psychologists picture the individual as existing independently of a social context; circumscribed by "firm boundaries that separate self from non-self, marking each person as an independent event in the universe" (Sampson, 1987, p. 85). The object of study is limited to the structures and processes within the individual's mind, which are taken to be an internal representation of the external world. In cognitive psychology primacy is given to the individual knower and to subjective determinants of behavior (Sampson, 1981).

By consequence, social phenomena tend to be either neglected, or conceived as a summing-up of individual events. This poses serious problems for social psychology, for there it is hardly possible to consider the interpersonal environment as a 'quantité négligeable'. Recently, this problem seems to have been solved by a merging of cognitive and social psychology, named *social*

cognition (Ostrom, 1984). At first glance one is inclined to welcome this tradition as a shift towards a more social conception of cognition. To do this, however, would be a mistake: "The study of social cognition concerns how people make sense of other people and themselves (...), it concerns both how people think about the social world and how they think they think about the social world" (Fiske & Taylor, 1984, p. 17). It is important to note that social cognition studies the cognitive processes involved in understanding social behavior. It is not a study of social influences on cognitive processes. Therefore, the cognitions are not social themselves, but they are individual cognitions about social phenomena. By way of preliminary conclusion it can be stated that social cognition is as individualistic as ordinary cognitive psychology is. In the next sections arguments will be presented to substantiate this thesis.

Mythical Schemata

Social cognitivists assert that people make sense of themselves, of other persons and of objects by using schemata. Schemata are conceptual structures in the mind (or, in the entire nervous system; Neisser, 1976, p. 54) that organize the storage of data in memory (Rumelhart, 1984). Knowledge is organized in a domain-specific way, so there are different kinds of schemata, for different kinds of issues, for example, a person schema and a role schema (Fiske & Taylor, 1984, p. 149).

The self-schema is generally considered to be the most important. As a "cognitive generalization about the self, derived from past experience", it is said to "organize and guide the processing of self-related information contained in the individual's social experiences" (Markus, 1977, p. 64). The information processed by this schema is of a personal nature and very close to the individual. As a consequence a central role is attributed to the self-schema: not only in analyzing knowledge about oneself, but in ordering information from other schemata as well (Fischer, 1987). The central position of the self-schema can be articulated as being situated on top of the pyramid of schemata, it supervizes the others.

One specific characteristic of the self-schema stands out in social cognition research. It is assumed that the self-schema (or, the self-concept as a set of self-schemata) plays an active role in interpreting reality and in manipulating reality. The self-concept (or schema) mediates and regulates behavior, it interprets and organizes self-relevant actions and experiences (Cantor, Markus, Niedenthal & Nurius, 1986; Markus & Wurf, 1987). In other words, the self-concept is viewed as dynamic. However, this dynamism is one-sided: the self-schemata constructing the self-relevant reality. On the other hand, self-schemata are resistant to change; in recent empirical research the structures of the self appear to be stable and immune to environmental influences

(Greenwald & Pratkanis, 1984). Consequently, this stability of self-schemata guarantees consistency and continuity in personal experiences (Jansz, 1988).

The self-concept's active role in constructing reality is best illustrated in Greenwald's work. The self-concept (or ego, which he uses as a synonym) is so active in organizing knowledge that it distorts reality. The working of the self-concept is characterized by three cognitive biases that function to preserve organization in cognitive structures. These biases are *ego-centricity*, *beneffectance*, that is, perception of responsibility for desired but not for undesired outcomes, and *cognitive conservatism* (Greenwald, 1980; Greenwald & Pratkanis, 1984).

Some important conclusions can be drawn here about the self, or self-schemata, in social cognition research:

1. The self is a conceptual structure of knowledge which constructs, even biases, the person's perception of his environment in an active way. This activity is not limited to actual needs but is expanded creatively into the future (Markus & Nurius, 1987).

2. It is, however, rather obscure what the *environment* is. One can guess that the environment consists of other people and objects. But close reading reveals that it in fact consists of the *perception* of other people and objects. This leads us to an intriguing paradox in social cognition. Every person creates his environment, so A constructs his or her perception of B, and B constructs his or her perception of A, so both have constructions of one another. But what happens if their creative constructions do not fit each other, how can communication be guaranteed? Due to an individualistic bias in social cognition, this 'reciprocity paradox' is not even formulated. The theory is restricted to an abstract individual, engaged in a constructive solo, without much discussion of the contribution of other individuals.

3. The relation between self and environment is conceived in a dualistic sense and the dynamic relation between them holds true only in one direction. The relation is dualistic because of the fact that two entities are presupposed with a gap between them. In social cognition, the entity self-concept/schema is researched methodically, about the nature of the environment we know very little. The self (or the person, for that matter) is said to possess certain characteristics, owing, however, nothing to the environment for them. This is, again, rather paradoxical, for the self-schema is derived from past experience (Markus, 1977, p. 64). It is hard to

believe that social cognitivists are bound to constrict this 'experience' to the person's former perceptions and constructions.

4. In emphasizing the autonomy of the self, social cognitivists create another duality, that is, the duality between an inner reality and a world 'out there'. In this "inner-outer duality" (Secord, 1986) the inner world is assumed to be a fenced off private realm of personal experiences. The individual is the only one who has access to this hidden world. This private access is, in a sense, a guarantee for the individual's privacy which is one of the constituent elements of individualism (Lukes, 1973, p. 59). As a matter of consequence psychological explanation is formulated in terms belonging to the inner world of mind, in this case the working of cognitive schemata. The outer world is assumed to be there, but is ignored in explanatory matters.

Social cognition attributes primacy to the individual. Description and explanation are put in terms of individual perceptions and cognitions, seen as processes of the schemata the individual does or does not possess. The self-concept plays a special role. Like a homunculus, it actively constructs the phenomenal world of the individual. This results in a *solipsist* version of the constructive process: the self creates its personal reality, according to its own standards. Contrary to this solipsism, a social alternative is proposed here. Social constructionism gives a natural priority to the realm of interpersonal, communicative interaction (Bruner, 1986; Gergen & Davis, 1985). Primacy is attributed to this "public-collective domain" (Harré, 1983, pp. 44-45) for two major reasons. First: the community is already there when the individual is born into it, and second: each individual human being becomes a person of worth in and through social interaction. These issues will be discussed more extensively in the next section.

Why Do We Need a Social Alternative?

In ontogenesis conclusive arguments can be found for an emphasis on the social domain. Let's discuss these matters from the perspective of actual person A. Before she was born, she was talked about by her parents and their friends and relatives. After her birth and during her development she is talked to by other persons before she can use language herself. In other words, she is defined as a person in a public-collective discourse before she herself can take part in the discourse. In this sense the 'you' is older than the 'I' (Shotter, 1989). However, this does not necessarily mean that the developing person is wholly determined by her discursive environment, it means that the public-collective domain has and retains a natural priority to the realm of individual

agency. Each and every person must communicate according to socially established rules in order to be intelligible and accountable (Shotter, 1984).

My second reason for a social alternative is somewhat different from the previous one, because it is formulated in moral terms. Nowadays, in the Western world the most pervasive and influential ideology is the ideology of individualism. People, successful people at least, are presented in public discourse as very powerful agents who owe almost nothing to others for their success. The 'autonomous person' is considered to be the cultural ideal and individuals are educated to live up to the expectations. On the other hand society, *any* society, is inconceivable without mutual commitments, so a responsible citizen cannot be too egocentric. This leaves us with a moral conflict between an individualistic ideology and a morality of care. In my opinion psychology can and must contribute to a solution of this conflict by debunking the individualistic ideology. The Western individual is not a modern-time Robinson Crusoe building his own isolated world according to his fancy (Billig, et al. 1988, p. 2), but a social animal, dependent on others for his or her perceptions of the material world, other persons and even for the perception of himself or herself. In the remainder of this paper an outline is presented of a social constructionist perspective on the self and the person, that may account for the moral fact that persons need other persons to live their lives as autonomous, responsible and reflexive individuals.

Persons and Selves Are Socially Constructed Realities

Theories of social cognition share the fundamental assumption that human beings need order and consistency. Order in the perceptual chaos is brought about by the individual's cognitive schema. Like social cognition, social constructionism emphasizes the man-made nature of our conceptions of the world; people are active construants of meaning (Zeegers & Jansz, 1988). However, social constructionism conceptualizes the construction of meaning as a collective enterprise: meaning is socially constructed. Consequently, order in human understanding does not come from the mental structures of the individual, but it is rooted in the man-made conventions of the interpersonal domain (Vygotsky, 1987). The syntactic and semantic rules set up order in our speech and language, and this holds true for other social and cultural rules as well. Part of our speech and action is structured by conventions. But part of the rule-governed reality remains contradictory: "contrary themes of common sense are neither rare nor unimportant" (Billig, et al., 1988, p. 15).

The primacy of the public-collective domain has, of course, consequences for the experiential world of the individual. This so-called private-individual domain, that is, the personal psychology of a human being is shaped by the public-collective domain (Harré, 1983; Vygotsky, 1987). "Insofar as individual people construct a personal discourse on the model of public

discourse, they become complex 'mental' beings with unique 'inner worlds'" (Harré, 1986, p. 120).

An important point to discuss is whether this constructionist alternative leads necessarily to social determinism, since so-called personal discourse is borrowed from public discourse. However, it is easily forgotten that public discourse can exist only in and through the speech of persons. As speakers, that is, bearers of the language, they carry public discourse and contribute to it. While speaking, that is, using the terms that are available in public discourse, the person connects his or her speech with action. In this discursive action new meanings are created. The individual speaker, however, is constrained by the language-games he or she takes part in. As a result, strictly 'personal speech' of an idiosyncratic nature will in general not have any influence on (public) communication.

An example may clarify the relationship between person and discourse. As the social representation of the *yuppie* was constructed in the media based upon a new professional category, a new term for person description became available in public discourse. Individual human beings could use this term to describe others or themselves. In the latter case, they verbalized their identity in a new way, that is, as a yuppie. Although a person might 'copy' the collective meaning of a yuppie and try to live up to that standard, a different consequence is possible. A person creates new meanings through his or her speech and actions within the constraints of public discourse. As an agent he or she might contribute to a change of the concept 'yuppie'. This is what has happened recently. Yuppie used to categorize a group of very rich and very young people. As this social representation became widespread and many persons defined others and themselves as 'yuppies', the concept broadened. Nowadays, a yuppie does not have to be that young and that rich. Any youthful, good-looking and well-dressed person with a reasonable income can attain a yuppie-identity.

The interaction between person and discourse is central to this point of view. Interaction is conceived here far more fundamentally than usual (as, e.g., in Bandura's reciprocal interactionism). Here, interaction is defined as *mutual construction*. Public discourse creates the person, but the person constructs discourse as well and consequently the interacting terms change in this constructive process (Shotter, 1984). The fruitfulness of this approach is best illustrated by an example from our empirical research. In one study, people are asked to tell how they would solve a conflict between self-interest and the interests of others. In the social cognitive approach, the respondent's internal representation of that conflict, that is, his or her moral attitude would be measured. In a social constructionist approach, research is focused upon the accounting practices as verbalized in ordinary discourse (Semin & Manstead, 1983; Shotter, 1984). In analyzing the accounts, it became clear that the participants did not limit themselves to a justification of their

proposed solution. In fact, they accounted as well for the kind of person they took themselves to be. Here are two examples: someone apologized for being an 'individualist' and by consequence not being able to practice commitments; someone else constructed himself as a 'Calvinist', doing things because of the guilt he felt towards others.

This truly social approach is based upon the primacy of the public-collective domain and directs psychology's attention to ordinary speech as the empirical reality in which the mutual construction of person and discourse takes place. In our research we can account for both personal perceptions and for public ways of world making, without having to assume hypothetical constructs like schemas. As a consequence 'social cognitions' are conceived as the public conglomerate of meaning, which shapes the person's attribution of meaning to his or her world.

References

Billig, M., Condor, S., Edwards, E., Gane, M., Middleton, D., & Radley, A. (1988). *Ideological dilemmas. A social psychology of everyday thinking.* London: Sage.

Bruner, J. (1986). *Actual minds, possible worlds.* Cambridge, MA: Harvard University Press.

Cantor, N., Markus, H., Niedenthal, P., & Nurius, P. (1986). On motivation and the self-concept. In R. M. Sorrentino & E. T. Higgins (Eds.), *Handbook of motivation and cognition. Foundations of social behavior* (pp. 96-122). New York: John Wiley & Sons.

Fischer, A. (1987). Zelfschema's. Schijn of werkelijkheid. *De Psycholoog, XXII*, 439-446.

Fiske, S. T., & Taylor, S. E. (1984). *Social cognition.* Reading, MA: Addison Wesley Publishing Company.

Gergen, K. J., & Davis, K. E. (Eds.) (1985). *The social construction of the person.* New York: Springer-Verlag.

Greenwald, A. (1980). The totalitarian ego: fabrication and revision of personal history. *American Psychologist, 35*, 603-618.

Greenwald, A., & Pratkanis, A. (1984). The self. In R. S. Wyer & T. K. Srull (Eds.), *Handbook of social cognition, Volume 3* (pp. 129-179). Hillsdale, NJ: Erlbaum.

Harré, R. (1983). *Personal being: a theory for individual psychology.* Oxford: Basil Blackwell.

Harré, R. (1986). Social sources of mental content and order. In J. Margolis, P. T. Manicas, R. Harré, & P. F. Secord, *Psychology. Designing the discipline* (pp. 91-127). Oxford: Basil Blackwell.

Jansz, J. (1988). Jij of ik? Van ego tot zelfconcept. *De Psycholoog XXIII*, 370-377.

Lukes, S. (1973). *Individualism.* Oxford: Basil Blackwell.

Markus, H. (1977). Self-schemata and processing information about the self. *Journal of Personality and Social Psychology, 35,* 63-78.

Markus, H., & Nurius, P. (1987). Possible selves: The interface between motivation and the self-concept. In K. Yardley & T. Honess (Eds.), *Self and identity: Psychosocial perspectives* (pp. 157-173). Chichester/New York: Wiley & Sons.

Markus, H., & Wurf, E. (1987). The dynamic self-concept: A social psychological perspective. *Annual Review of Psychology, 38,* 299-337.

McPherson, C. B. (1962). *The political theory of possessive individualism: Hobbes to Locke.* Oxford: Oxford University Press.

Neisser, U. (1976). *Cognition and reality.* San Francisco: Freeman.

Ostrom, T. M. (1984). The Sovereignty of Social Cognition. In R. S. Wyer & T. K. Srull (Eds.), *Handbook of social cognition,* Volume 1 (pp. 1-38). Hillsdale, NJ: Erlbaum.

Rumelhart, D. E. (1984). Schemata and the cognitive system. In R. S. Wyer & T. K. Srull (Eds.), *Handbook of social cognition,* Volume 1 (pp. 161-188). Hillsdale, NJ:Erlbaum.

Sampson, E. E. (1981). Cognitive psychology as ideology. *American Psychologist, 36,* 730-743.

Sampson, E. E. (1987). Individualization and domination: Undermining the social bond. In C. Kagitcibasi (Ed.), *Growth and progress in cross-cultural psychology* (pp. 84-93). Berwyn/Lisse: Swets & Zeitlinger.

Secord, P. F. (1986). Social psychology as a science. In J. Margolis, P. T. Manicas, R. Harré, & P. F. Secord, *Psychology. Designing the discipline* (pp. 128-164). Oxford: Basil Blackwell.

Semin, G. R. & Manstead, A. S. R. (1983). *The accountability of conduct. A social psychological analysis.* London: Academic Press.

Shotter, J. (1984). *Social accountability and selfhood.* Oxford: Basil Blackwell.

Shotter, J. (1989). Social accountability and the social construction of 'You'. In J. Shotter & K. Gergen (Eds.), *Texts of identity* (pp. 133-151). London: Sage.

Vygotsky, L. S. (1987). Thinking and speech. In R. W. Rieber & A. S. Carton (Eds.), *The collected works of L. S. Vygotsky. Volume 1* (pp. 39-285). New York: Plenum Press.

Zeegers, W. & Jansz, J. (1988). Betekenisgeving als sociaal proces. *Psychologie en Maatschappij, 43,* 117-132.

FROM FEMINIST RESEARCH TO NEW CATEGORIES IN PSYCHOLOGY: WHAT IS AND WHAT MAY BE

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SUMMARY: The author argues that feminist research helped to restructure the theoretical and empirical content of psychology despite being marginalized. It is claimed that feminist research, which has led to the conceptual shift from sex to gender, has helped to unmask a variety of hidden assumptions, such as using biological determinism to explain psychological effects. In addition, it is suggested that feminist research offers a new theoretical perspective whereby the sociologically and historically bound generalized beliefs about universal biological processes are first brought into clear focus, thus permitting a subsequent shifting of the analysis towards a more socially oriented perspective. The author asserts that feminist thought supports a more integrative base of knowledge and transcends traditional subdisciplinary frontiers.

What is a Name? Feminist Thought is not Feminism

At the 1989 Conference of the International Society for Theoretical Psychology, an interesting labelling phenomenon occurred. The abstracts of two papers, Mary Gergen's and the present author's, explicitly stated that their task was to reframe psychological theories. Rather than being assigned, separately or together, to sessions, say on 'alternative theory', they were placed together in a session with the 'simple' label 'feminism'. The placement of those two papers in a 'separate' session supports the thesis advanced in my article.   By assigning feminist ideas to a special category the various elites of our society attempt to marginalize them. The labelling of the session itself is neither completely accidental nor without a certain meaning. There are in fact consequential links for women between the process of labelling or titling, and women's treatment or 'women's Ms-entitlement' (Apfelbaum & Hadley, 1986). Whether it was the result of a deliberate decision or the work of the unconscious is not relevant; the point is that, as social scientists, we know that labelling can have far-reaching effects on the perception and reception of ideas. The category of 'feminism' is particularly ambiguous and, too often mistaken for militant advocacy and/or ideology. Feminism, as one of the major social movements of the late 1960's has indeed been the stepping stone which opened the pathway for feminist scholarship, much in the same way as

the social movements and upheavals of the last half of the 19th century opened the ways to the development of the social sciences (Apfelbaum, 1986).

But, feminist thought should by no means be mistaken for feminism. For the past two decades, women in psychology, sociology, history, and so forth, have been building a new, solidly researched knowledge base. One of the innovations of feminist scholarship and the source of its seminal heuristic power comes from the fact that the debates about research issues, metatheoretical and epistemological questions cut across the boundaries of the traditionally established disciplines. Feminist thought has thus provided conceptual categories which challenge the underlying assumptions on which traditional positivistic knowledge is generated. The addition of this broadened perspective and new analytic categories has helped renew theorizing in the social sciences.

In fact, of all the movements of the late 1960's that questioned the social sciences' implicit ideological choices and biases, feminism remains one of the last to be active and effective. In particular, feminist critique has exposed the assumption of universality, widely accepted in social science discourses. Universality implies that women in particular (but not only women), when not totally excluded from the subject matter, have been subsumed without any further specification in a generalized, unified conception that was represented in the idea of 'man'. It is my contention that feminist thought can contribute, in several ways, to reframing psychological theories, if the scientists from the established disciplines are willing to engage in a non-biased, equitable dialogue over the theoretical issues themselves. But one may wonder whether such an exchange is truly possible today?

Up to now, "Many traditional fields seem to have successfully fended off the feminist challenge." (Gergen, 1988). Although the growing number of publications about women's issues seems to testify positively to the vitality of feminist scholarship as a whole, women from a variety of areas of the social sciences (e.g., in history, psychology, and sociology) continue to denounce in unison the marginal status attributed to this trend of thought (Unger, 1982; Collin, 1988; Riot-Sarcey, Planté, & Varikas, 1988); they also denounce the "collective silence" (Farge, 1984) of scientist colleagues about feminist thought.

More specifically, studies on women gained a certain organizational visibility which coincided with the introduction of women as the subjects of research (Scott, 1987). Thus, for example, in 1973, the American Psychological Association created a separate APA section (Division 35), for the Psychology of Women. Institutional advances such as this, in professional status, organization, teaching and research were in part an expression of a progressive political stand, but were also to some extent a consequence of tokenism, a strategic response to the pressures of the social movements of the 1970's, just as Black Studies programs were created in the U.S. in response to the

various civil rights and Black Power movements and concerns of black scholars. The salience of the two-edged sword must be stressed: on the one hand, institutional recognition gave legitimacy *per se* to feminist research. At the same time, however, in defining it as a separate field of research, it also contributed indirectly to consigning women to the margins of traditional knowledge. This position of quasi-Apartheid creates a Catch 22 situation which then tends to perpetuate the deviant 'outsider' position of women and consequently to further limit the legitimacy of the feminist approach (Unger, 1982).

It is not coincidence that an increasing number of women are complaining that some mainstream scientists, although by now convinced that women can in fact provide knowledge about women, still deny that women's voices must be integrated and contribute to the elaboration of a 'universal' knowledge base, equally informed by both halves of humanity (Collin, 1988; Riot-Sarcey, Planté, & Varikas, 1988; Scott, 1987). To a certain extent, the psychological establishment acts very much like a patient with unilateral neglect or what Battershy (1956) labels "hemi-inattention", that is a patient who "behaves not only as if nothing were happening in the left hemispace but also *as if nothing of any importance could be expected to occur there.*" (Mesulam, 1985, cited by Sachs, 1987, p. 79; my emphasis). The marginal status of feminist scholarship needs to be clearly emphasized, because it limits in various ways the development of feminist thought itself. To explain how and why is beyond the scope of this paper. From a sociology of knowledge perspective, the way feminist research has been treated by the establishment can be analyzed in terms similar to those we have used for analyzing the fate of John Garcia's anomalous results in taste-aversion learning, which challenged a well established neo-behaviorist paradigm (Lubek & Apfelbaum, 1987; Apfelbaum, 1989).

Three Challenges from Feminist Research: Sex Differences, Advocacy, and Gender

I would like to review just three of the many challenging questions raised by feminist scholarship which open the way to alternatives concerning knowledge generation.

The psychology of sex differences. The area of sex differences is probably the most relevant to illustrating how feminist scholarship in psychology helped to identify and unmask some of the hidden unexamined theoretical assumptions of the discipline as a whole. The artefactual status attributed to 'sex' in psychology is a good illustration of how the language of universality actually leads to theoretical ambiguity and imprecision. Sex has often served merely as a "tacked-on variable" (Apfelbaum & Lubek, 1976, p. 82) which would provide a between-group effect in a scientific world of null-hypothesis

testing that puts a premium on statistically reliable differences. Women could be added to a paradigm when a main effect or interaction effect was needed, or they could be eliminated as irrelevant when their behaviors went against predictions from universalist theory (cf. Macaulay, 1985; Lubek, 1979). They simply would come and go at the beck and call of the statistical and design needs of a researcher. Sometimes, however, differences between men and women were deliberately sought by design, because, as one of my psychologist friends has said, it is an obvious difference to look for, and after all one can observe, in most species, differences between male and female. But, for the most part, the sex variable remained largely untheorized and, therefore, was somewhat of an epistemological embarrassment. Psychology seemed to become an "angelical psychology" (Hurtig & Pichevin, 1986), a description alluding to the classic dilemma worrying medieval theologians and painters about representing the 'sex' of angels.

In brief, sex differences were often sought or serendipitously found without being predicted on the basis of theory, aside from the underlying basic lay belief that there are 'natural' differences between the sexes in human beings. That lay belief simply surreptitiously reintroduces as an unquestioned axiom the social exclusion which characterizes the relations existing in our society between men and women. And this, in turn, tends to naturalize what may, at least partially, be mainly an effect of a socially induced differentiation. Either way, universalized or naturalized, women are still being Ms-treated by, for example, mainstream psychological research.

But the epistemological stakes for psychology as a whole are much broader than the mere issue of sex differences. Psychology has here bypassed a potential theoretical challenge; it has missed the opportunity to try to analyze in earnest how the social and the biological dimensions are interconnected. The fact that psychologists have in this area been uncritically fallen back on the postulate of biological determinism without testing its validity and/or the limits of its applicability may indicate that we, as psychologists, have other blind spots as well. In fact, psychology seems to have considered without further examination biology to be the bottom-line theoretical explanation in other areas of research as well. I shall come back later to the argument that the concept of gender which emerged as an alternative from this critical exploration into the traditional research of sex differences constitutes an analytical tool which leads potentially to a major paradigmatic shift.

Advocacy, objectivity, and value neutrality. Feminist scholarship has often been criticized and denied legitimacy in the kingdom of science because of its alleged advocacy. And indeed, even though the research itself relies on sound empirical logic, women who carry it out in fact have vested interests in its outcome and share common interests with the women who constitute the object of their inquiry. But what about research in which male researchers

ignore one half of humanity, or treat women differently because they assume them to be and behave differently? Even if such research follows the logic of empiricist positivistic investigation, is it really advocacy free? Where can we find the so called 'value free, neutral' science?

The mystifying nature of the claim that science is neutral and objective has of course already been called into question by various Marxist and radical scholars in the early 70's, (Plon, 1974; Apfelbaum & Lubek, 1976; Israel, 1979). But most radical critics at that time did not go the one necessary step further to ask how sexist biases could also be considered 'serious' cues for identifying ideological biases. To give just one example, Israel (1979) has sharply criticized certain social psychological theories, such as social comparison theory, for reflecting only a middle class view of 'Man'. While he soundly shows the ideological bias inherent in such a narrow 'class' focus, he does not discuss the bias due to the ignoring of women in these formulations.

If the concept of objective knowledge is further impugned and abandoned (cf. Apfelbaum, 1989), the modalities by which scientific knowledge is being generated must be reexamined. Minton (1986) has argued that an alternative to the positivist paradigm would be one in which "the scientist should prescribe what ought to be because knowledge is value laden and should serve humanistic concerns such as emancipation and moral responsibility" (p. 260). But such analysis in terms of oppositions between antithetic epistemological positions — positivism *versus* sociorationalism, for example — or in terms of pendulum swings in scientific practice seems to me misleadingly reductionist. For me, as for a certain number of European feminists, the notion of 'transparency' about our biases and current belief systems offers a real heuristic potential for generating knowledge as solid and pure as positivistic-based knowledge claims to be.

Since advocacy or holding a certain belief system about one's subject matter and about one's way of establishing scientific truth is de facto unavoidable, this bias should be explicitly stated and worked into the research design. Transparency with all beliefs explicitly stated and subject to visible scrutiny, forces one to evaluate constantly the evolving knowledge base against all one's research assumptions, overt or covert; and the basis of our knowledge is perpetually questioned, if there is ongoing reflexive evaluation of both one's scientific praxis and one's subject matter, then one's position of advocacy serves as a built-in critical, reflexive and regulating tool and actually becomes part of the 'normal science' methodological arsenal.

An example may perhaps illustrate what I have in mind. One of the leading themes at the European Conference on Feminist Studies held in Brussels in February 1989 was that we had to abandon thinking in general terms of 'women' as a homogeneous, monolithic category. Women come from different social and cultural backgrounds and, in not taking into account their diversity, we run the risk of falling back into the trap of the 'universal

woman' much in the same way as still one indiscriminately studies 'universal man' in many areas of mainstream psychology.

In the most recent issue of the review *"Les Cahiers du G.R.I.F."* devoted to "Le genre de l'histoire," Riot-Sarcey, Planté, and Varikas (1988, p. 21) explain why it is so important to look back at biographical data and case studies in history. As long as we look at women in general rather than examining individual cases in their specific political and socio-cultural background, we may too easily fall back into a reductionist explanation and run the risk of reducing women to their lowest common denominator: le féminin. And this then leaves us with the old mystifying alternative. We can either, on the one hand, restrict women to a simple biological category, or else, on the other hand, lump them together in a 'class' or ideological grouping.

But I want of course to take the transparency/advocacy/objectivity issue beyond questions of simple inequality between men and women. What holds true for research on sex differences is equally true for research on race, childhood, violence, and so forth. In each of those areas, we need to really face the issue of how our shared sets of representations shape our collective beliefs about sex, race, childhood, violence, and so forth, and actively shape as well the nature of our scientific problematics, methods, theories, and praxis.

The gender issue. The emergence of the notion of gender or, more precisely, the conceptual shift from sex to gender, represents, in my view, a major theoretical deconstructive/reconstructive step with broad epistemological implications for all psychology.

The emergence of gender indicates the shift away from the atheoretical variable sex, or more precisely, a shift away from an underlying determinism and biological *a priori* about sex. Gender is, to my mind, a different kind of variable, what Unger (1979) defines as one that produces "effects because of generalized socio-cultural assumptions about universal biological processes" (p. 1092) that is, what she calls "biosocial variables." In other terms, it means that the issue is no longer whether differences between sexes exist, but rather how to account for the consequences of the implicit theories of sex differences (Hurtig & Pichevin, 1986, p. 323) which we hold both as social psychologists and as social interactors.

But these general social representations about the distinctive positions that men and women hold and/or should be given in a given society vary from culture to culture; therefore, the way in which the biological sex characteristics are constructed into gendered identity also vary from culture to culture. For example, in some Inuit communities, the biological sex is socially meaningless during the childhood years. Until puberty a baby is socialized as a boy or a girl independently of his or her biologically defined sex, but according to socially determined requirements and needs of the family and/or the community (Mathieu, 1989). It is likely that those child-rearing conditions will affect the nature of the social relations of people during adulthood.

Furthermore, the differential positions of men and women also vary within a given society with the political, economic and sociological changing requirements of that society. During World War I, women entered the factories and were integrated into higher education (many got college degrees); but when men returned from war, the women were sent back home. Incidentally, in the area of self or 'identity', no one to my knowledge has yet studied in earnest what it does to a woman's self-image to be granted a status in the work place only when it suits one's family or the changing economic needs of society.

More generally, if biology is no longer destiny and if the way in which society treats men and women varies over time and culture, then, on a broader scale, the contemporary study of gender also calls for a critical reevaluation of the division between disciplines. It is no longer possible to separate psychology from history, from politics or economy. Nor is it possible to assume that social psychology can be subsumed as a subdiscipline of psychology, the 'social' being just another relevant variable.

If gender can be considered as an analytical tool of major importance, it is because it helps rethink all differences in terms which reach far beyond the old dualism between the social and the biological. If we acknowledge the hierarchy which implicitly governs the social relations between sexes, as such a hierarchy implicitly governs the social relations between Blacks and Whites, or between the old and the young and probably many other social dichotomies, if this hierarchy shapes and mediates the relation between the subject and his environment, we can no longer fall back on an explanation based on a simple biological determinism and leave out the social power dimension which underlies the very assumption of hierarchy. Even biology can today no longer be considered a stable reliable reference. Biological motherhood can no longer be defined unambiguously. The consequences of the new advances in medicine and biology starting with in vitro fertilization and going all the way to surrogate mother-implantation make it increasingly difficult to give a univocal unambiguous definition of the concept of mother. The mother could be the woman who provides the ovules or the surrogate person who carries the foetus through pregnancy. On the contrary, the man who provides his sperm to an in vitro fertilization knows with no ambiguity that he is the father. Paradoxically, then, in certain cases, biological fatherhood is less difficult to determine than biological motherhood. It becomes increasingly difficult to bypass the symbolic dimension which we have taken great care to hide up to now in the wings of behavioristic and even cognitive psychology. These facts challenge some of our most deeply cherished categories of thought.

Finally, let us examine one last implication of the shift from sex to gender. If biology can no longer be considered as destiny for women, neither is it any longer destiny for men. And if gender identity is culturally and

historically bound for women, it is equally culturally and historically bound for men. If women have been the missing half of the social sciences, men should not remain its mythical and often mystifying half. This means that as social scientists we must be equally interested in the way a given society shapes — and possibly shapes differently — the social development of men as well as women; men as well as women should become explicitly our subject matter.

Such assumptions may potentially lead to a more integrated knowledge base representing both halves of humanity and dealing with processes which equally shape them both. It is also the condition which allows us to conceive of a problematic in which sexism, ageism and racism are no longer studied as simple sex differences, age differences and ethnic differences; that is a problematic which no longer takes implicitly for granted: a) that both terms of the difference have to be evaluated against one single and same set of social references and b) that the two terms of the difference are connected by some kind of hierarchical order. On the contrary, a theory of diversity could take into account the processes by which a range of differences emerge and become functional in social relations, for better or worse.

References

Apfelbaum, E. (1986). Prolegomena for a history of social psychology: Some hypotheses concerning its emergence in the 20th century and its raison d'être. In K. Larsen (Ed.), *Psychology and Ideology*, (pp. 3-13). New York: Ablex.

Apfelbaum, E. (1989). Voices from the other half: Psychology's responses to women's concerns. Invited address at the Annual British Psychological Society's Conference. Saint Andrews.

Apfelbaum, E., & Hadley, M. (1986). Leadership Ms-Qualified: II. Reflections on and initial case study investigation of contemporary women leaders. In C. Graumann & S. Moscovici (Eds.), *Changing conceptions of leadership*, (pp. 199-221). New York: Springer-Verlag.

Apfelbaum, E., & Lubek, I. (1976). Resolution or Revolution: The theory of conflicts in question. In L. Strickland, F. E. Aboud, & K. J. Gergen (Eds.), *Social Psychology in Transition*, (pp. 71-95). New York: Plenum press.

Battershy, W.S. (1956). Unilateral "spatial agnosia" (inattention) in patients with cerebral lesions. *Brain, 79*, 68-93.

Collin, R. (1988). Introduction: Sexe et savoir. *Les Cahiers du G.R.I.F.* (Le genre de l'histoire). *37/38*, 5-7.

Farge, A. (1984). Pratique et effets de l'histoire des femmes. In M. Perrot (Ed.), *Une histoire des femmes est-elle possible?* (pp. 17-36). Paris: Rivages.

Gergen, M. M. (Ed.) (1988). *Feminist thought and the structure of knowledge.* New York: New York University Press.

Hurtig, M-C., & Pichevin, M.-F. (1986). *La différence des sexes: Questions de Psychologie.* Paris: Tierce.

Israel, J. (1979). From level of aspiration to dissonance. In A. Buss (Ed.), *Psychology in social context.* New York: Irvington Publishers.

Lubek, I. (1979). A brief social psychological analysis of research on aggression in social psychology. In A. R. Buss (Ed.), *Psychology in social context,* (pp. 259-306). New York: Irvington (Halstead/Wiley).

Lubek, I., & Apfelbaum, E. (1987). Neo-behaviorism and the "Garcia effect": A "social psychology of science" approach to the history of a paradigm clash in psychology. In M. Ash & W. Woodward (Eds.), *Psychology in the twentieth century thought and society,* (pp. 59-91). Cambridge: Cambridge University Press.

Macaulay, J. (1985). Adding gender to aggression research: incremental or revolutionary change? In V. E. O'Leary, R. H. Unger, & B. S. Wallston, (Eds.), *Women, gender and social psychology,* (pp. 191-224). Hillsdale, NJ: Erlbaum.

Mathieu, N. (1989). *Communication au Colloque "Sexe et Genre".* Paris: C.N.R.S.

Minton, H. L. (1986). Emancipatory social psychology as a paradigm for the study of minority groups. In K. S. Larsen (Ed.), *Dialectics and ideology* (pp. 257-277). New York: Ablex.

Plon, M. (1974). On the meaning of the notion of conflict and its study in social psychology. *European Journal of Social Psychology, 4,* 389-436.

Riot-Sarcey, M., Planté, C., & Varikas, E. (1988). Femmes sujets de discours, sujets de l'histoire. *Les cahiers du G.R.I.F.* (Le genre de l'histoire), *37-38,* 21-23.

Sachs, O. (1987). *The man who mistook his wife for a hat.* New York: Harper and Row.

Scott, W. (1987). History and difference. *Daedalus, 116,* 93-119.

Unger, R. (1979). Toward a redefinition of sex and gender. *American Psychologist, 34,* 1085-1094.

Unger, R. (1982). Advocacy versus Scholarship revisited: Issues in the psychology of women. *Psychology of Women Quarterly, 7,* 5-17.

MENTAL REPRESENTATION AND MEANING: ARGUMENTS AGAINST THE COMPUTATIONAL VIEW[1]

William E. Smythe

SUMMARY: The present paper surveys and discusses some notable recent critiques of the computational theory of mind. Computational environments normally include at least three distinct levels: *physical implementation, formal computation*, and *semantic interpretation*. The computational theory of mind is problematic insofar as it attempts to collapse the semantic level onto the other two levels. The problems are brought into focus by discussing three recent critiques of computationalism by John Searle (1980, 1984), John Heil (1981), and Hilary Putnam (1988). It is argued that mental representation, like more public symbolic activities, functions relative to the interpretive practices of a community.

Widespread access to computers has, by now, served to dispel certain of the more common misconceptions about how they work. The notion that there is any intrinsic connection between computation and intelligence has all but disappeared, for example, and, with it, the popular conception of the digital computer as a 'thinking machine' or 'electronic brain.' Among the more persistent misconceptions which still remain is the notion of an intrinsic connection between computation and meaning. This may account for much of the intuitive plausibility and uncritical acceptance of the computational theory of mind that is now felt in many quarters of the cognitive science community. Effective criticism of the computational approach to meaning and mental representation can only begin once this intuitive connection between formal computation and semantics is severed. The present statement is aimed in this general direction.

Computational Environments

To put the computational theory of mind in perspective, it is useful to make explicit three distinct levels at which to consider any computational environment, as displayed in Figure 1. At the bottom is the *physical level*, in which computational processes are realized or *implemented* mechanistically in the physical states and processes of some device. Next is the *formal computational level*, in which computational properties, as such, are specified in terms of purely formal syntactic relations among symbols (ranging from those which define the primitive 'virtual machine' to syntactic definitions in
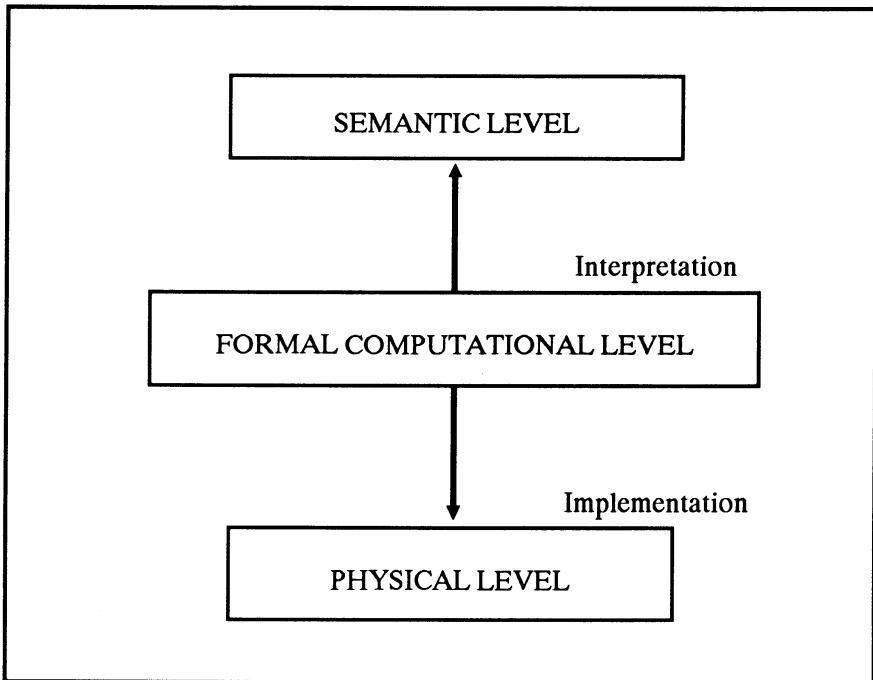
Figure 1. The structure of computational environments.

the various 'higher level' programming languages). Finally, there is the *semantic level*, in which computational symbols get *interpreted* in ways that are meaningful and useful to the programmer and system user.

The relation of *implementation* which maps computational structures onto their physical instantiations is, in principle, one-to-many, as proponents of modern functionalism are quick to point out. What is less readily acknowledged is that there is also a one-to-many relation of *interpretation* between computational structures and their semantic properties. Just as there is no limit to the number of possible ways in which a formally-defined computational system may be implemented in a physical device, so is there, potentially, an unlimited number of ways to interpret the system meaningfully. In practice, the interpretation of a computational system is determined by the shared conventions of a community of programmers and system users. As a number of commentators have recently pointed out, this form of interpretive activity is not unlike the meaning-fixing practices of natural language (cf. Heil, 1981; Smith, 1982).

For many of its practitioners, the computational theory of mind no doubt seems to be thoroughly consistent with conventional computational practice, indeed to be a direct extension of it. Actually, it represents a rather radical departure from conventional practice. This is most evident in the attempt of the computational theory, as strictly applied, to collapse the semantic level of

its computational environments onto the other two levels, thereby dispensing with interpretation altogether as a separate process. The motivation for this collapsing of levels comes from the fact that the computational theory does not want to claim merely that computational models of mental processes can be *interpreted* semantically, for that is something trivially true of any computational system; rather, the claim is that cognitive computational systems actually *generate* their own semantic properties, that their semantic properties are in some way "original" with the systems themselves, and not merely attributed by an outside agency (Pylyshyn, 1984).

One version of this claim, called 'strong AI,' holds that the formal level is, by itself, sufficient for its semantic properties. Another version that now enjoys wider acceptance is the "natural computation" approach, which holds that the formal level, together with certain 'natural constraints' about implementation, can fix semantic properties. Whatever may be the intuitive plausibility of either view, it is important to point out that neither has any precedent in conventional computational practice, which remains a thoroughly interpretation-laden enterprise in its day-to-day activities.

This computational reification of meaning is not only unprecedented in everyday computational practice, it is at odds with virtually every other form of symbolic activity in existence. No other type of symbol system that people use has what Putnam (1988) terms the "magical property" that the formal properties of its symbols somehow determine their meaning. For any of the various interpersonal symbol systems such as natural languages, pictorial and gestural systems, and the like, it is well understood that semantic properties are something attributed to or bestowed upon symbols. They are fixed, in a word, by a process of *interpretation*.

Critiques of Computationalism

The computational view of representational semantics assumes, in contrast, that semantic properties of representations can be founded somehow on *uninterpreted* formal structures and processes. Some of the more notable recent critiques of the computational view are based on challenging just this assumption. What follows is a quick survey of three such critiques and a discussion of their implications.

Searle. John Searle's (1980) "Chinese room" argument is perhaps the best known of the recent critiques of computationalism. The argument is based on the observation that computational processes are defined on formal syntactic properties of symbols and that these, by themselves, are not sufficient for the semantic properties of mental states. As Searle (1984) points out, in a recent statement of his position, "having mental states ... involves more than just having a bunch of formal symbols. It involves having an interpretation, or a meaning attached to those symbols" (p. 33). He illustrates

the point through the use of a thought-experiment in which a human agent instantiates a computational procedure defined on formal properties of a set of symbols, while remaining ignorant of their intended semantic interpretation. Thus, Searle argues, interpreted semantic properties of representations cannot be derived from uninterpreted formal properties.

Heil. John Heil (1981) makes a similar point in arguing for the general principle that no formal system can contain within its intrinsic structure, no matter how extensively articulated, its own rules of interpretation. One can, for instance, imagine various ways of augmenting the structure of a formal system with encodings of rules of interpretation, but these encoded rules would still themselves require interpretation, and so on, recursively, for any "meta-rules" for interpreting the rules of interpretation. Thus there is an essential gap between any formally defined system and its semantic interpretation that cannot, in principle, be bridged by any amount of formal structure. Formally encoded rules do not eliminate the need for interpretation but, rather, presuppose it.

Putnam. Among the most comprehensive recent treatments of the problem of interpretation in relation to issues of meaning and mental representation is Hilary Putnam's (1988) critique of modern functionalism — a view which Putnam himself had earlier helped to originate.

Interpretive practice is, in Putnam's view, generally governed by a principle of "charity", by which we take account of the normal processes of belief fixation and justification in making judgements about the meaning of various expressions. According to Putnam, "all interpretation depends on charity, because we always have to discount at least *some* differences in belief when we interpret" (p.13). For example, we can interpret the usage of certain words by young children and by members of other cultures in terms of our own usage, despite vast differences in the accompanying beliefs. It is argued, on the basis of this principle, that "sameness and difference of meaning cannot coincide with the presence and absence of any local computational relation among our 'mental representations'" (p. 15); in particular, it cannot simply be computed from representations of the contents of beliefs. Rather, meaning is constrained in a global way that is somehow capable of generalizing over substantial differences in belief. To take representations of the contents of propositional attitudes such as beliefs to be the basic 'building blocks' of cognitive meaning cannot be right, then, because judgements about meaning can take into account arbitrary differences among sets of beliefs.

Putnam goes on to argue that, in the general case, a complete computational formalization of interpretation is not possible, because it would have to encompass all conceivable conceptual schemes for interpretation and there is no way of knowing, in advance, what these might be.

Conclusion

There is much more to be said about these and other similar critiques of computationalism. Suffice it to say, for the present purposes, that these sorts of critiques should help to motivate a reconsideration of the role of interpretation in the domain meaning and mental representation. Mental representation, like normal computational practice itself, is perhaps best viewed, not as an autonomous semantic system but, rather, as something which functions relative to the interpretive practices of some larger community. As Heil (1981) suggests, "a theory of representation, whatever its details, must make essential use of the fact that representation is something accomplished by persons who employ a public mode of discourse" (p. 342).

Social constructionist theorists and ethnomethodologists have been advocating this sort of view for some time (e.g., Coulter, 1983). Recently some notable proponents of computationalism have found themselves making certain concessions in this direction as well. For example, Pylyshyn (1984) advocates a "coherence view" of representational semantics in which meaning is said to be constituted by the functional role of mental representations within the complete theory of a cognitive system's behavior, which is said to include an account of its responses to linguistic and other sorts of messages "that are considered to be interpreted" (p. 42). Similarly, Fodor's (1987) approach to the semantics of mental representation is based on a notion of "narrow content", which is said to be "radically inexpressible" and is, in fact, semantically inert until it is considered as "anchored" to the context provided by some appropriate linguistic community. No advocate of computationalism has yet been able to formulate an approach to meaning that resists appeal to such consensual practices — a view which would show how semantic properties could come to rest on fundamentally uninterpreted formal structures and processes. Given this state of affairs, the notion of purely "computationally constituted meanings" remains empty and unproductive; it is, I submit, a most unuseful fiction.

References

Coulter, J. (1983). *Rethinking cognitive theory.* London: MacMillan.

Fodor, J. A. (1987). *Psychosemantics: The problem of meaning in the philosophy of mind.* Cambridge, MA: MIT Press.

Heil, J. (1981). Does cognitive psychology rest on a mistake? *Mind, XC,* 321-342.

Putnam, H. (1988). *Representation and reality.* Cambridge, MA: MIT Press.

Pylyshyn, Z. W. (1984). *Computation and cognition: Toward a foundation for cognitive science.* Cambridge, MA: MIT Press.

Searle, J. (1980). Minds, brains, and programs. *The Behavioral and Brain Sciences, 3,* 417-457.

Searle, J. (1984). *Minds, brains and science.* Cambridge, MA: Harvard University Press.

Smith, B. C. (1982). *Semantic attribution and the formality condition.* Paper presented at the 8th annual meeting of the Society for Philosophy and Psychology, University of Western Ontario, London, Ontario, May.

COGNITIVE REPRESENTATIONS AND INTENTIONALITY AND THE REALISM-RELATIVISM CONTROVERSY

Sacha Bem

SUMMARY: A large part of this paper is devoted to a discussion about the way computationalists inflate internal representations to symbols in the mind. This is partly due to an unhappy fusion of formal (computational) systems and cognition, that is, knowledge in the full sense of the word. It is also a remnant of a form of realism that cannot be accepted. At the end of the paper I suggest, however, that this critique must not lead to the other extreme, relativism. Both sides take language as the heart of cognition. Analyzing intentionality rightly will show that this mental property does not consist only in thinking and speaking but also includes our active involvement in the world.

Cognitive representations were once conceived of as entities which were offered by the outside world to be accepted by the mind. In that conception for something to be represented was for that something to be depicted. John Locke compared the mind to a mirror (Locke, 1690, 2.1.25). The mind "can no more refuse to have, nor alter when they [the ideas] are imprinted, nor blotted out and make new ones itself, than a mirror can refuse, alter, or obliterate the images or ideas which the objects set before it do therein produce." In this conception there is a causal relation between the representation and what is represented. The senses are the gateways to the mind. The working of the eyes is paradigmatic; their function is like the lens of a *camera obscura* which projects what it meets in the mind. If that is done well, the mind contains the truth, that is, there is a correspondence between the projection and what it represents. That is what 'knowledge' is about; it deserves that name if it is true in this way.

This imagistic conception was the foundation of realism and although perhaps no one nowadays would endorse this idea for all knowledge, some of this line of thought is implicit in certain theories of cognitive functions like visual perception and memory. This is the case for the computational theory of mind where the idea of the passive reception of data still controls the relation between mind and reality.

On the opposite side relativistic theories reject the idea of the passive reception of data so vigorously that they think that what we believe and assert about the world appears to be totally dependent on social factors like rules, language and communication.

In this paper I will contend that, first, a confusion of two concepts of representation is responsible for this unhappy opposition of conceptions about cognition and reality. Second, that both sides have an incomplete

conception of intentionality. And, third, that both sides overstress the role of language in the relation between mind and reality.

In cognitive psychology two representational units are now widely accepted: images and language, that is, a form of mental language. As for images, in view of the objections against the old idea of mirroring one now speaks of isomorphism, that is, a structural similarity between the representation and the object represented. Conceived of as a correspondence of aspects of objects in the world and certain forms or structures in our brains, this idea of internal representation could safely be accepted. Here, 'to represent' means 'to replace' or 'to stand for' in a straightforward fashion meaning that the representation is caused by what is represented. The storage locations in a computer are filled by representations of the pressed keys of the keyboard connected to it. The turning of the hands of the clock represents the movements of the cogwheels and the spring inside the watch. This mechanistic meaning of 'representation' is without problems because it is without a 'higher order' interpretation. Discoveries in our visual perceptual system, for instance, sustain the idea that physical aspects of objects, that is, structures of light, are physiologically represented in the visual cortex via the light-sensitive cells in the retina and neuronal pathways in our brains.

Cognitivists and computational cognitivists, however, consider this kind of representation as too empty. This is obvious because cognition has to do with understanding and interpretation; cognition is knowing that this or that is the case, is seeing *as*. So, it is thought, what is going on in our heads must be more than the processing of these physical representations. There is more to seeing *as* cows and horses in a pasture than optics, photochemistry, neurophysiology, and the whole train of that kind of representation can explain. This is the lesson for cognitivists from the post-positivistic criticism directed against naive empiricism. Hanson wrote: "At least, the concept of seeing embraces the concepts of visual sensation and of knowledge." (Hanson, 1958, p. 25). And this holds true for all our cognitive functions; they include more than physical processes.

However, according to the cognitivists thinking and having beliefs is something we carry out with our heads, so that one way or another, those cows and horses in the pasture in the outside world must be represented in our heads. Cognitive or mental representations, they think, must be more than those physiological representations. How do they arrive at this conception of a representation with interpretation?

They do three things. First, as a solution to the mind/body- (mind/brain) problem they opt for a weaker form of materialism which allows them to disregard brains: not the identity of mind and brain; not a type-type-materialism; but functionalism provides them with a cognitive middle level; a design or architecture that can be studied as it is and that operates so autonomously that it could in principle be taken with you as the software on

a floppy disk from machine to machine, and from machine to animal and human being.

Second, they think that the units of that cognitive design represent the world. But this time those units are representations *with* interpretations. They inflate representations to symbols. And the computationalists want to have it both ways. They consider cognition as a computation of mechanistic systems and they want interpretation or meaning within those systems. In their view, the mind is both a mechanistic system and a network of symbols. The alleged cognitive middle-level bears the ambiguity of the unhappy pairing of the physiological neighbor and the volatile mind.

Third, the computationalists think they can do this by claiming that the cognitive design is a formal system. I'll come back to this conception of a formal system later.

But, what are symbols? And what is my reason for contending that the computationalists do inflate the physiological representations to symbols? What are their reasons or presuppositions for committing this inflation? It is these three questions I would now like to pursue.

What are symbols? A symbol is something that stands for and replaces something else. As far as this goes, it could be the definition of a representation I mentioned earlier. This 'stare pro' or 'stand for' appears in every definition of symbol or of the more generic concept 'sign'. However, the element that is quite often left out is that a symbol or a sign, in the words of Charles Sanders Peirce, "is something that stands to somebody for something in some respect or capacity" (Peirce, 1931/1935, Vol. 2, par. 228). What Peirce is actually defining is: "Someone takes x to be a sign of y" rather than "x is a sign of y" (Alston, 1964, Ch. 3 & 1967, Vol. 7, p. 437).

It could be that this 'subject'-element is too strong for 'signs' in general — after all, x could be a sign of y without somebody seeing or understanding it — but it cannot be disconnected from symbols. The concept 'symbol' implies that there is somebody who links up the connection, who does the interpreting. The relation of a symbol and that for which it stands needs interpretation, otherwise it could not be a symbol. "Symbols without interpretations are blind" is Schwartz's paraphrase of the famous dictum by Kant (Schwartz, 1984, p. 1049). Something cannot be a symbol without there being someone who produces or understands the symbolic force, because the relation of a symbol and that for which it stands is not as natural as, say, smoke and fire.

If we talk about symbols in general, we mean symbols that we as human beings are able to understand; and for someone to understand a particular symbol she needs to share the life-world of the one who uses that symbol. Now, life-world is a rather complicated concept and I cannot elaborate on this here (Bem, 1989). But allow me to say that the third place in the triadic

symbol-relation is a complete human being with all the experiences needed for the symbol understanding.

This introduces the answer to my second question: why do I think the computational cognitivist is guilty of blowing up representations to symbols? When cognitivists claim that the cognitive system manipulates symbols, who ties up the symbolic relation; who understands the symbols? The system? If the 'system' stands for a 'human being', then nothing is wrong. But I suspect that by a cognitive system is meant a subsystem, a system that operates in our heads, but behind our backs, so to say. Small wonder that this idea always has to cope with the homunculus-objection, an objection that, for instance, lies at the heart of Skinner's contempt for cognitive psychology. If we cannot accept that there is a representation of a complete human being working inside our heads, such a system or subsystem cannot manipulate symbols at all.

In addition to this, when the computationalists talk about the mind as a symbol-manipulating system they claim that it is a formal system. The most radical formulation comes from Fodor (1980). In a formal system only the formal properties of representations are of importance, that is, it doesn't matter how or to what they refer.

> If mental processes are formal, they have access only to the formal properties
> of such representations of the environment as the senses provide. Hence,
> they have no access to the *semantic* properties of such representations,
> including the property of being true, of having referents, or, indeed, the
> property of being representations *of the environment*. (Fodor, 1980, p. 314,
> emphasis in original).

This elimination of the semantic relation Fodor calls "methodological solipsism." I'll come back to that. The computationalist wants to have it both ways: a) representations that are formal, not referring to the outside world, defined as dyadic relations, and being formal, susceptible to computations; and b) representations that are really cognitive and thus symbols; but symbols are triadic relations.

I have now come to my third question. Why this inflation of representations to symbols? I shall mention four reasons or causes.

In the first place, I think that the computationalists wrongly identify the cognitive middle-level with a formal system. The appeal to cognitivists of formal systems lies in the fact that you can compute with formal systems. Now, what is a formal system? In formal sciences such as logic we use symbols. Those symbols, we are told, have no content; that makes logic formal. Logic is about the form of arguments, not about content. The symbols are enough to show the trick. But are those symbols really without reference? Of course not; they are true symbols, or triadic relations. The tokens of a formal system are symbols, indeed, with a meaning given to them by logicians. And she who understands logic does refer to something. The meaning of the tokens is that they are neutral marks and that many interpretations are possible; they can stand for anything. So a formal system is, in fact, a system of symbols. But we

can't find formal systems in our heads, at best we can look upon the cognitive system *as* a formal system, but that's not what computationalists aim at. They claim that a cognitive system *is* ontologically a formal system. As in the case of symbols and also of rules, however, we can't find symbols, rules or formal systems in nature. There are no physical symbol systems. But, of course, we can see any physical thing as a symbol of something else. Symbols, rules, and formal systems are products of our minds. So, in order to compute, computational cognitivists assume formal, but nevertheless symbol-systems in our heads.

The second reason for the inflation of representations to symbols is due to a confusion consequent to the ambiguity of the term 'representation' itself. As mentioned before, one meaning of 'to represent' is 'to stand for' in a straightforward fashion, as when a pattern of light is represented by the rate at which ganglion cells in the retina fire impulses. But we can use representation easily with symbolic force as in: The Arc de Triomphe in Paris represents the ideology of the Second Empire. Therefore, we should be careful not to overload internal representations with symbolic force in such a way that such a representation carries the meaning of what is present to the mind like the content of an act of thinking, as in "to picture to oneself, to imagine"; and in French in *"se représenter"* (Lalande, 1985, p. 920).

The third reason for the inflation is that cognition — developed at the outset as a theory by philosophical linguists and psycholinguists — has been identified exclusively with thinking and speaking and for that reason with the manipulation of words and propositions or mental sentences. And because words are symbols par excellence, representations are thought to be like words. This is wrong, I think, first of all because cognition doesn't consist only of thinking and speaking, but as pragmatists showed: a belief is something upon which a man is prepared to act. Thoughts without actions are dead. Cognition is also about doing things. Furthermore, cognition is not only knowing that, but also knowing how. And cognition is connected with motivation and emotion. The idea of cognition as thinking is a fallacy of theorizers and intellectuals. Secondly, meanings are not attached to words as they are in a dictionary. We, human beings, give words their meanings in using them. That is, I think, the essence of Wittgenstein's dictum 'Meaning is use' (Wittgenstein, 1953). Cognition is so exclusively identified with thinking, and thinking with the manipulation and combination of words 'in our heads', that it is hard to get rid of the idea that words, sentences, ideas and knowledge are located in our heads as parcels packed with meanings and ready for retrieval. And so our heads are furnished with symbols.

The fourth reason why cognitivists consider representations as the units of a natural system, but nevertheless, as symbols, is a more technical philosophical point. Fodor opts for methodological solipsism, as mentioned above. This is the result of his characterization of psychology. If someone's

beliefs are the causes of her behavior, as the cognitivist Fodor claims against the behaviorist, we are able to understand and to predict her behavior only on account of her beliefs. We are able to understand that Leonora hangs a rope of garlic at the window by referring to her beliefs and expectations about Dracula. Whether the vampire-count really exists has no effect upon the causal power of Leonora's beliefs.

Fodor's technical considerations are the logical properties of intensional and extensional sentences. 'Leonora doesn't appreciate a visit from Dracula' is an intensional sentence that tells us what the credulous girl has in mind but nothing about the existence of the vampire. But the sentence: 'The Transylvanian Count Dracula is a vampire' has an existential import and is for that reason an extensional sentence. Fodor claims that those intensional sentences that do not refer to the outside world are most important for psychology, the study of mental states. In Fodor's theory cognitive representations are both symbols and formal, that is, without a subject referring to the world.

This being directed towards the world is widely considered as the specific property of mental phenomena, that is, intentionality. Fodor, however, reduces this psychological property of 'intentionality' to the logical property of sentences, 'intensionality', in order to get his formal computational system.

The assumption that language or words, a language of thought and mental sentences for that matter, carry their own meanings and that in the processing of formal computational systems somehow symbols pop up, is playing tricks on the computational cognitivists. This is a residue of empiricism and naive realism. Computational cognitivists join certain philosophers of language in the analytic tradition who, with respect to the meaning of meaning stress sense (intension) and disregard reference (extension) and the use in a context. In this same frame of mind intentionality is being reduced to certain logical characteristics of intensional sentences.

However, intentionality is our engagement in the world. We are involved and interested in the world, and in doing this we use symbols. It is not words or sentences that *have* meanings, not formal systems, but we in our thought, action and communication use symbols and *give* meanings. That is what human intentionality is about; it is an interplay of our thinking, our moving body, and our intersubjective engagement with other persons. It is a property of beings with this complex of experiences in the world.

Are we left then with a non-materialistic mind and do we have to accept relativism? Relativists and so-called social constructionists (Farr & Moscovici, 1984) consider the mind as a social product. Inspired by hermeneutics they make much of communication (to mention only Gadamer, 1960; Rorty, 1979; & Habermas, 1981), and they too like the computationalists, but in there own way, suppose that our engagement in the world is exhausted by using language. In their view, however, language is not a bundle of objective representations coming in from the outside world and forming

symbols manipulated by a formal system in our heads, but language is a social-subjective phenomenon. Symbols and meanings originate in communication and in social interaction. So, all our thoughts, concepts, theories, and so on, are social and truth is a matter of concensus, claim the relativists.

I would like to argue that we can overcome the realism-relativism controversy in analyzing rightly the concept of intentionality. Our involvement in the world is not exhausted by language and communication. Our mind is not a formal system; it is not a so-called physical symbol-system.

References

Alston, W. P. (1964), *Philosophy of language*. Englewood Cliffs, NJ: Prentice Hall.

Alston, W. P. (1967). Sign and symbol. In P. Edwards (Ed.), *The Encyclopedia of Philosophy*. 8 vols. (pp. 437-441). New York: Macmillan.

Bem, S. (1989). *Denken om te doen. Een studie over cognitie en werkelijkheid*. Leiden: Faculteit Sociale Wetenschappen.

Farr, R. M., & S. Moscovici (Eds.) (1984). *Social representations*. Cambridge: Cambridge University Press.

Fodor, J. A. (1980/1981). Methodological solipsism considered as a research strategy in cognitive psychology. *Behavioral and Brain Sciences, 3*, 63-73. Reprint in J. Haugeland (Ed.) (1981). *Mind design. Philosophy, psychology and artificial intelligence* (pp. 307-338). Montgomery, VT: Bradford.

Gadamer, H-G. (1960). *Wahrheit und Methode*. Tübingen: Mohr. (G. Barden & J. Cumming, Trans. & Ed., 1975. New York: Seabury Press.)

Habermas, J. (1981). *Theorie des kommunikativen Handelns*, 2 vols. Frankfurt a/M: Suhrkamp.

Hanson, N. R. (1958). *Patterns of discovery*. Cambridge: Cambridge University Press.

Lalande, A. (Ed.) (1985). *Vocabulaire technique et critique de la philosophie* (15th ed.). Paris: PUF.

Locke, J. (1690). *An Essay concerning human understanding*. (A. C. Fraser, Ed.). New York: Dover.

Peirce, C. S. (1931/1935). *Collected papers* (Vol.2), Ed. C. Hartshorne & P. Weiss. Cambridge, MA: Harvard University Press.

Rorty, R. (1979). *Philosophy and the mirror of nature*. Princeton, NJ: Princeton University Press.

Schwartz, R. (1984). 'The' problems of representations. *Social Research, 51*, 1047-1064.

Wittgenstein, L. (1953/1968). *Philosophical investigations.* (G. E. M. Anscombe, Trans.). Oxford: Blackwell.

THE COMPUTATIONAL THEORY OF MIND AND CONSTRAINTS ON THE NOTIONS OF SYMBOL AND MENTAL REPRESENTATION

René J. Jorna

SUMMARY: In this article I will show in what sense symbol systems and representations are necessary theoretical aspects in the study of human cognition. The article consists of three sections. In the first section I will discuss three central notions in cognitive psychology and I will demonstrate how these notions can be transformed into criteria in order to characterize and compare theories of mental representation. In the second section I will demonstrate the use of these criteria with Kosslyn's (1980) theory of pictorial representations.

The Symbol System Hypothesis

In recent cognitive psychology the processing of information is viewed in terms of computation, symbol manipulation and mental representation. According to Pylyshyn (1984, p. 54) *computation* can be seen as "the rule-governed transformation of formal expressions viewed as interpreted symbolic codes." Fodor (1983) remarks that computation is the execution of transformations on mental representations. The interesting point concerning computation is not the meaning of the term, but its veiled assumption. The assumption is that the processing of information in human cognition takes place independently of the physical or physiological characteristics of the system. Consequently, it is stated (Pylyshyn, 1984; Jackendoff, 1987) that cognitive psychologists primarily study the functional aspects of cognition, or, stated in computer terms, only the software and not the hardware.

Concerning the notion of *symbol manipulation*, Newell and Simon (1976) have argued that humans and computers can be seen as parallel versions of physical symbol systems. The core of this notion is that the behavior of a system is flexible and referential and that its procedures are effective in view of certain goals. "The processing of symbols is the basis of all intelligent action" (Pylyshyn, 1984, p. 51), and this means that the notion of a discrete atomic symbol is fundamental in cognitive psychology. Symbols are the constituents of our thought. Apart from the research of Newell and Simon, Pylyshyn, and very recently Jackendoff (1987), the importance of symbols often remains implicitly in cognitive theories.

One minor thing has to be said about symbols. What is meant by symbol is not that it is just a mathematical symbol or a religious symbol. The notion of symbol has to be understood in a very broad sense. As the philosopher Goodman says:

> ... it covers letters, words, texts, pictures, diagrams, maps, models, and more,
> but carries no implication of the oblique or the occult. The most literal
> portrait and the most prosaic passage are as much symbols, and as 'highly
> symbolic', as the most fanciful and figurative. (Goodman, 1981, p. XI).

This statement explains why there is so much interest, lately from fields such as semiotics for cognitive science and vice versa.

It is often stated that cognitive psychology is the study of *mental representations*. Considering various cognitive theories, we find expressions like: propositional representations, pictorial representations, procedural representations, representational content, and processes of representation. The usual meaning of representation is that something is standing for something else (Palmer, 1978). This is very general and vague, but it covers at least two aspects of representation. The first aspect is representation in the sense of description, that is to say that it is a formal scheme or a symbol set (Marr, 1982). The second aspect is representation in the sense of depiction. That is to say that it is not the symbol set which is important, but that which the symbol set represents. A less common meaning of representation is representation in the sense of procedure or process (Norman & Rumelhart, 1983) where the dynamical character of information is emphasized.

As a matter of fact, the concepts of computation, symbol system, and mental representation, mentioned above, are rather familiar in cognitive psychology. This being the case, there are two problems with these concepts. The first problem is that these concepts are very ambiguous, as I have shown in the case of representations. Specifically, sometimes two cognitive theories use the same concept with two different meanings, whereas in other cases different concepts are used with the same meaning. The second problem, that follows partly from the first one, concerns the fact that there is no unified theory of cognition. There are a lot of cognitive theories, but they contradict one another or are overlapping one another (see, e.g., Tulving, 1983, & Kintsch, 1974 on episodic memory).

Given the fact that there are commonly accepted notions in cognitive psychology and that it is not a fruitful matter to have a lot of contradicting theories, the problem is how to bring the latter issue in accordance with the former one. As a solution I have tried to formulate five criteria in order to characterize and to compare theories of human cognition. These criteria, that I deduced from the concepts of computation, symbol system and representation themselves, can be seen as a sort of instrument in order to evaluate rather than to condemn cognitive theories.

The first criterion is related to the functionalist assumption in the notion of computation. I will call this criterion: the criterion of the levels of description (c1). In explaining and predicting human cognition three levels of description can be distinguished. The philosopher Dennett (1978, 1987) has called these: the physical stance, the design stance, and the intentional stance. Regarding the physical stance, all that matters is the description of the system

in terms of the laws of nature, for example, in mechanical terms or in neuro-physiological terms. In the design stance or the functional level of description one is looking at the combination of components and sub-components of the system. The important aspect of the functional level is the input-output connection in the system. An example of a description at this level is: 'I could not retain the ten digit telephone number, because my short term memory has a limited capacity'. A third level of description is the intentional level. In this case the behavior of a system is explained by reference to a notion of rationality or intelligence. One ascribes to the system wishes, desires, plans, and goals. Take, for example, a chess computer. One could describe the behavior of that system by supposing that it wishes to win or that one of its higher goals is to win. Intentional descriptions are problematic because one tries to explain rationality in terms of rationality and this implies a circular line of reasoning. The important aspect of this criterion is that the levels of description should be separated at a conceptual level as strongly as possible. Confusion of concepts at the physical and the functional and at the functional and the intentional level should be avoided.

The second, the third, and the fourth criterion are related to the notion of representation. The second criterion is called the morphological criterion (c2) and is about the various meanings of the concept of representation. If representation, among others, means that something is standing for something else, then it is important to know which domains are involved and which elements are associated with the domains (Palmer, 1978). So, when in a cognitive theory a mental representation is mentioned, it follows that the cognitive entities should be made clear, at least in the representing domain. Furthermore, the various meanings of representation should be discerned. These meanings are, firstly, representation in the sense of a formal scheme, in which case the cognitive entities matter, secondly, representation in the sense of procedure or process and, thirdly, representation in the sense of a depiction. In most cognitive theories this last sense of representation presupposes two domains, of which it is said that the representing one shows a resemblance with the represented one. From this it follows that similarity or resemblance should be a necessary condition for representation. In my opinion this implication is totally wrong and it commits cognitive psychology to study the represented domain instead of the representing domain, that is to say the mental domain.

The third criterion is called the criterion of n-place predicates (c3). It is directly connected to representation in the sense of depiction. It is very enlightening to formulate representation as a predicate that takes several arguments, because in this way one can examine a theory in order to decide which domains are involved. If representation is a two-place predicate it takes two arguments. Without precisely indicating the domains x, y and z, two examples of two-place predicates are: y is a representation *of* x (this diagram is a representation of a tree) and y is a representation *for* z (this is an image

for my mind). If it is a three-place predicate it takes three arguments. An example of a representation as a three-place predicate is: y is a representation *of* x *for* z (this mental image of a tree is a representation of a real tree in (or for) my mind('s eye)).

The fourth criterion is called the criterion of the routes of reference (c4) and is also closely connected to representation in the sense of depiction. This criterion explores the nature and the direction of the reference of the represented and the representing domain. The normal interpretation of reference is that it is a relation from symbol to object, which is called a denotation. Two important non-denotational interpretations of reference in relation to cognitive theories, are exemplification and expression. In the case of exemplification something refers to something else by literally illustrating certain properties. The small swatches of cloth in a tailor's booklet exemplify certain properties (Goodman, 1981). So, exemplification is reference plus possession. Expression is very close to exemplification. It does not illustrate literally but metaphorically. We could say that, for example, coffee exemplifies brown, but not that coffee expresses brown. In the case of expression it is quite normal to say that the ninth symphony of Beethoven expresses 'friendship', but not that it exemplifies it. Beethoven's symphony *has* metaphorically, but not literally friendship. The criterion of the routes of reference results in a distinction in denotation, exemplification and expression as different relations between domains.

The fifth and last criterion concerns the concept of symbol in symbol sets (c5). Goodman has developed an instrument to evaluate symbol sets (Goodman, 1981; Jorna, 1988). His main interest was how to define a symbol set such that it could be seen as a notation. Notational systems have the advantage of enabling unique identification. In the case of a notation it is possible to give an authoritative identification from work to work and from performance to performance. Because notational systems fulfill certain syntactic and semantic requirements, they can be discerned from other symbol sets. If a symbol set is syntactically disjoint (i.e., that it is possible to say whether two symbols are replicas of one another) and has a syntactically finite differentiation (that it is possible to decide whether a mark belongs to the one or the other symbol), then the symbol set is a notational scheme. Examples of this are the Roman and the Japanese alphabet and the Arabic number system. If a notational scheme also fulfills the requirements of unambiguity, of semantic disjointness and of finite differentiation, then we are dealing with a notational system. Examples are the Laban-notation for dance, the zip code and the musical scores.

The five criteria mentioned above, that were derived from the central notions in cognitive psychology, make it possible to characterize, to evaluate, and to compare different cognitive theories in terms of these criteria. What I suggested in discussing these criteria is that there are ways to make an

inventory of basic assumptions in cognitive psychology. A conceptual analysis of central notions in cognitive psychology makes it possible to dissect various meanings of terms upon which everybody seems to agree. The dissection yields interpretations of the notions of symbol and representation that could be seen as indicators of positions. I call these the valences of the criteria.

Kosslyn, Mental Images and the Computational Theory of Mind

It is impossible to discuss here an example of a comparison of two cognitive theories, in depth. To give an impression of what the results in applying the criteria above look like, I will give an evaluation of Kosslyn's (1980) theory of mental images. The reason to take Kosslyn is that mental images seem to be of a special nature. Symbol structures and mental images seem to exclude one another. That this is wrong will be shown by giving a description of the main starting points of Kosslyn's theory in the light of the five criteria. In the end of this section, the ideal (and necessary) valences of a computational theory of mind will be discussed.

To understand the importance of mental images — sometimes called 'pictures in the head' — try to imagine the following situation. Suppose someone asks you whether the bell on the front door of your house is on the right or on the left side. You answer this question by firstly making a picture of the front door in your head and then making some internal transformations towards an appropriate mental representation (see also, Shepard & Metzler, 1971). Formulated in terms of the processing of information, what people do in manipulating mental images is that they combine various forms of mental representations, that is to say symbol structures, with one another and with processes that operate upon these representations or structures.

The central theme in the research on mental images concerns the nature and the structure of mental images. The research question is: what occurs inside the human mind when mental images are generated and how are these images combined with episodic and semantic structures? According to Kosslyn:

> What researchers usually mean when they talk of having pictures in one's head is that one has retrieved, or generated from memory, representations like those that underlie the experience of seeing. (...) Image representations are like those that underlie the actual experience of seeing something, but in the case of mental imagery these representations are retreieved or formed from memory, not from immediate sensory stimulation. (Kosslyn, 1980, p. 18).

Kosslyn's model of mental images started as a computer model. After that, he validated and adjusted his model by doing experiments on mental imagery tasks with human subjects. A description of the model, follows.

The model is based on the metaphor of the cathode ray tube — the television screen — and consists of a visual buffer and several memory structures in which various forms of mental representation are present and upon and between which various mechanisms are active.

> Let us start with an analogy: What if we think of images as being like displays on a television monitor screen attached to a computer? The computer can generate images on the screen from information that is not picture like; data that are stored as symbols in the computer's memory emerge on the screen in pictorial form. (Kosslyn, 1984, p. 21)

According to Kosslyn, the mental image is present in the visual buffer, where it is called a surface representation. This representation is like a quasi-picture with the appropriate spatial structure and the suitable ratio of the several parts that constitute the picture. The generation of mental images is done in two steps. The first step consists of the retrieval of a skeletal structure — Kosslyn calls this a literal representation — which is constantly informed about the parts of the image in the visual buffer. The second step is the precise elaboration of the parts of the image in the cognitive system, in which an appeal is made upon a layer of representations below the one of the literal representations, which Kosslyn calls propositional representations. These last representations constitute the fundamental structure of memory for images. Skeletal and propositional representations are called deep representations in contrast to surface representations.

Characteristics of the surface representations are its co-ordinate space, the fact that the buffer itself consists of grains of different size and the fact that the image itself is created by composing parts, not simply by projecting a stored slide (Kosslyn, 1980, p. 141). Skeletal representations form part of long-term memory and its storage is in non-visual entities; it is in the form of files, identifiable and accessible by their name. The format of skeletal represent-ations is a polar co-ordinate system in which every part of an image is indicated by an index. Propositional representations consist of lists. These lists are constructed from scenery or object parts and can be organized hierarchically or in a graph structure. The lists are searched in a serial manner, starting from the top. This is called 'association strength', because the list is arranged in such a way that strongly associated parts of an object or scenery are in the top of the list and can be activated very quickly after which they can be included in the image in the visual buffer.

Until now nothing has been said about the mechanisms that operate upon the various representations. According to Kosslyn, four fundamental processes are involved at and between different levels of representation. The PICTURE process generates the surface representation from the lower level representations, the FIND process searches the visual buffer looking for a particular object or part of an object. The PUT process "performs a variety of functions necessary to image a part at the correct location on an image. Finally, the IMAGE process is responsible for coordinating the activities of

the other three processes." (Eysenck, 1984, p. 185) Furthermore, there are several additional processes, such as ZOOM, PAN, SCAN, ROTATE, and others, that operate when special operations are required, for example, image inspection, image transformation or image adjustment.

As an illustration of what a determination of the criteria means for a cognitive theory, the following conclusions can be drawn. Concerning the criterion of the levels of description (c1), Kosslyn's theory is mainly formulated in functional terms. However, physiological and intentional levels of description are often used implicitly. This means that Kosslyn's theory is conceptually confounded. With respect to the morphological criterion (c2), concerning the different senses of the notion of representation and the elements involved in the representing and represented domains, it is questionable whether the entities in the represented domain are clearly formulated. Therefore, it is unclear whether he uses representation in the sense of a symbol set. What he does when he speaks about representation is that he uses the term in the sense of a procedure and in the sense of a depiction relation. However, with respect to depiction, there is no indication of resemblance between an inner and an outer domain. This means that concerning the criterion of representation as a n-place predicate (c3), pictorial representations, because they are built up from long term memory, are representations *for*. As a consequence of this the direction of reference is not a matter of denotation, but, also because different levels of representation are involved in constituting mental images, each level is an expression of another level. Finally, although Kosslyn tries to defend the position that representation is without symbol sets, it is clear that his model only functions if one assumes that there are structures which constitute representations. Kosslyn's unwillingness to define symbol structures in a more precise way, makes it clear that the symbol sets in mental imagery are neither notational schemes nor notational systems (c5).

Comparisons have been made for the theories of Marr, Kosslyn, Tulving, Anderson, Kintsch, and Schank and Abelson (Jorna, 1989) which gives a clear insight in what respect various cognitive theories differ from each other. This is one of the advantages of the approach that is proposed here. A second advantage is that an analysis of a cognitive theory elucidates on what points the theory has deficiencies and what measures could be taken in order to clarify these confusing points.

A third and, in my opinion, very important advantage of the formulation and application of the criteria is that it gives an opportunity to establish the necessary requirements for a computational theory of the mind. If we run down the criteria this implies the following. A cognitive theory should use a functional level of description as clearly as possible (c1), whereas the elements should be defined in the representing, that is to say the mental, domain (c2). Concerning the syntactic and semantic aspects of the symbol sets, they should

at least constitute a notational scheme (c5). This is, of course, the reason why Kosslyn introduces propositional representations at the bottom level of long term memory. Furthermore, representation as a three-place predicate should be avoided on the penalty of circularity of explanation (c3). In the case of representation as a two-place predicate, the position that similarity between represented and representing domains is necessary, should be abandoned. Similarity of domains is a consequence of the choice for a representation and not the reason for representation (c2), which means that denotation is not the primary route of reference in case of symbol systems (c4).

Finally, I recommend a distinction between (a) representation in the sense of formal scheme, (b) representation in the sense of depiction, and (c) representation in the sense of process. This differentiation could be a first step in further investigating a precise notational scheme for the symbols in the computational mind.

References

Dennett, D. C. (1978). *Brainstorms*. Brighton, Sussex: The Harvester Press.

Dennett, D.C. (1987). *The Intentional Stance*. Cambridge: The MIT Press.

Eysenck, M. W. (1984). *A Handbook of Cognitive Psychology*. Hillsdale: Erlbaum Associates.

Fodor, J. A. (1983). *The Modularity of Mind*. Cambridge: The MIT Press.

Goodman, N. (1981). *Languages of Art*. (2nd ed.) Brighton, Sussex: The Harvester Press.

Jackendoff, R. (1987). *Consciousness and the Computational Mind*. Cambridge: The MIT Press.

Jorna, R.J. (1988). A comparison of presentation and representation: linguistic and pictorial. In G. Mulder & G. Van der Veer (Eds.), *Human-Computer Interaction — Psychonomic Aspects* (pp. 172-185). Berlin: Springer-Verlag.

Jorna, R. J. (1989). *Kennisrepresentaties en symbolen in de geest*. Haarlem: Thesis. English edition (1990). *Knowledge Representation and Symbols in the Mind*. Tübingen: Stauffenburg Verlag.

Kintsch, W. (1974). *The Representation of Meaning in Memory*. Hillsdale: Erlbaum Associates.

Kosslyn, S. M. (1980). *Image and Mind*. Cambridge: Harvard University Press.

Kosslyn, S. M. (1984). *Ghosts in the Mind's Machine*. New York: Norton.

Marr, D. (1982). *Vision*. New York: Freeman & Company.

Newell, A., & Simon, H. A. (1976). Computer science as empirical inquiry: symbols and search, *Communications of the ACM, 19*, 113-126.

Norman, D A., & Rumelhart, D. E. (1983). *Representation in Memory*. La Jolla: Cognitive Science Laboratory.

Palmer, S. E. (1978). Fundamental aspects of cognitive representations. In E. Rosch & B. B. Lloyd (Eds.), *Cognition and Categorization* (pp. 259-303). Hillsdale: Erlbaum Associates.

Pylyshyn, Z. W. (1984). *Computation and Cognition*. Cambridge: The MIT Press.

Shepard, R. N., & Metzler, J. (1971). Mental rotation of three-dimensional objects, *Science, 171*, 701-703.

Tulving, E. (1983). *Elements of Episodic Memory*. Oxford: Oxford University Press.

BOTTOM-UP APPROACHES TO COGNITION: A DEFENCE OF COGNITIVE NEUROSCIENCE

Claude M. J. Braun

SUMMARY: In his book on vision published in 1982, David Marr claimed that bottom-up or neurophysiological approaches to cognition had failed to live up to expectation, and were unable, in effect and in principle, to explain cognition. He advocated a top-down approach where formalization of cognitive operations was to serve as a starting point, followed by implementation of cognitive models, followed only in the last instance by neurophysiological investigation. Other commentators have followed suit. This presentation is an attempt to refute this position by showing that the relationship between neuroscience and cognitive science is in effect, and in principle, a two way street. The validity of bottom-up illumination of cognitive theorizing is demonstrated with numerous examples.

In a 1988 article, Fodor and Pylyshyn (1988) claimed, as had done Marr (1982) previously, that bottom-up or neurophysiological approaches to cognition were unable, in effect and in principle, to explain cognition. They advocated a top-down approach where formalization of cognitive operations was to serve as a starting point, followed by implementation of cognitive models, followed only in the last instance, if at all, by neurophysiological investigation.

Their defense of what they term 'classical' cognitive theory essentially states that representations are indissociably co-existent with syntactic and semantic combinatorial rules (nativistic structuralism). Connectionnism (associationistic atomism) is disparaged for viewing representation as a 'mere' cause-effect matrix generated entirely by external stimulus configurations.

The purpose of the present paper is to take issue with Fodor and Pylyshyn on their indictment of cognitive neuroscience, or more specifically of bottom-up approaches to cognition. A definition of the class of bottom-up approaches in behavioral neuro-science is, therefore, required specifying the necessary and sufficient conditions for a behavioral neuroscience theorization to be bottom-up: (1) It must aim to *describe directly*, one or several microscopic (i.e., invisible to the naked eye) brain units, real or virtual, (2) it must aim to explain, *directly or indirectly*, organized behavior (learning, perception, memory, action, etc.), real or virtual.

The main issues of micro-structural brain-behavior science can be represented by means of a mnemonic device. I call this device the five-E mnemonic, referring to (1) the *engram*, (2) *energy*, (3) *evolution*, (4) *ergonomics*, and (5) *eonics*. Indeed, the current agenda for micro-structural science consists of discovering the nature of the 'memory trace', the 'metabolic economics' (or

energetics) of mental processing, the 'phylogenetic determinants' and species-specific properties and developmental characteristics of brain units, and the effective description and explanation of the 'workings', particularly the performance, of the various levels of micro-elements of the brain, in 'real-time'.

Antiquated notions of hereditary determination lead to antiquated notions of cognition which is then simplistically considered to be either innate or not. In fact, the domain of interaction between heredity and environment, termed epigenesis, has become a major bottom-up corpus with obvious relevance for cognitive psychology. A concise set of epigenetic principles of embryogenesis of the nervous system as elaborated by Changeux in his 1987 book, *Neuronal Man*, can be summarized as follows:

1) *The principle of profusion-to-selectivity*: neural embryogenesis can but be characterized as comprising massive redundancy in its early phases. Indeed, not only is redundancy observed at every developing synapse (axonal terminals compete for synaptic adhesion; only a minority survive; defeated terminals withdraw and degenerate), but even entire neural populations are known to degenerate.

2) *The principle of volatility-to-stability*: this principle applies particularly to molecular aspects of synaptogenesis. Indeed, neuroreceptive molecules coalesce at the dendritic receptor *prior to* arrival of axonal presynaptic candidate growth cones. These molecules are initially located in highly variable (moving) and distributed position over wide contours of the dendrite. Following the arrival of candidate axonal terminals, these molecules aggregate, concentrate, and stabilize at the synaptic junction and are reduced in overall number.

3) *The principle of ephemerity-to-durability*: these same neuroreceptive molecules manifest an embryonic half-life of 18 to 20 hours, and an adult half-life of 11 days.

4) *The principle of variability-to-fixity*: this is a particular aspect of the dynamically *selective* nature of the progressive implementation of the neural micro-structure. The more the species is complex (evolved), the more micro-structural patterning escapes the direct control of structure genes. A direct consequence of this is that primate brains are the most variable in terms of cell types, numbers and connectivity within the species, including among isogenic exemplars (identical twins). As embryonic neural connectivity stabilizes, so do the synaptic function rules, including pre-synaptic system properties, and post-synaptic retrograde (metabolic) influence.

5) *The principle of redundancy-to-parsimony*: though there is still some debate on this question, Changeux postulates that the *information value* of a synapse in a behavioral repertoire further contributes not only to the lowering of signal-to-noise ratio of the discharge rate at any given synapse, but to prevent degeneracy and cell death.

In 1987, Edelman disagreed with this principle. He postulated that massive redundancy is required to support learning, that is, cumulative commitment of synapses to increasingly demanding sampling of the environment. The two positions are not really incompatible however, as the next principle will show.

6) *The principle of plasticity-to-rigidity*: embryonic activity itself, auditory and tactile sensation, trunk muscle contraction, sleep and waking, thumb sucking, and so forth, produces *learning*, which in turn, conditions synaptic selectivity and response committment.

7) *The principle of activity-to-facilitation*: neural networks, as previously stated, are conditioned by feedback mechanisms which are quite numerous in embryonic life. Embryonic activity in itself is, however, under heavy genetic influence. The embryo is, therefore, programed to create a learning environment for itself.

8) *The principle of diffusion-to-selective emplacement*: neuronal growth cones make mistakes and occasionally end up in networks to which they can but remain alien. Functionally homogenous cell populations interact to regulate single cell metabolism responses to cell adhesion molecules which in turn influence cell adhesion molecule genes. There are more places that are forbidden to a growing neuron than there are viable targets.

9) *The principle of critical periodicity or of the epigenetic cascade*: total DNA mass per cell is only 600 times greater in man than in drosophilia, while the number of neurons in man is a factor of 10^5 greater. The 10^{14} or 10^{15} synapses of a normal adult brain are the result of a fertilized ovum containing a mere 2^5 genes. Obviously, some constituants of the human body, (proteins for example), are recuperated and re-used, from plant life to animal life, from species to species, and from organ to organ within an organism, in different 'sauces', so to speak. Structure genes can, therefore, be recruited for different purposes at different time points throughout embryogenesis. But this is not the only way by which

epigenesis economizes on primary materials. It makes
extraordinary use, everywhere we look, particularly in
neurogenesis, of topographical continuity of systems —
particularly fiber projections. Temporal and parietal visual
maps are continuous with geniculate maps which are
continuous with retinal maps. These growth patterns
economize, in the embryo, on genetic programing, relying on
each other's selective continuity by small numbers of binding
molecules, programed by incredibly small numbers of
distinct structure genes. This economy of genetic influence
would be impossible without dynamic embryonic cascades
controlled by small numbers of regulator genes.

I do not wish to demonstrate the implication for cognitive science of all
of the above, but I do suggest that these general principles apply also to the
development of early cognition. We will consider only one example. Infantile
pre-verbal babbling illustrates the passage from *plasticity-to-rigidity*. Each
human infant spontaneously emits the phonemes of all natural languages. He
or she soon becomes incapable of reproducing phonemes other than those of
his or her own cultural environment however. The infant's phonological
repertoire becomes 'rigid'. It is interesting to note that it is at this
microfeatural level that such a phenomenon is observed, while simultaneous-
ly, the infant rapidly learns to master phonological, semantic, and grammati-
cal rules of his 'maternal' language thereby demonstrating remarkable
plasticity — at another level.

Recent animal and human neonate research suggest that (1) Early forms
of categorization performance are relatively species-specific. (2) Ecologically
relevant stimuli are categorized in a manner, according to a logic, which is
different from non-ecologically relevant stimuli. Primitive categorization is,
therefore, not strictly 'veridical' but answers to 'polymorphous sets'. (3)
Categorization initially rests upon innate mechanata which draw upon
primary repertoires of neural networks. Edelman interprets these as
evolutionarily determined structures serving as abstractors in sensory sheets
and acting in concert and simultaneously with motor ensembles in an adapted
phenotype. (5) Prototypical features of an object exemplar are selected
extremely parsimoniously, that is, are heavily sampled rather than exhaustive-
ly inventoried, by the organism. Other features are ignored. (6) Re-entry
among levels and between maps — the first to accommodate dynamic shifts
and readjust the mapping, and the second to create derivative maps resulting
in classifications couples, must be invoked to explain resistance to back-
ground noise, and micro-feature changes. (7) Learned categorization, based
on formal instruction, obeys a particular logic, probably more dependent
upon secondary repertoires of neuronal networks. Massive two way re-entry
occurs between primitive feature detector networks and feature correlator
networks — thereby explaining how learned behavioral repertoires rest upon,
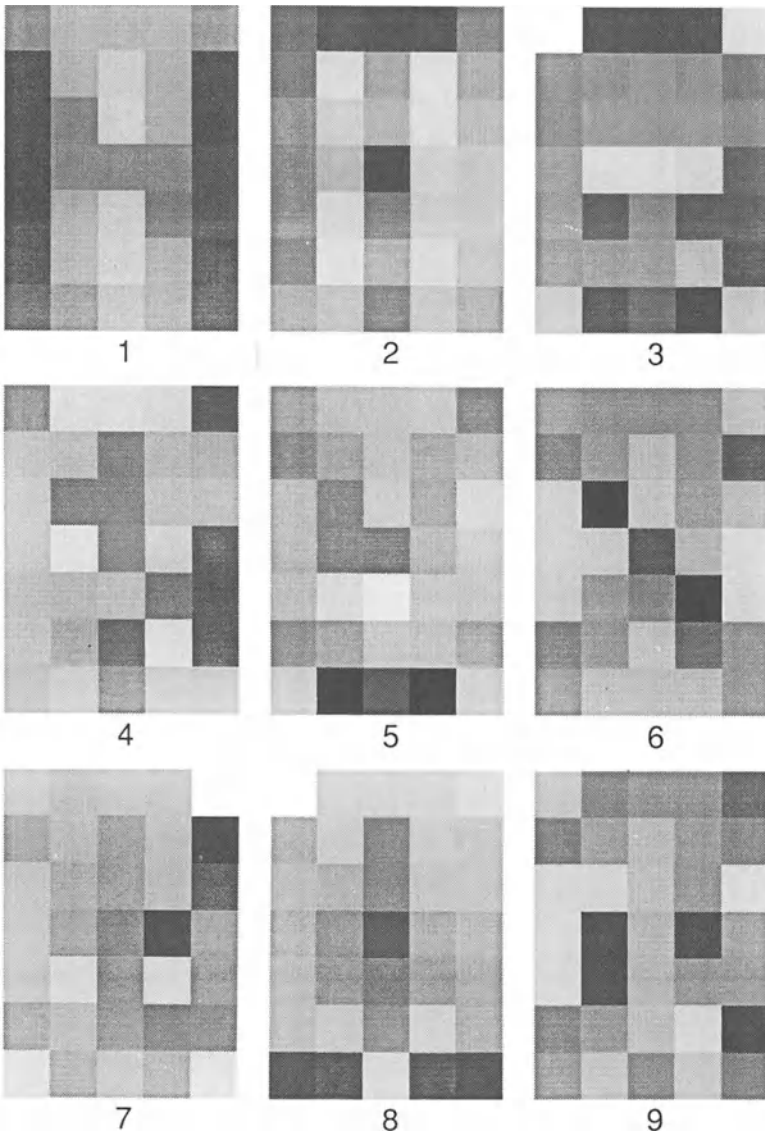
Figure 1. Representation in the stimulus space of the first 9 eigenvectors of the connectivity matrix after 1,000 learning trials of the EIDOS neural model for the letters A, E, H, I, N, O, R, S, and T. The configuration illustrates the purely associationistic emergence of clearly identifiable distinctive features forming the basis for letter-prototypes. For further details see Figures 2 and 3 and accompanying text. Data and illustration courtesy of Robert Proulx and Jean Bégin, Psychology, UQAM.

are continuous with, but also smother, to some extent, primary behavioral repertoires in early infancy.

Carrying these interpretations to their logical conclusion would consist of applying them to memory. Information theory approaches based on limited channel apparata for exhaustive sequential processing of 'bits' of data, are rejected in favor of a new concept of memory. According to the 'bottom-up' approach whose components are not filters and bins, but innumerable synaptic membranes, large sets of local object features are not stored at all. Instead, dysjunctive exemplary features are parsimoniously sampled, in each case. Not only are the functional units interpreted as populations, but so are the features of the outside world to be perceived. To quote Edelman, 1987, "Memory is re-categorization." Memory functions both on a probabilistic mode and on a prototype (or exemplar) mode. Primitive memory, the one used most of the time, even by civilized humans, has a strong 'procedural', rather than 'declarative' (or propositional) flavor in this approach. Primitive memory is associative, yet not atomistic (mentalistic) in the 'Lockean' sense.

Finkel and Edelman, in 1987, helped carry the field of membrane physiology into the domain of cognitive science. They have proposed a theory of what they call "synaptic modification rules" applicable to the human brain. Some of these rules are the following: (1) Pre- and postsynaptic changes, involving more than one cell, occur at the same synapse. (2) No aspect of either pre- or postsynaptic modification is required for the other to operate at the molecular level. (3) Co-activated heterosynaptic inputs to a neuron alter the states of ion channels at a given synapse. The ensuing change in population-distribution of local channel-states affects the postsynaptic potential produced at that synapse by subsequent inputs. In general these effects are short term and local. (4) If the long-term average (over 1 second) of the presynaptic efficacy exceeds a threshold (neurotransmitter depletion), baseline presynaptic efficacy determining neurotransmitter release is reset by the cell to a new value. This presynaptic rule applies to large numbers of synapses, is widely distributed and explains long-term memory.

This model, or theory, summarized in Edelman, represents the ascension to a population theory of the engram. It explains neuronal information processing in terms of the complete activity and anatomy of the ensemble of networks of units comprising the brain. It is strongly based on recent experimentation. More specifically, it explains why individual neurons do not, in fact, behave according to Hebb's rule, by which connection strength is stated to be a function of discharge frequency *of a single synapse.* Membrane mechanata are described here, for the first time, as parallel, distributed, hierarchical, and plastic. In short, population properties of networks are described in real time and space based on appropriate experimental and descriptive empirical investigation, the detail of which cannot be elaborated upon here.

Merzenich, et al. (1984) recently mapped the representation of the hand in monkeys by carefully and exhaustively stimulating small surfaces of skin and recording the somatosensory cortex with microelectrodes before and after amputation of single digits, transection of peripheral nerves, functional alteration without transection by appropriate bandaging, casts, finger tapping protocols, and cortical ablations.

The key observation is that cortical maps rapidly reorganize. As the maps are restructured, basic topography (or somatotopy) is maintained. Initial post-traumatic silent cortical areas gradually shrink and may occasionally replace areas that were active immediately after transection. Silent areas persist only after ablation of several adjacent digits. The reorganized map following nerve regeneration is initially fragmented, disorganized, and manifests redundancy (multiple representation) but congeals and reoccupies an area quite similar, but not identical, to that before transection. Re-mapping occurred too fast to be explained by re-sprouting, nor was resprouting observed histologically (see Edelman for a review).

The authors conclude that (1) from the primary repertoire, a dynamic process must select particular neuronal groups in a secondary repertoire to form the functional map, (2) there must be presence of a large proportion of uncommitted functionally degenerate neurons interdigitated (segregated) in the midst of the primary repertoire map, and (3) neurons of secondary and primary repertoires may compete to form maps.

The new trend in artificial intelligence is the bottom-up approach, as opposed, precisely, to the top down approach. Scientists have begun attempting to explain cognition using parallel and distributed processing (PDP) networks of virtual neurons.

All of these models are set up in the following manner: (1) a given task is selected for simulation but is only minimally defined, (2) neuroscience-based feature committments are programed for input reception, (3) a few neuroscience-based rules of association of 'unit' activity are pre-specified (typically involving reinforcement or feedback of some kind), and (4) a sufficient number of iterations of the complete set of matrice transformation routines are allowed for desired output (appropriate learning rates, error profiles, etc.).

Strong points of the neural modeling PDP approach include the following: (1) the implied (simulated) hardware resembles much more closely the architecture of real brains (real brains do not contain RAMs, ROMs, buses, nor do they contain hardwired culturally specific symbol processors, operate in strict serial fashion, or possess micro-second speed), (2) the models manifest the ability to draw signals from noisy backgrounds, (3) the models manifest graceful (i.e., biologically plausible) degradation upon removal (i.e., destruction) of sub-components, (4) the models are capable of answering to real-time performance criteria (i.e., they can manifest cognitively plausible
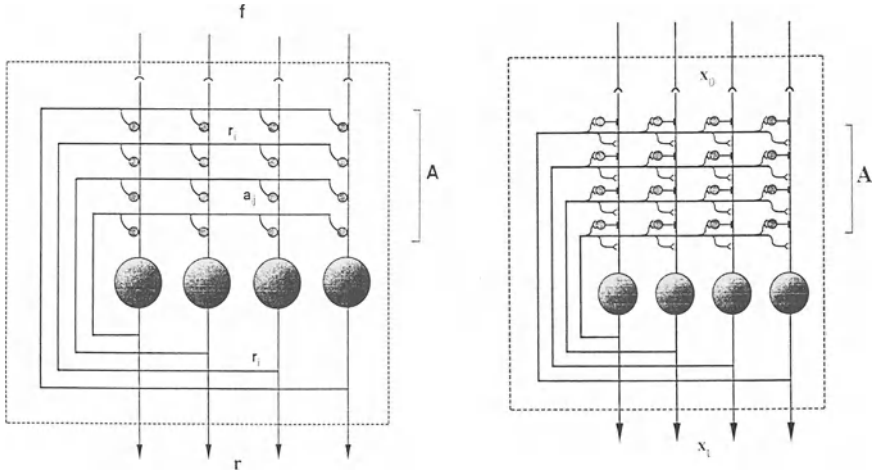
Figure 2. Left: Standard BSB neural model as proposed by Anderson, et al. (1977). An input stimulus (f) is fed to the system to produce an initial response (r) which is constantly recycled in a positive feedback loop (matrix A) until r becomes invariant. Right: The EIDOS neural model is the same as the BSB model except for presence of a negative in addition to a positive feedback loop, and incremental delay of the negative feedback. Illustration courtesy of Robert Proulx and Jean Bégin, Psychology, UQAM.

learning curves), and (5) the models develop rules stochastically and are, therefore, capable of simulating both low level (associational) and high level (propositional) information processing, both of which are known to occur in human performance.

Bottom-up approaches in behavioral neuroscience, are still quite weak with regards to the *ergonomic* dimension of brain function. Unit activity is typically interpreted in a framework reminiscent of Hobbesian or Lockean abstract atoms. The brain, however, is an agglomerate of energy-expending networks composed of neurons consuming varying amounts of glucose, that is, *working*, in complementary manner. To this extent, the brain should not be conceived of as an entirely *parallel* machine.

Neuroscientists have been accustomed to investigate very small neuronal circuits physiologically — preventing them from approaching ergonomic explanations of cognitive systems. Physiological brain-imaging techniques, BEAM, PET, SPECT, RCbF, MEG[1], describe local metabolic (i.e., ergonomic) investment over large brain circuits. They are generally used, however, only in synchronic (anatomic) rather than diachronic (physiological) frameworks, not only for clinical diagnosis but in fundamental behavioral neuroscience research as well. One very elegant bottom-up potential solution
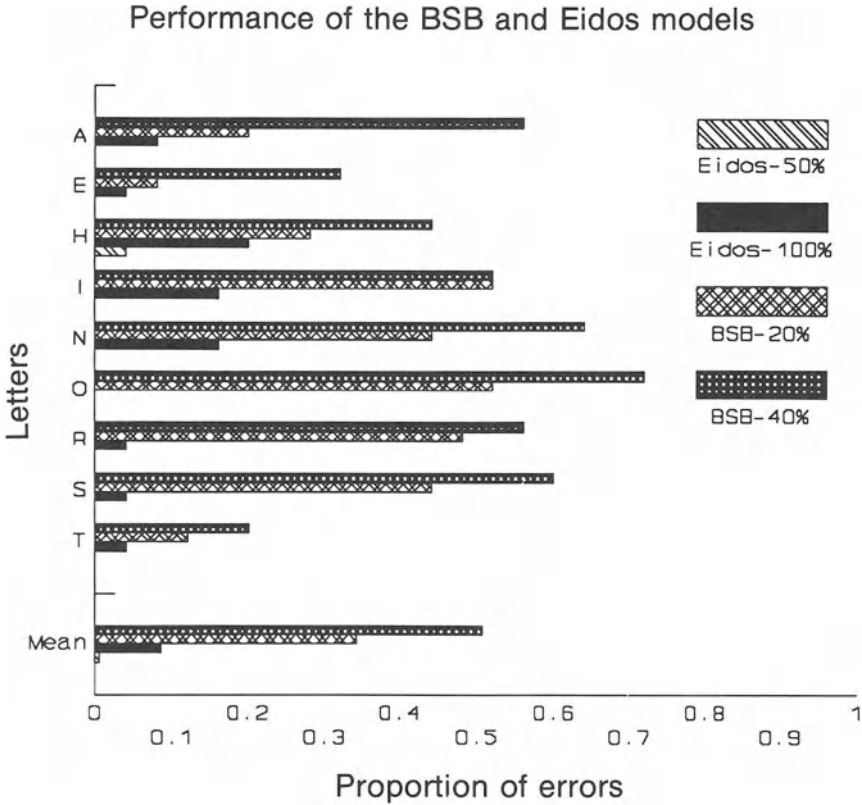
Figure 3. Proportions of letter categorization error by the BSB and
EIDOS models under differing noise levels (random distortion)
added to initial input. The better performance of the EIDOS model
may be related to neurophysiologically more relevant dynamically
intercalated interaction of bivalent feedback (neural excitation and
inhibition). Data and illustration courtesy of Robert Proulx and
Jean Bégin, Psychology, UQAM.

to this problem has been developed by Prigogine (1980) and his collaborators
in Brussels. The solution requires some introduction.

In the physical and biological sciences, complex structural changes can
occur when the state of a system is far from equilibrium. Even seemingly
'simple' systems may change their structure in very dramatic ways. These
range from simple periodic 'behavior', to complex aperiodic changes referred
to as 'chaos'. Numerous scientists have long understood that diverse forms of
chaos exist, and have developed means of quantifying specific irreversible
systems far from equilibrium such as liquid and gas turbulences, coastline
configurations, cloud structures, and so forth (one example is fractal theory).

Prigogine's team has devised a mathematical model so rich, however, that it can process virtually any set of variables over time no matter how complex. The model further provides intuitively (qualitatively) understandable quanta, allowing hypothesis testing and theory building. The rest of the explanation of the model will focus on these intuitive aspects and will purposely avoid any mathematical aspects.

Phase space is a multidimensional space whose dimensions constitute all of the variables needed to completely describe a system as a whole. A two variable system would be a phase-plane. Each possible 'instantaneous' state of the system is represented by a single point, and each point in the phase space corresponds to one and only one state of the system. Changes in time can be conceived of as the 'trajectory' of the phase space. Though the phase space 'portrait' of a system consists of all 'possible' combinations of variables, the 'actual' trajectory of a system is represented by a much more limited set of points. If in a phase space trajectory, the probability of finding a point is equal in all areas of the phase space, then changes are 'random' in the system. The extent to which the trajectory departs from randomness is the degree of 'determinism' within the system. In such an instance there is a tendency over time toward occupying a certain portion of the phase space. This portion of the phase space is referred to as an 'attractor'.

There are several types of attractors. When a system tends toward a state of 'equilibrium', whatever the initial conditions, the trajectory will eventually evolve toward one unique 'fixed' point. Approaches to equilibrium may be indirect (heterotonic), spiral shaped, for example. If, however, rather than tending toward a single point, phase space trajectories tend toward a 'closed line', the system develops toward a 'periodic regime'. Biological rhythms and chemical oscillations can be modeled as such periodic attractors. Quasi-periodic systems (with two or more incommensurate frequencies) appear as surfaces in the form of tori (deformed planes). Chaotic attractors may constitute a sub-area of phase space toward which a trajectory develops over time (thereby illustrating that a deterministic process is occurring), but which combines two 'antagonistic trends', an 'instability of motion' and 'stability of the whole'. The "correlation dimension" of an attractor (Grassberger & Procaccia, 1983 a, b), is the smallest whole number which exceeds the fractal dimensionality of the chaotic attractor constituting the minimum number of variables needed to fully describe the attractor.

One of the amazing characteristics of several chaotic attractors found to represent experimentally observed situations is their relatively low dimensionality despite their outward appearance of complexity and confusion.

The relevance of the Brussels school's work for bottom-up brain-behavior theory building is obvious. Mentation is a global property of the

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1 Brain electrical activity mapping, Position emission tomography, Single photon emission tomography, Regional cerebral blood flow, magnetoencephalography.
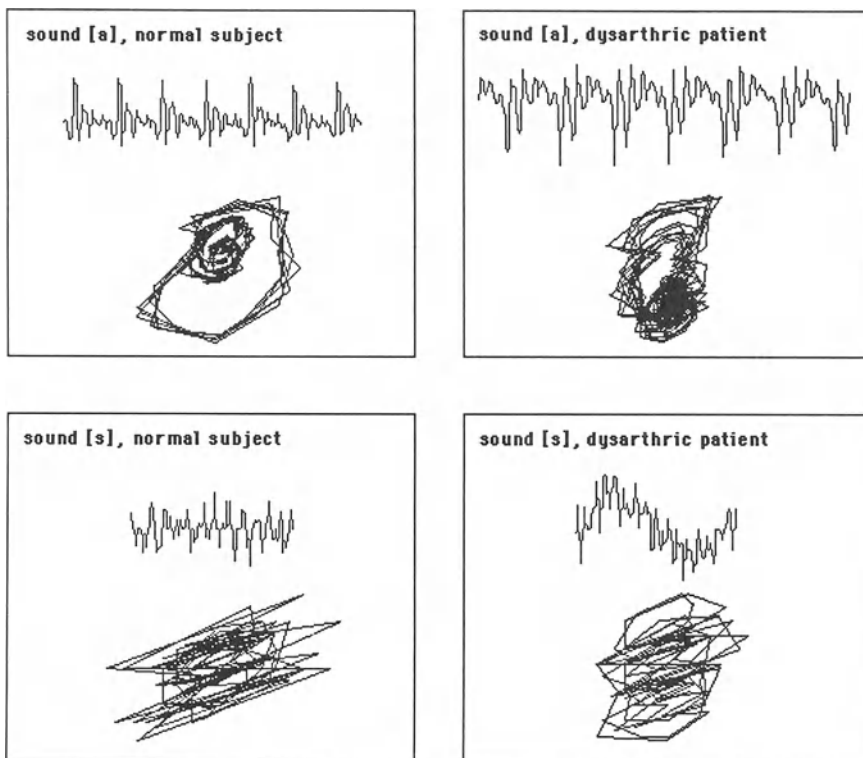
Figure 4. Digitization (upper) and phase portraits (lower) of the vowel [a] and the fricative [s] of a normal adult female and an adult female with dysarthria following hereditary cerebellar degeneration. The phase trajectory function is y = amplitude and x = delta of amplitudes between successive samples (sampling rate: 10 kHz). Normal and dysarthric vowels are easily distinguished in the phase portraits by the difference between 'tight' and 'looser' loops, while signal representations are less clearly distinctive. On the other hand, normal and dysarthric fricatives are well distinguished in both representations, where a slow dysarthric modulation in the signal translates into a less angular and more vertical structure in the phase representation. Data and illustration courtesy of Eric Keller, Linguistics, UQAM.

activity of close to a trillion synapses, involving sets of networks which develop and change over time, as a function of genetic, epigenetic and internal and external environmental variables: an apparently chaotic matrix if there ever was one.

Mainframe computers are sufficiently powerful to store repeated sequences of BEAM, RCbF, PET, SPECT, and multichannel MEG recordings.

Though there are logistic problems related to purchase of such prolonged computer time, there is no technical impediment, as such, to going beyond the usual static two-dimensional image derived from 3-D brain scanners. The only *apparent* serious impediment to such an enterprise is the mind boggling conceptual difficulty involved in making any sense out of such a 'chaotic' mass of data.

Conclusion

The brain is *the* information processing device of the organism. This paper has attempted to demonstrate that the brain *can* and *must*, in and of itself, inspire us to understand it's own mode of functioning at the cognitive level.

References

Anderson, J. A., Silverstein, J. W., Ritz, S. A., & Jones, R. S. (1977). Distinctive features, categorical perception, and probability learning: Some applications of a neural model. *Psychological Review, 84*, 413-451.

Changeux, J. P. (1987). *Neuronal man: the biology of mind*. New York: Oxford University Press.

Edelman, G. M. (1987). *Neural darwinism: the theory of neuronal group selection*. New York: Basic Books Inc.

Finkel, L. N., & Edelman, G. M. (1987). Population rules for synapses in networks. In G. M. Edelman, W. E. Gall, & M. Cowan (Eds.), *Synaptic function* (pp. 711-757). New York: Wiley.

Fodor, J. A., & Pylyshyn, Z. W. (1988). Connectionnism and cognitive architecture: a critical analysis. *Cognition, 28*, 3-71.

Grassberger, P., & Procaccia, I. (1983a). Measuring the strangeness of strange attractors, *Physica, 9*, 189-208.

Grassberger, P., & Procaccia, I. (1983b). Characterization of strange attractors. *Physical Review Letters, 50*, 346-349.

Marr, D. (1982). *Vision: A computational investigation into the human representation and processing of visual information*. San Frnacisco: Freeman.

Merzenich, M. M., Nelson, R. J., Stryker, M. P., Cynader, M., Schoppman, A., & Zook, J. M. (1984). Somatosensory cortical map changes following digit amputation in adult monkeys. *Journal of Comparative Neurology, 224*, 591-605.

Prigogine, I. (1980). *From being to becoming: time and complexity in the physical sciences*. New York: W. H. Freeman.

THE SOCIAL CONTEXT OF RESEARCH PRACTICE AND THE HISTORY OF PSYCHOLOGY

Kurt Danziger

SUMMARY: The empirical domains about which psychologists theorize are not raw natural phenomena but carefully constructed products of psychological practice. The rules governing the construction of such products are enforced by communities of practitioners. Such communities are themselves part of the history of the societies in which they flourish. They adapt to the demands imposed on them by their social context by modifying the rules governing their professional activity, including the production of empirical domains. These rules are subject to historical change, and the knowledge products that are constructed with their help are historical products. At the same time, rules for the production of acceptable empirical domains are based on theoretical presuppositions about the nature of psychological reality. Changes in these rules are also theoretical changes. On this level there is a profound historicity of theory, but the theorizing at issue here is that which goes on implicitly before and during the construction of empirical domains rather than explicitly afterwards. A major historical change in rules of practice and their implicit theories occurred when psychology switched to a preference for certain types of statistical data. This preference can be traced to practititoners' need to legitimize their activity in terms of a particular interpretation of what constitutes science and a limited interpretation of what constitutes socially useful knowledge.

How one conceives of the relationship between theory and history will obviously depend on what one takes 'theory' to be. The term 'theory' is open to a number of different interpretations, each of which assigns theories a different place in the scheme of things and, therefore, poses their relationship to history in a different way. For the sake of brevity I will limit myself to a rough distinction between three diverging conceptions of the theoretical which seem particularly relevant for the field of psychology. I will refer to these as the idealist, the positivist, and the post-positivist view respectively.

The idealist view ultimately takes its inspiration from Plato and conceives of the realm of the theoretical as that of pure, that is, disembodied, ideas which are universal and, therefore. trans-historical. In this framework historical investigation has the task of stripping aside the merely contingent and superficial aspects of theoretical forms to reveal the timeless ideal core which they express. History allows us to discount surface differences in terminology and context so that we can glimpse the eternal return of timeless psychological problems and the fixed possibilities for solving them theoretically. Paradoxically, this kind of history is directed at showing the irrelevance of history at the most fundamental level of psychological theorizing. History

is simply an instrument for penetrating to an ultimately ahistorical ideal reality (cf. Watson, 1971).

The positivist view, which is the taken for granted view in the classical historiography of psychology (cf. Boring, 1950), is based on a sharp separation between the realm of the empirical and the realm of the theoretical, with the former providing the basis for the latter. History is partly an account of the cumulative growth of empirical data and partly an account of the successive replacement of theories by better theories that are more convincingly supported by the data. The main task of historical analysis seems to be the demonstration, or perhaps one should say, the celebration, of the cumulative progress of scientific psychology. For the practitioner at the famous cutting edge of empirical research history has at best an ornamental function, for, by definition, earlier formulations are merely stepping stones to the more adequate theoretical notions achieved in the present.

Whereas both the idealist and the positivist view are associated with a certain 'discounting' of history in relation to theory, the more recent post-positivist view seems to make room for an emphasis on the essential his-toricity of theory. Abandoning both the disembodiment of pure ideas and the strict separation of the theoretical from the empirical, post-positivism replaces the substantive concept of 'theory' with the notion of 'theorizing' as a human acitivity. In other words, the realm of theory is seen as embedded in human practices, whether they be discursive practices, the practices of everyday life, or laboratory practices. It is the essential historicity of human practice that entails the historicity of theorizing. In what follows I want to present a brief outline of one variant of this approach on which I have been working in recent years.

Quite pervasively, traditional psychological theories take for granted the 'natural' existence of the domains to which they apply themselves. They take for granted the natural existence of a distinct domain of events labelled 'motivational' or 'emotional' for example, so that one can then have a theory of motivation, emotion, and so forth. They take for granted the naturally given nature of empirical data, and that means that they are presented as theories about the data and not theories about the scientific activity without which these data would not exist.

In fact, of course, all these domains of psychological categories, of special kinds of people (experimental subjects), of empirical data, are not simply presented to us by nature on a platter but are the products of human interven-tion. It seems appropriate to refer to these domains as psychological objects. Psychological objects are simply the things that psychologists take to be their proper objects of investigation or professional practice. But psychologists can never investigate any 'natural' human category directly — they must constitute an object of investigation in the course of that investigation (Gergen, 1982).

Our experiences and actions do not bear little tags, supplied by nature, that identify them as instances of motivation, a personality trait, or a bit of information. They have to be construed as such. Experiences do not naturally arrange themselves in the form of statistical series; they have to be arranged accordingly. People have to agree to act as experimental subjects and modify their conduct in terms of the structure of that role. To understand psychological objects we need some understanding of the way in which they are constituted. But that is something that has changed historically. Psychological categories, rules for producing acceptable data, and rules for arranging research situations have all been subject to quite drastic changes, and we have little hope of understanding the constitution of pscyhological objects without some understanding of these changes. Psychologists are always recreating the objects of their investigation in the course of investigating them, but they are not free to do so at random. They are constrained by historically constituted structures, both cognitive and practical.

Psychological objects are embedded in the life and work of communities of specialists in psychology. The emergence of such communities of specialists, who claim a monopoly on the production of psychological knowledge, is of course the crucial feature that distinguishes the short history of modern psychology from its long past. A key element in this development is a sharp separation between the discourse of the specialists and lay discourse. Specialist discourse constitutes its own world of psychological objects within which it operates. If we look at the way in which these objects are constituted we find that practical action plays a key role. Modern communities of psychological specialists do not base their claim to superior knowledge on the fact that they have *thought* harder about certain matters than lay people, but on the fact that they have *in practice* produced and changed a variety of interesting psychological objects. In other words, in 20th century psychology the constitution of psychological objects has relied heavily on the practical production of so-called empirical domains.

Typically, the construction of empirical domains takes place in two phases. In the first phase, a number of participants work together in defined investigative situations to produce 'raw data'. The work of the participants proceeds according to strict rules that govern their inter-relationship. In the second phase the investigators manipulate the record that constitutes the raw data so as to produce a form of product that is publishable according to the conventions of the day. This process also is governed by strict rules that have nevertheless seen considerable historical modification. Needless to say, investigators' knowledge of these rules in large measure determines what aspects of the investigative interaction are considered worth recording and, therefore, worth eliciting. For instance, an investigator who knows that lengthy introspective reports are not publishable is not likely to ask for them or to take them seriously as recorded data if they are spontaneously offered.

But we have to distinguish between the motives and actions of individuals and the social patterns prevailing in the discipline to which the individual investigator has to react. From the point of view of the individual actor and its social psychological analysis the prevailing social patterns, whether they regulate the structure of the investigative situation or the nature of publishable data, can be taken for granted. But from the point of view of the discipline and its historical development, it is precisely these social patterns that are the major object of interest. This requires a different level of analysis, one which is necessarily historical.

From the point of view of the individual investigator the choice of procedures may indeed often be reduced to essentially technical, and that is to say, rational, considerations. But this is only possible because the historical development of the discipline has predetermined the nature and the variety of alternatives that are available to the individual investigator at a particular time. In the construction of empirical objects one has, therefore, to distinguish between specific instances of such objects, produced at a particular time and place, and the general features of such objects which characterize them over relatively extended historical periods and in numerous locations.

These general features of empirical objects imply theoretical presuppositions which are usually taken for granted by those who constitute such objects in the course of their daily scientific work. For instance, the practice of what used to be called "systematic experimental introspection" (Ach, 1905; Danziger 1980) presupposed quite an elaborate set of theoretical assumptions about the existence of private worlds of individual experience whose general features were nevertheless universal and unambiguously communicable. These theoretical assumptions were prior to any more specific theories regarding the details of the general features of private consciousness, and they set the framework within which the more specific theories had to operate.

But the most interesting aspect of this level of implicit theorizing was its relationship to research practice. In acting on their fundamental theoretical assumptions experimental introspectionists proceeded to construct the kind of empirical world that their research practice presupposed. This was a level of implicit theorizing which was embedded in the rules and conventions of investigative practice. Not only were there never any atheoretical research methods, but the practice of psychological research involved theorizing of the most fundamental kind (Danziger, 1988).

As well as being a deeply theoretical activity the investigative practice of psychologists has always been a profoundly social activity. I have already indicated that the construction of empirical objects requires the socially organized interaction of participants in research situations and is guided by intra-disciplinary norms. But these norms are part of the historically evolving life of the discipline that is determined by the broader societal context out of which the discipline emerged and in which it must survive, and, if possible,

grow. The patterns of disciplinary practice, and, therefore, of fundamental theorizing, are decisively shaped by the kinds of demands that the socio-historical context imposes on the discipline. This has been a particularly important source of the historicity of fundamental theorizing in twentieth century psychology. A brief, and necessarily incomplete, example will serve to illustrate the way in which this historicity of theory has assumed practical forms.

In order to mobilize the resources on which its life as a discipline depended psychology had to show that what it did and what it produced was valuable, by the standards prevailing in its society. "Being valuable" was often translated as "being useful," especially in the pragmatic American context (Danziger, 1979; James, 1892). But the crucial issue was, of course, how usefulness was defined. In the always dominant interpretation usefulness meant useful to agencies of social control, of management, of institutional administration (Napoli, 1980). Certainly, psychology promised great benefits to individuals, but in the dominant model these benefits accrued to individuals as the objects of agencies of social control, schools, clinics, personnel departments, and so forth.

Such agencies, however, were primarily interested in certain kinds of psychological knowledge objects. The contributions of psychologists were acceptable insofar as they permitted defined institutional goals to be achieved more efficiently and insofar as they provided a legitimation for institutional practices that might arouse doubts or opposition. Psychological knowledge objects which depended on the statistical construction of 'individual differences' in performance measures fitted these requirements perfectly, and so we find such objects above all others being put to work in these practical contexts (Danziger, 1987). This undeniable practical success quickly led many American psychologists to take it for granted that the kind of knowledge which would be socially useful was statistically constructed knowledge.

The consequences of successful institutional application for psychology's investigative practice were all the more profound because they converged with the effect of a certain interpretation of science that was widely used to establish the legitimacy of that practice. If the enterprise of modern psychology was to succeed, it was imperative that it be recognized, not only as socially useful, but also as 'scientific'. This consideration was important to those in control of relevant resources, but also to potential recruits to the discipline, and to the practitioners themselves, whose belief in the worth of their work was often closely tied up with their faith in 'science'. The legitimacy of psychological investigation depended on its perceived conformity to certain commonly held beliefs about the nature of science, that is, to a particular ideology of science.

One pervasive version of the criterion of scientificity took the form of a belief that psychology could not qualify as a science unless it devoted itself to

the search for universal, and, therefore, ahistorical, 'laws' of human behavior. However, those who shared this belief faced a problem, because, with the exception of some very restricted areas of research, psychological phenomena lacked the stability and consistency to make them promising candidates for the display of such laws. By far the most popular solution to this dilemma involved the reconstruction of inconsistencies in terms of a particular statistical model. Variations in the conduct of different individuals were first reduced to quantitative form by constructing appropriate investigative situations, and then these variations were treated as 'individual differences' on some supposed underlying dimension. This procedure was based on the crucial, but implicit theoretical assumption of a continuous distribution of differences on a dimension that was appropriate for all individuals (Harré, 1979, p.108; Lamiell, 1987; Valsiner, 1986). Generalization across individuals was now possible, but the common understanding of such statistical generalizations as 'laws' of individual behavior implied a particular theoretical model of the causal structure of the factors underlying such behavior. This model was never challenged by the research practices which presupposed it, and it put its stamp on a whole set of more specific theories that sought to account for the empirical regularities obtained by these means.

The situation was quite analogous to that which had existed in introspective psychology. In both cases empirical objects were constructed by means of investigative practices that implicitly presupposed a certain model of psychological reality. This resulted in an array of empirical objects of a certain type. The significance and relevance of the more specific, explicit, theories developed to account for such arrays depended entirely on the validity of implicity theoretical commitments embodied in the investigative practices used to construct empirical objects. These practices in their turn reflected the historical situation in which the practitioners found themselves. In the case of the later statistical practices that situation is tied up with the fate of an emerging discipline struggling to establish its legitimacy. Analogous contextual factors operated in the case of the earlier introspective psychology, though these cannot be pursued here (see however Danziger, 1990).

We are accustomed to thinking of theories as sets of disembodied propositions. That, however, is an idealization. Theories are not only the products of the human activity of theorists, they are also embedded in that activity. Like all human activity, theorizing takes place in a historical context. The connection between history and theory is, therefore, an intrinsic one. History is not something to be brought to bear on theory from the outside. Rather, historicity has to be seen as an essential feature of theorizing.

References

Ach, N. (1905). *Ueber die Willenstätigkeit und das Denken.* Göttingen: Vandenhoeck & Ruprecht.

Boring, E. G. (1950). *A history of experimental psychology,* (2nd ed.). New York: Appleton-Century-Crofts.

Danziger, K. (1979). The social origins of modern psychology. In A. R. Buss (Ed.), *Psychology in social context* (pp. 27-45). New York: Irvington.

Danziger, K. (1980). The history of introspection reconsidered. *Journal of the History of the Behavioral Sciences, 16,* 241-262.

Danziger, K. (1987). Social context and investigative practice in early twentieth-century psychology. In M.G. Ash & W. R. Woodward (Eds.), *Psychology in twentieth-century thought and society* (pp. 13-33). New York: Cambridge University Press.

Danziger, K. (1988). On theory and method in psychology. In W. J. Baker, L. P. Mos, H. V. Rappard, & H. J. Stam (Eds.). *Recent trends in theoretical psychology* (pp. 87-94). New York: Springer-Verlag.

Danziger, K. (1990). *Constructing the subject: Historical origins of psychological research.* New York: Cambridge University Press.

Gergen, K. J. (1982). *Toward transformation in social knowledge.* New York: Springer-Verlag.

Harré, R. (1979). *Social being.* Oxford: Blackwell.

James, W. (1892). A plea for psychology as a 'natural science'. *Philosophical Review, 1,* 146-153.

Lamiell, J. T. (1987). *The psychology of personality: An epistemological inquiry.* New York: Columbia University Press.

Napoli, D. S. (1980). *Architects of adjustment: The practice and professionalization of American psychology, 1920-1945.* Pt. Washington, NY: Kennikat Press.

Valsiner, J. (1986) (Ed.). *The individual subject and scientific psychology.* New York: Plenum.

Watson, R. I. (1971). Prescriptions as operative in the history of psychology. *Journal of the History of the Behavioral Sciences, 7,* 311-322.

RECONTEXTUALIZATION AS A CONTRIBUTION OF HISTORY TO THEORETICAL PSYCHOLOGY

Pieter J. van Strien

SUMMARY: In most parts of psychology, theory-building aims at general propositions and law-like formulations that, in the sense of the old Greek ideal of Truth, are not subject to the vagaries of time and place. As soon as theories are accepted by the scientific community and receive a place in textbooks, they become *decontextualized* or, a-historical. Only the names of the authors remind us of their origin: for example, the James-Lange theory of emotions, Pavlov's conditioned reflex theory, Festinger's cognitive-dissonance theory, and so forth. My contribution to this symposium consists of a plea for *recontextualization* of theory as an important contribution of the history of psychology to theoretical psychology. This means a rehabilitation of the context of discovery, which in current philosophy of science is relegated to the psychology of creativity and the curiosity-shop of biography. I will try to show that a contextual analysis, in which the *original problem situation* is reconstructed, does not necessarily lead to relativism and resignation, but also can help us to assess the contribution of past theoretical ideas to current theoretical discussion. Progress in science thus is furthered by historical regression.

The Components of Scientific Problem Solving

Science can be conceived of as the continuous effort of human kind to solve, with the help of cumulative theorizing, the intellectual and practical problems with which it is confronted. The accumulation of knowledge requires social structures within which methods and outcomes are discussed, criticized, systematized, and handed down to newcomers. A further elaboration of this characterization allows the following components of scientific problem solving to be distinguished (see Figure 1).

1) The theoretical and practical *problems* that constitute the impetus to a specific line of investigation.

2) The *conceptual tools* that are used in answering the problems, namely, already existing theories, methodological notions, models, analogies and metaphors, and (often implicit) notions about man and society by which scientists and practitioners are guided in their approach to problems.

3) Lines of theorizing and *paradigmatic* ways of problem solving that are developed in the course of time as a result of the investigative practices of scientists. By paradigm I under-

stand a way of solving a theoretical or practical problem that has proved to be successful, and, thus, becomes exemplary for further problem solving. Paradigms involve theoretical elements but also a shared set of notions, assumptions and specific investigative practices in the sense of Danziger (1987). (It will be clear from this description that I do not take the term paradigm in the encompassing sense that Kuhn [1962] used it to describe scientific revolutions but, rather, in the micro-sense of 'exemplars' [Kuhn, 1970], which also adheres more closely to the current use of the term).

4) The *institutional framework* (e.g., university institutes or laboratories, and professional service organizations), within which the resulting theories, paradigms and programs are employed as tools, and handed down to newcomers in the science or the profession.

Before proceeding to the elements of a contextual analysis, I first want to point out that all the above mentioned components can become the focus of historical study. In fact, the variations in intellectual and social history discerned by Scheerer in his contribution to this symposium reflect the different components of my analysis.

Problemgeschichte. In its original form, specific, and, more or less, perennial problems, that have engaged psychological thinking through the ages, are used to construct lines of continuity through history. In Pongratz's (1967) *Problemgeschichte* the way the subject-matter of psychology has been approached from ancient times onwards forms the organizing principle of his historiography of psychology. However, when we start out from the wider conception of problems implied in the preceding analysis of the components of scientific problem solving, practical problems can also become the point of departure. Thus, *social problems*, such as occupational problems, problems of education, of sexuality and sexual relationships, and problems of prejudice and conflict, that have formed an impetus to theorizing and to professional practices in psychology, also belong to 'problem-oriented history'!

History of concepts, and history of ideas. The way specific concepts are used in the course of history (Scheerer) or, in the case of the history of ideas, the way certain 'unit-ideas' have guided thinking on the human mind and society are included under this heading. In a broader sense, the investigation of the succession of metaphors that have been used as vehicles in representing psychological functions such as memory and thinking, also belong to this category.

Doxography; history of theories and systems. Under this heading, I include the traditional way in which the contribution of our predecessors is handed down in history courses in psychology and the other sciences. In its
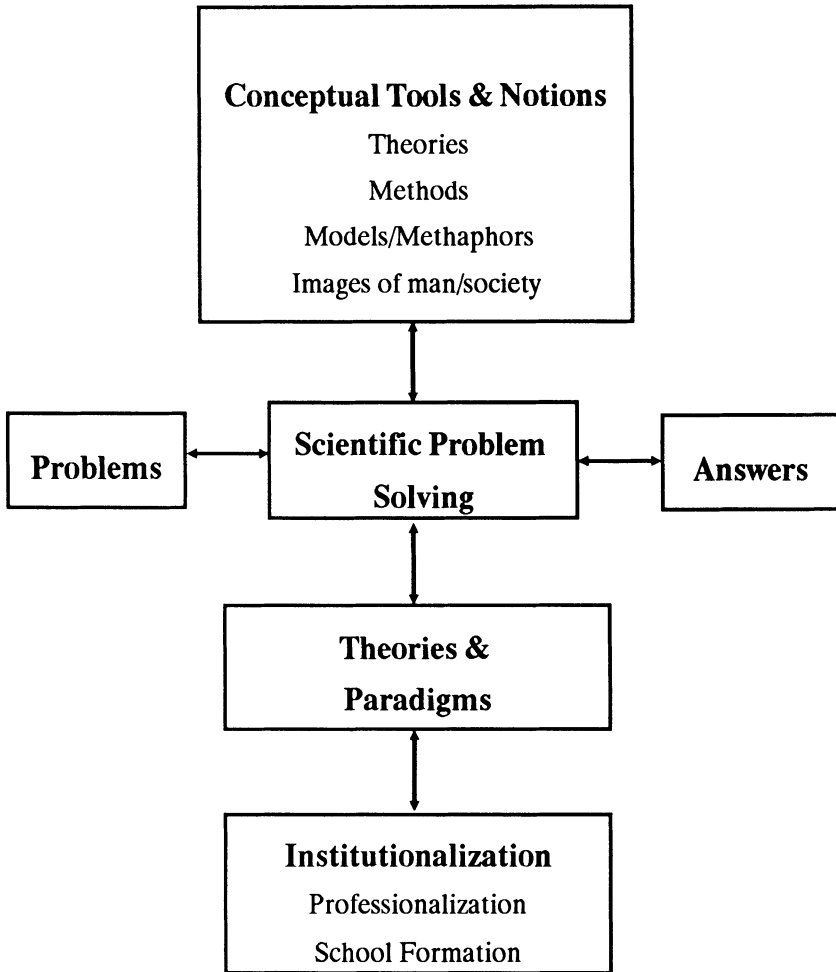
Figure 1. The components of scientific problem solving.

purely descriptive form this approach may be out of fashion, but it still forms the skeleton of most textbooks. In fact, the 'Problemgeschichte' for which van Rappard tries to make a case is not a history of *problems* at all but, rather a history of *answers*. Nevertheless, in his effort to systematically group separate theories and systems into two or three main streams or directions, he provides new directions for theoretical psychology. At a lower level of abstraction, 'embodiments' (Lindenfeld-Scheerer) can be seen as units corresponding to our paradigms (in the sense of exemplars) in historiography.

Institutional history. This approach to history is not directly referred to by Scheerer. Often it is part of the traditional theories and systems-approach, in which the rise and fall of the successive *schools* and the role of founders and other 'great men', form the obvious organizing principle.

Let me conclude this section with the explicit acknowledgement of the value of these various forms of historiography. Together they form the necessary backbone of the history of a science. They offer, however, only a limited insight into the dynamics of change in theory-formation and into the way the theoretical efforts of our predecessors can contribute to our own theoretical problem solving.

A Relational Model of the Context of Scientific Problem Solving

Let me now explain what I understand by a contextual analysis, and how it can contribute to a theoretical understanding. An analysis of the components of scientific problem solving, given in the preceeding section, can serve as a starting point. In addition to asking which problems were instrumental in giving rise to specific lines of problem solving, what conceptual tools were used, and so on, we also have to ask *who posed the problems*, and from where were the conceptual tools derived. Furthermore, one aspect has been under-exposed up to now. Figure 1 contains a box labeled 'answers' and, of course, the formulation of a new theory and the development of a new paradigm can be seen as an answer (and, indeed, this is the way I dealt with it in the foregoing). In fact, however, the 'problem-solving cycle' of science also has a much more direct and concrete side, usually, that of providing answers that are accepted by colleagues within, and interested laymen and clients outside the scientific world, as pertinent to their own problems and questions. This asks for a presentation that inspires confidence in the own contribution. I will call this the *legitimation* aspect of science. Passing the legitimation test is a precondition for a solution, or answer, becoming paradigmatic and getting institutional roots.

In Figure 2 the components of a contextual analysis are represented. In the center I placed the theory to which a contextual analysis pertains. As will become apparent below, theory should not be conceived of as merely academic theorizing, in the sense of 'pure science', but also comprises theories of practice; theory developed as a tool in solving practical problems. In the four boxes that surround the center box I have put the four types of contexts that follow from the exposition just given.

1) The *context of origination*: whose problems started the problem solving cycle from which theory originated?

2) The *intellectual context*: from where came the conceptual tools and notions that were used in conceiving the theory?

3) The *context of legitimation*: to whom had the resulting answers to be acceptable?

Four Reference Systems
Academic Community
Forum of the Discipline
Clients of Psychological Services
Wider Society

Intellectual Context
In which line are concep-
tual tools & notions?

Context of Origination
Whose problem?

↔ **Theory** ↔

Context of Legitimation
Who legitimizes
answers?

Institutional Context
Which material basis?
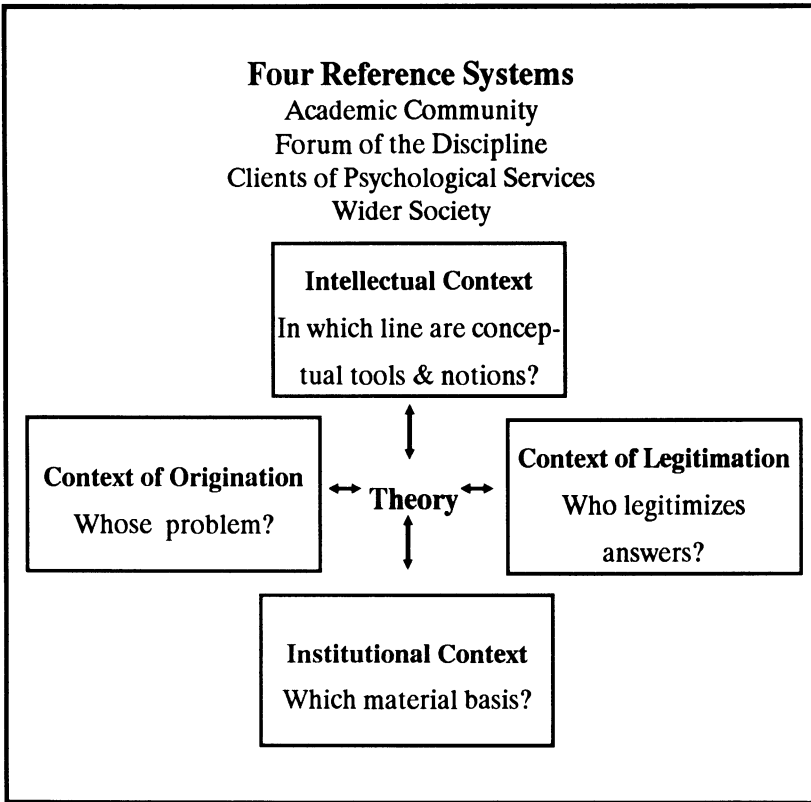
Figure 2. The components of a contextual analysis.

4) The *institutional context*: how did a theory get its paradigmatic
character and its institutional continuity (including becom-
ing part of the educational program of an university depart-
ment or professional institute, and receiving its place in the
textbooks)?

Of course, the contextual questions of the 'who' and 'which' have to be
answered in each historical investigation in its own specific way. Neverthe-
less, it is possible to bring some system into the contextual exploration by
discerning different types of 'audiences' towards which scientists and profes-
sionals are related in their work. In a study of the legitimation efforts of
psychologists in the Netherlands, during the pioneering and the consolidation
phase of their work, I found that the audience toward which psychologists
primarily directed themselves in demonstrating the value of their contribu-
tion changed during the successive stages of the development of their dis-
cipline. Essentially four audiences or 'publics' could be discerned (van Strien,
1984).

1) The *academic community*. Especially during the initial stages
   when a new science has to establish itself as a separate
   discipline, its proponents direct themselves to others in the
   academic world in order to defend their independent exist-
   ence and to draw demarcation lines between their discipline
   and other disciplines. During a later stage the academic
   community may be addressed again when there are common
   problems among disciplines or when the demarcation lines
   betweeen disciplines are questioned.

2) The *wider society*, or significant segments within it. In early
   Dutch psychology the different religious and political seg-
   ments into which Dutch society was split, formed the
   dominant 'reference systems' for psychologists in their ef-
   forts to gain recognition.

3) As a science matures to become a discipline, with own
   institutions and platforms of exchange (journals, conferen-
   ces, etc.), the *internal 'forum'* of the discipline — as part of
   the larger academic community — becomes the most
   predominant audience to which scientists, especially those at
   universities, address themselves. They seek the approval of
   their colleagues in order to get their ideas accepted, publish-
   ed, and taken up by others.

4) In so far as a science also aspires to develop into a profession,
   which offers services on practitioner-client bases, *clients*
   become an important public. Thus, during the stage when
   Dutch psychology began to present itself as a service-
   profession, it became necessary to convince potential clients
   that their problems are essentially psychological problems,
   and to popularize the blessings of psychological expert
   advice and treatment.

In a content analysis of sample publications, I showed that in the first
stage of Dutch psychology legitimation efforts were primarily directed
towards the academic community and wider society. During the interbellum
and during the first period after World War II, clients were the predominant
source of legitimation. Only during the sixties did the internal forum of the
discipline become the dominant audience towards which psychologists
directed their legitimative efforts.

As far as I can see, this distinction among four publics is not only
pertinent to psychology in the Netherlands but, by and large, also applies to
the context of legitimation of psychology in other countries (our four publics
can be, for instance, clearly recognized in Danziger's [1979] analysis of the
social origins of modern psychology in Germany and the United States), and

to the legitimation-activities in other disciplines. However, the four audiences or publics discerned, are not only relevant to the context of legitimation, but also play a significant role in the other three contexts, namely,

1) as sources of theoretical and practical problems;

2) as groups with which cognitive tools, concepts and notions are shared; and

3) as powers that have influence on the institutionalization process.

The concept of audience or public is less appropriate outside the context of legitimation and, hence, I prefer the concept of *reference systems* as a more general denotation. In a contextual analysis attention should be paid to the possible influence of all reference systems in the development of a science. However, it should be clear that a contextual analysis is not the same as a so-called externalistic approach to history. The 'internal' dynamics of theory-formation, as directed by the exchange and critical discussion within a scientific community, also gets full credit. As I will argue below, the polarization of internalistic and externalistic historiography should be avoided.

Before demonstrating, with the help of an example, how we have to proceed in formulating a recontextualizing analysis, I want to make two remarks on the contribution of the contextual-relational model proposed here to historiography in general. The figures I have presented may leave the historical investigator with a laming feeling of complexity; everything is relevant, one has to look in all directions! However, what I have presented here should be, in the first place, conceived of as a *heuristic checklist*. In my opinion the history of psychology is in want of well-chosen *case-studies*, in which those factors are selected and documented with historical data that are relevant for a good explanation.

My second remark is that only with the help of an analytical approach, such as implied in the model presented, can the move be made from a descriptive to an *explanatory* historiography. The guiding principle of my *theory of history* is that changes in the configuration of significant reference systems are a major explanatory factor in accounting for changes in historical orientation. But this is not the place to further develop this (see van Strien, 1987); rather, what concerns me here is not a theory of history, but theory in history and history in the service of theory.

Recontextualizing Exemplified

In what way can a contextual analysis contribute to a better understanding of how psychological theories develop and change? In a larger essay on the same topic (van Strien, forthcoming), I have illustrated that recontex-

tualization can shed light on 'the historical practice of theory construction' with the help of examples from the history of psychology in the Netherlands. In this contribution I will draw from material presented at this conference. The origins of Clark L. Hull's theory of value, and of neobehaviorist theories in general, as analyzed by John A. Mills (this volume), will serve as my main example.

Mills shows in his paper that Hull's theory should be understood as an answer to the program of the Progressive Movement in the U.S.A., namely, that of controlled social reform. In terms of our model, the Progressive Movement can be seen as the reference system within the academic world, and the intellectual elite within society as the context of origination. This intellectual stratum of society formulated the problems of modernizing industrial society in a way that made them open to psychological research. The (technocratic) image of man and society of the Progressive Movement provided the general intellectual context of Hull's thinking. However, the specific conceptual tools Hull used, were taken from behaviorism and couched in the methodological terminology of the American brand of logical-empiricism that fitted very well with the intellectual climate just sketched (Smith, 1986). Because of its promise to provide a basis for behavioral engineering — a promise further developed by later behaviorists — the scientistic and technocratic style of Hull's theorizing served as a welcome basis for legitimation by, and for winning approval in, the academic and disciplinary community of his days. It also formed a solid basis for uniting a group of disciples into a school that provided a firm institutional basis for Hull's work. It may be added that this school-formation was reinforced by Hull's personal style of leadership, which was quite different from the style of, for instance, E. C. Tolman (see Krantz & Wiggins, 1973).

The question remains what can the theoretician learn from this contextual analysis. Thus, while many will grant that taking the context into account can lead to a better understanding of history than, for example, a purely 'doxographic' account, the question remains whether the context also sheds new light on the text. Are not these philosophers of science correct who contend that the context of discovery is only of a historical interest, and that what really counts is the context of justification?

To get a clear answer to these questions, we should start from a contemporary perspective on learning theory. Current theorizing on learning can be compared to an estuary that is fed by several rivers. The widest of them is the river of behaviorism, that, in turn, gets its water from a number of larger and smaller rivers and streams. A recontextualizing analysis is only relevant for current theorizing if we start with the present and follow the stream upward to its various origins. As such a contextual analysis of, for instance, Hull's thinking solely is of historical interest. Recontextualizing gets its theoretical significance only when it is part of a larger venture into the tributary waters of

the present mainstream or — in other cases — into the dry seams of rivers that even now hardly contain any water. We gain by this insight into the original problem situations that formed the origin of lines of theorizing that shaped the present theoretical landscape. It contributes to what van Rappard has called a *historically informed theoretical psychology.* It helps us not only to reconstruct the context of origin of a theory, but also to trace its intellectual tributes, to recover the patent and more hidden ways of its legitimation, and to unravel the intricacies of its institutional establishment.

Of course, the value of a theory depends not on its birth certificate, but on its contribution to further problem solving along the line of a 'progressive program', as Lakatos (1970) calls it, or in terms of a viable paradigm as in the first section of this paper. But historically informed scientific problem solving is better able to recognize similarities with past problem-situations; avoiding the use of unsuitable or blunt conceptual tools, aware of the images of man and society, of ideological presuppositions implicit in certain approaches, of the rhetoric elements in legitimation, and of the sociological pitfalls of the institutionalization process. It cannot be denied that awareness of the history of current theories also leads to a sense of the relativity and temporality of all theories. Of course, this will only lead to resignation if we are still hoping for timeless Truth. Problem situations are always historically relative situations. Can we then expect the answers to be context-free?

Before concluding this section, I want to briefly address the question whether what we are now doing is still history. There is a difference, indeed, between history in the service of theory and history proper. The former looks for lines of continuity that lead up to the present. The danger in this approach is that we see only the seed from which current ideas have germinated. Moreover, if we consider the present as more perfect, we are liable to, what Stocking (1965) has dubbed, presentism. The latter is interested in past theorizing — including historical backwaters — only for the sake of finding out how the present came about. However, as long as we seriously follow up the contextual determinants of a theory, in the sense of the foregoing analysis, we are engaged in history, be it in the service of theory.

Three Barriers to Recontextualization

I want to conclude this contribution to the theory-history debate with a warning against three pernicious dichotomies that have to be avoided in contextual historiography and in recontextualized theorizing.

1) The *context of discovery — context of justification dichotomy.* From the preceding analysis it follows that justification (legitimation for the forum of the discipline) is itself part of the context of discovery. In so far as it makes sense to speak of justification, we have to take account of the attunement

to all four audiences or reference systems of science. But then, too, justification is only one determinant of the dynamics of science.

2) The *internal-external dichotomy*. Instead of two kinds of influences, one from within and one from without science, we found at least four reference systems in terms of which science is related to its theoretical and professional problem-solving activities. We can, of course, call the forum of the discipline the internal factor and all other reference systems external. In fact, however, the lines of demarcation are much more gradual and change in the course of history. These lines of demarcation are not given, but are constructed historically as part of the legitimation process. This implies that there is no sense in opposing problem-history and social history as, for example, van Rappard would have it. Both are part of a broader socio-intellectual history.

3) The *theory-practice dichotomy*. Theory is often given priority over practice. As I have already stated, 'theories of practice' are in no lesser sense theories than the, perhaps more abstract, 'theories of research'. As I have shown elsewhere (van Strien, 1986), there is a continuous interaction between both types of 'theories'. In fact, many 'grand' theories as, for example, Freud's, were developed in the context of practice. Again it is the reference system, the network within which the scientist is operating, which determines whether a problem solving activity is primarily called 'pure' or 'applied'. Ultimately, however, the term 'applied psychology' should be abandoned altogether.

In discussing the false conceptualizations that stand in the way of historical understanding of the development of science, we have broadened our scope from the contribution of history to theory to the contribution of history to the philosophy of science. Clearly, much more needs to be said, but this would exceed the bounds of this symposium. Suffice it to conclude that there is not only a need for historically informed theorizing in science but also a need for a historically enlightened science of science.

References

Danziger, K. (1979). The social origins of modern psychology. In A. R. Buss (Ed.), *Psychology in Social Context* (pp. 27-45). New York: Irvington.

Danziger, K. (1987). Social context and investigative practice in early twentieth-century psychology. In M. G. Ash & W. R. Woodward (Eds.), *Psychology in Twentieth Century Thought and Society* (pp. 13-33). Cambridge: Cambridge University Press.

Krantz, D. L., & Wiggins, L. (1973). Personal and impersonal channels of recruitment in the growth of theory. *Human Development, 16*, 133-156.

Kuhn, Th. S. (1962). *The structure of scientific revolutions*. Chicago: University of Chicago Press.

Kuhn, Th. S. (1970). *Postscript* in: Kuhn (2nd ed.).

Lakatos, I. (1970). Falsification and the methodology of scientific programs. In I. Lakatos & A. Musgrave (Eds.), *Criticism and the growth of knowledge* (pp. 91-196). Cambridge: Cambridge University Press.

Pongratz, L. J. (1967). *Problemgeschichte der Psychologie*. München: Francke.

Smith, L. D. (1986). *Behaviorism and Logical Positivism. A Reassessment of the Alliance*. Stanford, CA: Stanford University Press.

Stocking, G. W. (1965). On the limits of 'Presentism' and 'Historicism' in the historiography of the behavioral sciences. *Journal of the History of the Behavioral Sciences, 1*, 211-218.

Strien, P. J. van (1984). Psychology and its social legitimation, the case of the Netherlands. In S. Bem, H. Rappard, & W. van Hoorn, (Eds.), *Proceedings of the Second European Meeting of Cheiron, Heidelberg* (p. 80-89).

Strien, P. J. van (1986). *Praktijk als wetenschap; methodologie van het sociaalwetenschappelijk handelen*. Assen: Van Gorcum. English Abstract in *Cheiron-European Newsletter*, Winter 1988, pp. 51-52.

Strien, P. J. van (1987). The mediation between science and society in the history of psychology; a theory and a case-study. *Paper* presented at the 6th Cheiron Conference for the History of the Behavioral and Social Sciences, Brighton, U.K., September 1987.

Strien, P. J. van (forthcoming), The historical practice of theory construction. (To be published in: H. van Rappard, P. J. van Strien, & L. Mos (Eds.), *History and Theory, Annals of Theoretical Psychology*, Vol. VIII).

IN PRAISE OF 'PROBLEMGESCHICHTE'

Hans van Rappard

SUMMARY: In this paper the concepts of theoretical psychology and history of psychology are examined. With regard to the former, a case is made for the Koch-Bergmann-Madsen conception of theoretical psychology as meta-psychology. It is further argued that theoretical psychology as theory-construction may also be seen as metapsychology. Next, history of psychology is scrutinized. Two main approaches to the field will be dealt with: (1) the social-historical, and (2) the intellectual-historical (intellectual history, *Problemgeschichte*). It is held that from a systematic point of view the intellectual-historical approach has the advantage over social history that it may have a direct bearing on the discipline. Finally, it is contended that for this reason the link between theoretical psychology and intellectual/problem history is likely to be fragile. So fragile indeed, that the latter may be seen as forming part of theoretical psychology.

In the title of the present symposium on history and theory in psychology two concepts can be noted whose meaning is ambiguous and often controversial. Therefore, I will take a look at 'history of psychology' and 'theoretical psychology'. As can be noted below, the scrutiny of these concepts will in itself wellnigh suffice as a discussion of the theme of the symposium.

Theoretical Psychology

Let me first take up theoretical psychology. In 1984 Michael Hyland and the present speaker struck up a correspondence on the question "What is theoretical psychology?" As a consequence of this discussion the idea came up to organize a conference in Plymouth — which indeed took place the following year — and a questionnaire was circulated to elicit opinions on our correspondence topic. The response to the questionnaire suggested two broad meanings of theoretical psychology: metatheory, and the development of new theory.

The only rounded-out conception of theoretical psychology that I know of is the Bergmann-Koch-Madsen view. Rappard (in press) pointed out that Madsen (1987) derived a good part of his "definition and systematic classification of theoretical psychology" from Bergmann (1953) and Koch (1951). In particular he concurred with Bergmann's view of theoretical psychology as "a branch of the philosophy of science (which) not itself a science, is about science" (Bergmann, 1953, p. 435). Consequently, theoretical psychology is a branch of metascience, that is, theoretical psychology is *about* psychology. What this means is nicely phrased by Gergen, even if he takes an entirely different philosophical position than Bergmann's positivism.

> The practice of prefixing *meta* to the name of a discipline is usually meant to
> designate a "higher", or transcending, discipline. It deals with problems that
> are both beyond the scope of the original and more fundamental. *Meta-*
> *psychology*, then concerns itself with the foundational problems of psychol-
> ogy. (Gergen, 1987, p. xiii)

The details of Madsen's first outline (Madsen, 1959) were borrowed
from Koch's 1951 program for theoretical psychology, especially points 3 and
4: "differential analysis of conflicting theoretical formulations," and the con-
struction of new theory conceived as "systematising" already formulated laws.
Later, this view was changed into the three-leveled conception (Madsen,
1974). In the 1959 draft, however, no distinction was yet made between the
description level, the theory level, and the philosophy level of metapsychol-
ogy. In these levels one can see the influence of Kuhn (1962) whose historical
(i.e., *descriptive*) study of scientific *theories* had a tremendous *philosophical*
impact.

Now, for reasons given elsewhere (Rappard, 1990) I reject Madsen's
three-floored design of theoretical psychology but I do accept the general
Bergmann-Koch-Madsen conception of theoretical psychology as meta-
psychology and I do concur with the specifically Madsenian view that
philosophy, history, sociology, and psychology of science may provide useful
perspectives on the field.

Speaking of psychology as the object of metapsychology implies
psychological theories in a broad sense of the word: comprehensive theories,
psychological systems, and even 'psychologies' *tout court*. This broad meaning
of the theory concept is probably typical of the mental and social sciences. In
the natural sciences, theory is usually understood in a more restricted and,
shall we say, technical sense. But in this sense the concept of theory is used in
psychology too, of course. And where this is the case, theoretical psychology
would seem to assume a correspondingly restricted meaning. But arguably it
would still be *meta*psychology. For instance, in the preface to his *Introduction*
to Theoretical Psychology, Hyland (1981, p. viii) writes: "This is the first
textbook to be written solely on theory construction and theory testing in
psychology — which is what I term theoretical psychology."

On the other hand, the development of new theory has in the past often
involved developing "grand theory" and even today this is sometimes en-
deavored (Staats, 1983). In other words, the precise meaning of theoretical
psychology seems to hinge on the scope of the pertinent theory. Hence, it
would seem that theoretical psychology in the sense of 'being about theory'
covers both meanings that came to the fore in the 1984 questionnaire.

History of Psychology

After this hasty sketch of my interpretation of 'theoretical psychology', it is time to take up the second concept featured in the symposium title: history of psychology.

Leaving aside the probably outdated but usually almost parodied 'great men/women' approach, two broad perspectives on history of psychology are available: the social-historical, and the intellectual-historical (Petzold, 1984). It should be clear that I am using these labels here in a very general and possibly unduly dichotomous way. I would also like to stress that in the following I will be primarily concerned with the *systematic* question of the relation between the history of psychology and theoretical psychology. From this position, it would in my view appear that the social-historical perspective has the inherent diadvantage of being relatively far removed from the scene of psychology proper. Social history seems of necessity confined to what I would like to call, a *bystander* perspective.

Most of us will get our pay cheques from a psychology department. But if you are not an empirical scientist you are bound to be regarded as a rather peculiar kind of colleage. Although we have apparently learned to live with that I see no reason why one should wish to exacerbate the situation. To put matters more seriously, by choosing to live your professional life as a social historian of psychology you tend to place yourself apart from the day-to-day activities and concerns of your colleagues. And this will be even more the case when you choose the so-called critical variety of the social-historical perspective. Many of us have already had an opportunity to see what may well happen then; the empiricists go on empiricizing and the critics go on criticizing and the twain never meet. Worse still, given the opportunity the empiricists will happily dismiss the critics thus making room for more responsible colleagues. But let me try to put this matter in a more systematic way.

As we all know, Kuhn's (1962) work has heralded an empirical turn in metascience. In the wake of this empirical shift, history and sociology became essential metascientific approaches. Consequently, contemporary metascientists are usually not very much interested in the traditional claims of science on the production of objective knowledge. In this respect, metascience can be said to have become agnostic. Nowadays it focusses on what scientists actually do, as distinguished from the results of their activity. The new emphasis has produced fascinating views, along with a healthy demythologizing of science. However, according to Boon (1989), a Dutch metascientist and a sociologist by training, metascience paid a price. The current perspective is arguably at odds with the needs of empirical scientists. Is it indeed a good thing for metascience to take an independent stance *vis á vis* its object of study? Should metascience really distance itself from science? Boon is of a different opinion. I paraphrase his view as follows:

The discussion by metascientists *of* science has rather eclipsed their discussion *with* scientists. The latter are concerned with the advancement of knowledge. Their work towards this aim would become meaningless, however, if the stakes were no longer truth and falsity. Current metascience and the philosophy of science supporting it have pointed out that it is not possible to algorithmically end debates by means of certainty criteria developed by philosophy. In view of this, however, the unsalubrious conclusion has been drawn that the universalistic claims of science to objectivity and truth are merely artefacts of a wrong philosophy, rather than an inherent part of the scientific enterprise. Hence, the lifeline between metascience and science is in danger of being severed. (Boon, 1989)

It is clear that Boon does not applaud this particular 'externalist' shift. As he sees it, metascience ought to become 'native', that is, the problems that empirical scientists are trying to solve should be taken seriously, along with their claims to truth and objectivity.

I would like to endorse this position. I feel strongly that the history of psychology should be conformable with the rest of the psychological enterprise. And that is where intellectual history might come in.

Intellectual History/Problemgeschichte

Earlier in this paper, I pointed out that 'intellectual history' was being used in a broad sense and, moreover, as dichotomous to social history. Strictly speaking, we should distinguish between intellectual history and history of ideas, and — in German — between *Begriffsgeschichte* and *Problemgeschichte*. Scheerer (in this volume) has given a detailed review of these concepts, plus a few more. It is my contention, however, that the differences are largely irrelevant to current historiography and some support for this view may be found in Scheerer's paper. Some of the distinctions are recent and have as yet had no impact, whereas other concepts seem outdated or specifically German. Nevertheless, I would think that in terms of Scheerer's review the approach advocated here as probably most congenial to theoretical psychology is found in history of ideas (in the narrow sense of the term) and in *Problemgeschichte*. In the Anglo-American literature, however, these fine-grained distinctions are not commonly found. Intellectual history and history of ideas are often used interchangeably. In this paper the term intellectual history will henceforth be used in a general sense, that is, comprising history of ideas, and interchangeably with *Problemgeschichte*.

According to the general historial John Tosh, intellectual history originated in the 19th century, as did scientific history at large, and should be considered a distinct speciality. It includes political, economic, and social thought, theology, scientific thought, and the values and assumptions expressed in the writing of history itself. Most work in intellectual history or *Problemgeschichte* deals with political thought and is based on the assumption

that what gives history its coherence and continuity is the power of ideas and values to shape human destiny. To explain the evolution of ideas is to explain the process of history itself. As Larry Laudan (1977) put it, "what our ancestors *thought* is as interesting as what they *did*" (p. 171). Important intellectual historians are J. G. Droysen (1808-1884), on whose innovative methodological ideas Pongratz (1967) grounds his *Problemgeschichte*, and R. G. Collingwood (1889-1943) from whom Robinson's *Intellectual history of psychology* (1976) derived a great deal of inspiration.

In the 20th century *Problemgeschichte* drew heavy fire from various directions. On the one hand, the Freudian concept of the unconscious induced a fair bit of scepticism as to whether professions of belief bear much relation to what people actually think, let alone do. And on the other hand, the Marxist interpretation of history came as a full-scale attack on the assumed autonomy of ideas. Tosh writes:

> The result of these changes in the intellectual climate is that the pretensions of today's historians of ideas are more modest than those of their predecessors, and they do not claim the same autonomy for their field. Their work continues to be significant because, although social and material conditions may place limits on the range of ideas which can gain acceptance in any age, they certainly do not determine the precise form which those ideas take. Much can only be accounted for by the inventiveness of the human mind and by the power of tradition (Tosh, 1984, pp. 69-70).

As mentioned above, intellectual history went out of sight when diving for cover under the bombardment by Freudians and Marxists. It seems, however, that the times have changed. Ideas are no longer considered mere ideological reflections of the economic base and, according to general historians intellectual history has become respectable again (Ankersmit, 1986).

According to Laudan (1977), however, who devoted an entire chapter of *Progress and its problems* to the defence of this approach, "the history of ideas is regarded in many quarters as passé and irrelevant, as a discipline with outmoded presuppositions and outrageous ambitions. Many historians see intellectual history as an anachronistic excrescence on the scholarly and ideological integrity of their field" (pp. 171-172). Laudan also cites a number of complaints against *Problemgeschichte* such as its assumption that ideas have an independent reality, and that ideas are a far less potent source of change than the underlying socio-economic realities (p. 172). But John Tosh has already taken care of these complaints.

Intellectual history maintains that ideas can be pretty tenacious and tend to live longish lives. This is easy to appreciate for the participants of this conference. On the program is a discussion between professors Gergen, Giorgi, and Tolman. If you reflect briefly on the foundational backgrounds of the discussants, you cannot but grasp the tenacity of foundational ideas in psychology; Giorgi takes his starting point in phenomenology, while Tolman is committed to Marxism. Neither is exactly new, nor are Gergen's founda-

tional backgrounds. Gergen (1982) put them together in the concept of the endogenic worldview, which — set up against the Anglo-American exogenic worldview — enabled him to describe the history of social psychology as a longtime zigzagging between the two.

The mere fact that it is possible for one to take such an approach may be understood as another example of the tenacity of what van Strien (in this volume) likes to call, the conceptual tools of the discipline. The two conceptual frameworks sketched by Gergen can in fact be construed as much more tenacious than he saw them to be. Thirty years ago, Gordon Allport (1955) conceived of the so-called Leibnizian and Lockean traditions, which because of their similarities with the endo- and exogenic worldviews greatly add to the timespan of these latter two — and hence to the tenacity argument. Coan (1968), Eisenga and van Rappard (1987), Watson (1967), and Wertheimer, 1972), to name but a few, have also conceived of psychological mainstreams, that is, persistent clusters of foundational ideas, and all these studies provide examples of intellectual history.

In Pongratz' *Problemgeschichte der Psychologie* (1967) intellectual history is characterized as a longitudinal section of history (*Längsschnittsbetrachtung der Geschichte*). Such a section is not made haphazardly but is typically guided by questions pertaining to object, theory, and/or method of psychology. In other words, foundational problems. This also means that the problems mentioned in the word *Problemgeschichte* should not be taken too literally. 'Problem' here refers to the relatively fundamental questions that have persistently plagued the discipline, and the concepts used to phrase them. This is one more reason, incidentally, why I find it hard to distinguish between history of ideas and *Problemgeschichte*. Moreover, although it is probably alright to argue, as van Strien (this volume) does, that *Problemgeschichte* in most cases comes down to "the history of answers given in the course of time," I fail to see how this view bears on the present argument. Problems — answers — what's the difference? Is introspection in Brentano an answer to certain difficulties seen by him in Kantian philosophy, or is it a fundamental problem in nineteenth century psychology? And is it not the concept of introspection that is needed in either case? I could elaborate on this point: since Pongratz wrote his study in the mid-sixties, it has become possible to add to his set of guiding questions another set consisting of the key concepts of post-positivistic philosophy of science such as paradigm, research program, and research tradition. Not many historians have used these foundational concepts yet, but it has been done. Recently, Madsen (1988) used the paradigm and Elbers (1988) the research program, while Larry Smith (1985) made some use of the research tradition.

According to Pongratz:

> ... there is no better way to demonstrate what science essentially is all about. History introduces one to the problems in their developmental context, *im Hin und Her des Fragens*, and in the fire of controversy. Hence, these

> problems become clearer and more transparent. But there is more. An (intellectual) historical perspective may draw one's attention to topics and aspects that supplement current research and point to alternative solutions. In short: *Problemgeschichte* demonstrates not just the origin and development of the foundational problems of a discipline but contributes to their clarification. Conceived in this way, (intellectual) history forms an essential part of foundational research (*Geschichte ist ein notwendiger Teil der Grundlagenforschung*) (Pongratz, 1967, p. 10; present author's translation).

Robinson defends a similar view, writing "The history of psychology points at least generally to those paths the discipline is likely to take. It even anticipates those that might well be deadends" (1976, p. 25). In these observations on the possible use of doing history the exemplary view of history may be recognized, which traces back to antiquity.

A more specific example of the kind of longitudinal section that was advocated above may be found in *Memory in historical perspective* (Herrmann and Chaffin, 1988). The book presents a collection of classical texts on this topic. According to the editors, knowledge of the literature before Ebbinghaus,

> ... allows the student of memory to place a question about memory in its historical context and to distinguish those questions that are really new from those that have been debated for centuries. Scholars who are ignorant of history may pursue intractable approaches or may rediscover phenomena and theoretical constructs that have been thoroughly discussed in earlier eras (...) Rediscovery is not a rare event in psychology, or in other disciplines. To illustrate the point, imagine what most memory psychologists would say when asked to name the major phenomena of memory and the people who discovered them. For example, who discovered: abstract/concrete memory codes, attributes of encoding, levels of processing, and the semantic-episodic distinction. The typical answer would likely be the names of twentieth-century psychologists; yet all of these phenomena were discovered by pre-Ebbinghaus scholars. (Herrmann and Chaffin, 1988, p. 4)

Conclusion: History and Theory

What I have tried to do in this brief article is to make a case for intellectual history as the proper pursuit of history of psychology for those who would abhor the idea of falling entirely out of step with the rest of the discipline.

It should be clear that, as seen by Pongratz and Robinson intellectual history is typically concerned with foundational problems and ideas. It is my contention that by virtue of this foundational perspective the link between *Problemgeschichte* and theoretical psychology becomes pretty fragile. It may become even more fragile when I tell you that according to some general historians there has always been a very close connection between intellectual history and philosophy (Ankersmit, 1986). There is thus in my opinion nothing to be gained by continuing to think of history of psychology and theoretical psychology as separate fields.

An argument for the view that *Problemgeschichte* and theoretical psychology are inherently related may be derived from my tenacity argument. Social psychology's zigzagging between the two worldviews outlined by Gergen (see above) does not only entail the tenacity of many foundational ideas in the discipline but also, and more importantly, that these ideas are very much alive – that they are relevant to psychology as currently pursued.

If you accept this point of view, I can see no reason why you should not want to take the next step and accept that history and theory are inextricably linked. As Robinson put it, in a purely Collingwoodian vein, "psychology *is* the history of ideas" (1976, p. 413). Many of the basic concepts of the past, in other words, are relevant to, nay, are part and parcel of psychology as currently pursued.

I submit, then, that *Problemgeschichte* may be conceived as an inherent part of theoretical psychology and that the latter might, therefore, be thought of as a 'historically informed meta-psychology'.

References

Allport, G. W. (1955). *Becoming.* New Haven: Yale University Press.

Ankersmit, F. R. (1986). *Denken over geschiedenis.* Groningen (Netherlands): Wolters-Noordhoff.

Bergmann, G. (1953). Theoretical psychology. *Annual Review of Psychology, 4,* 435-458.

Boon, L. (1989). Wetenschapstheorie en historisme. In L. Boon & G. de Vries (Eds.) *Wetenschapstheorie,* (pp. 108-117). Groningen (Netherlands): Wolters-Noordhoff.

Coan, R. W. (1968). Dimensions of psychological theory. *American Psychologist, 23,* 715-722.

Eisenga, L. K. A., & Rappard, J. F. H. van (1987). *Hoofdstromen en mensbeelden in de psychologie.* Meppel (Netherlands): Boom.

Elbers, E. (1988). *Social Context and the child's construction of knowledge.* Dissertation, University of Utrecht (Netherlands).

Gergen, K. J. (1982). *Toward transformation in social knowledge.* New York: Springer-Verlag.

Gergen, K. J. (1987). Introduction: Toward metapsychology. In H. J. Stam, T. B. Rogers, & K. J. Gergen (Eds.), *The analysis of psychological theory: metapsychological perspectives,* (pp. 1-21). Washington: Hemisphere.

Herrmann, D. J., & Chaffin, R. (Eds.) (1988). *Memory in historical perspective.* New York: Springer-Verlag.

Hyland, M. (1981). *Introduction to theoretical psychology.* London: Macmillan.

Koch, S. (1951). Theoretical psychology 1950: an overview. *Psychological Review, 58,* 295-301.

Kuhn, T. S. (1962). *The structure of scientific revolutions.* Chicago: University of Chicago Press.

Laudan, L. (1977). *Progress and its problems.* Berkeley, CA: University of California Press.

Madsen, K. B. (1959). *Theories of motivation.* Copenhagen: Munksgaard.

Madsen, K. B. (1974). *Modern theories of motivation.* Copenhagen: Munksgaard.

Madsen, K. B. (1987). Theoretical psychology: a definition and systematic classification. In W. J. Baker, M. E. Hyland, H. V. Rappard, & A. W. Staats (Eds.), *Current issues in theoretical psychology.* (pp. 165-174). Amsterdam: North-Holland.

Madsen, K. B. (1988). *A history of psychology in metascientific perspective.* Amsterdam: North-Holland.

Petzold, M. (1984). Methoden und Theorien psycholgiegeschichtlicher Forschung. In H. E. Lück, R. Miller, & W. Rechtien (Eds.), *Geschichte der Psychologie,* (pp. 3-10). München: Urban & Schwarzenberg.

Pongratz, L. J. (1967/1984). *Problemgeschichte der Psychologie.* Bern: Francke.

Rappard, H. V. (in press). History and system. In H. V. Rappard, P. J. van Strien, & L. P. Mos (Eds.), *Annals of theoretical psychology (Vol. 8).* New York: Plenum.

Robinson, D. N. (1976). *An intellectual history of psychology.* New York: Macmillan.

Smith, L. D. (1985). *Behaviorism and logical positivism.* Stanford, CA: Stanford University Press.

Staats, A. W. (1983). *Psychology's crisis of disunity.* New York: Prager.

Tosh, J. (1984). *The pursuit of history.* London: Longman.

Watson, R. I. (1967). Psychology: a prescriptive science. *American Psychologist, 22,* 435-443.

Wertheimer, M. (1972). *Fundamental issues in psychology.* New York: Holt, Rinehart and Winston.

HOW CAN INTELLECTUAL HISTORY HELP US TO UNDERSTAND PSYCHOLOGICAL THEORIES?

Eckart Scheerer

SUMMARY: An attempt is made to describe the uses of intellectual history for understanding and criticizing psychological theories. The historical analysis of mental products works at three levels, defined by the degree of articulation of mental products and the broadness and type of social support for them. The top level comprises the doxography of theories, the history of problems, the history of concepts, and the history of ideas. The middle layer is the domain of intellectual history proper. The bottom layer consists of the study of mentalities and belongs to historical psychology rather than to the history of psychology. A model for intellectual history recently proposed by Lindenfeld is reviewed, where a distinction is made between systems and embodiments and their respective social functions. Lindenfeld's model is applied to explain certain peculiarities of German psychology during the Weimar Republic. It is concluded that the widespread and often purely rhetorical use of the *Ganzheit* concept indicates that the concept had the function of an embodiment in Lindenfeld's sense.

In the present report I want to defend the utility of intellectual history for the understanding and criticism of psychological theories. I shall first sort out some approaches to intellectual history and their potential use for the historiography of psychology. Second, I shall briefly review a recent model of intellectual history. And third, I shall try to show the fruitfulness of this model for understanding certain peculiarities of German psychology in the Weimar Republic.

Approaches to Intellectual History

Let me start with a disclaimer. I do not want to oppose intellectual history and social history as two mutually exclusive explanatory approaches. The study of intellectual history does not hinge on any metaphysical presuppositions about the existence of disembodied, 'eternal' ideas or on a Popperian 'third world' conception. In fact, I would be quite content with describing ideas as the *explanandum* and their social, political and economical context as the *explanans* of the history of science, though on second thought this seems a bit too unidirectional. On the other hand, if we want to *explain* ideas by their social context, our first task will be to *describe* them as accurately as possible — and this alone is sufficient to justify the autonomous study of the history of ideas.

At a descriptive level, intellectual and social history differ with respect to a number of dimensions, such as subject matter (theoretical content *vs.* practice and function of science), level of analysis (individual scientists or the 'average scientist' *vs.* the scientific community in its internal structure and external relations), and data and methods (textual analysis and interpretation versus statistical or quantitative data from a variety of fields such as economics or demography). To be sure, there will be some overlap — for instance, scientific texts often carry a social message even if not intended and thus are material for the social historian — but on the whole the picture just drawn will not be totally inaccurate.

Up to now I have treated intellectual history *(Geistesgeschichte)* as if it were a unitary enterprise, and I have used the terms 'intellectual history' and 'history of ideas' as if they were synonymous. But this is a simplification. For several decades (an important pioneering paper was Mandelbaum, 1965) philosophers and historians have been debating the status of historiographical approaches to mental products in the broad sense, but these debates have had little impact among historians of psychology. While I cannot pretend to have a complete knowledge of the relevant debates, I think that the following outline sketch is appropriate.

There is a three-layer structure resulting in the fields of the history of ideas (in the broad sense), of intellectual history, and of the historical study of mentalities. These three layers are ordered in terms of at least two dimensions: the degree of articulation and transparency of the mental products, and the extent to which they are shared by the members of a society. Thus, as we move from the top layer to the bottom layer (these spatial terms are not meant to be evaluative), we move from a fully systematized set of propositions making up a scientific or philosophical theory, and perhaps underlying works of literature and the fine arts, the former accessible only to a limited number of specialists or at least highly educated people, through the somewhat more diffuse world views and presuppositions shared by the educated strata of society independent of their specialty, to the often opaque and implicit beliefs and attitudes shared by an entire society or at least by one of its major constituents or classes.

The bottom layer — that of *mentalities*, dealing with what is best termed as 'popular culture' — though perhaps of most interest to general history, is probably least relevant to the history of psychology. In fact, the study of mentalities is one promising approach to building a *historical psychology*. This need not be totally irrelevant to the history of psychology, inasmuch as mentalities might be responsible for some very general features of psychological thought; for instance, the predominance of cognition over action as a 'constant' of classical Western psychology, ultimately arising from the mentality of the slave-holders in classical antiquity. But systematically, it certainly belongs to historical psychology rather than to the history of psychology.

The top layer, on the other hand, has as one of its subdivisions the history of psychology and more precisely its *doxographic* part, that is, the description and analysis of psychological theories. Doxography seems to be pretty much out of fashion today, but I believe there are still important tasks for it. Doxography will never end, not only because new theories are constantly arising, but also because it will always be colored by the viewpoints of the doxographer and must be rewritten when another viewpoint is adopted. As a rule, the particular viewpoint of the doxographer will induce distortions and even outright errors; the best-known example is Boring's (1950) notorious misrepresentation of Wundt's system.

In addition, doxography carries the danger, pointed out by van Strien at this conference, of decontextualization. One antidote against this is to assume a *problem-oriented* approach, to turn to *Problemgeschichte* as advocated by van Rappard. Though certainly not constituting the mainstream of psychological historiography, the problem-centered approach (started by Klemm, 1911) does have a fine tradition in our field. Its obvious limitation is that it tends to assume that across history the problems are constant and only the answers are variable. Are there, at a theoretical level, 'perennial' problems of psychology? Somewhat surprisingly, a Soviet historiographer (Jaroschewski, 1975) has answered in the affirmative and has identified a set of such problems (the psychognostic, psychophysical, psychopractical, psychobiological, and psychosocial problems). But the 'perennial' nature of these problems becomes doubtful when we consider that the concepts entering into them (such as mind, body, society, action, and so forth) do not have a constant meaning across history.

This brings us to a third approach at the top layer, the *history of concepts*. Up to now, *Begriffsgeschichte* has been practiced almost exclusively in Germany, where it is a minor industry that has resulted in the publication (still in progress) of three encyclopedias (*Historisches Wörterbuch der Philosophie, Geschichtliche Grundbegriffe, Handbuch politisch-sozialer Grundbegriffe in Frankreich, 1680-1820*) and has a specialized journal (*Archiv für Begriffsgeschichte*). On the face of it, these repositories of knowledge, which only recently have attracted the interest of the international community of intellectual historians (Kelly, 1987; Richter, 1987), have little relevance to psychology, specializing as they do in philosophy and in political science. But psychology, after all, used to be a subfield of philosophy, and at any rate the history of concepts by its very nature is interdisciplinary; in the *Historisches Wörterbuch der Philosophie*, provided that they have philosophical relevance, concepts are traced through their entire history, disregarding disciplinary boundaries as seen from the standpoint of today. The history of concepts focusses on the diachronic and synchronic *variability* of concepts, the only constant element often being the term used to denote a concept. Sometimes this has curious effects. For instance, the entry on psychology (Scheerer,

1989a) starts neither with Aristotle nor with Wundt, but in the Renaissance period, where the term 'psychology' made its first appearance.

Its focus on variability and its close ties to terminology set *Begriffge-schichte* apart from the related but different endeavor of the *history of ideas*, which is about as American as the former is German. In order to avoid confusion with the history of ideas in the broader sense, which I have used as a label for the entire 'top layer' of intellectual historiography, I refer to the movement started by Lovejoy (1936) as history of ideas in the narrow sense. The original program, as put forward by Lovejoy, rested on the assumption that the human mind disposes of a limited number of 'unit-ideas', which make their appearance and reappearance throughout history and may be traced through various historical epochs and various provinces of human productivity, such as philosophy, science, literature, and the arts. In a sense, Lovejoy's approach is similar to the problem-centered approach outlined above, only that he was dealing with answers or solutions rather than with problems. Small wonder, then, that his program has been criticized on similar grounds, on account of the presumed timelessness of the ideas and the lack of a contextual analysis. But if the history of psychology is to make sense at all, it certainly must display some minimum of continuity, and I think that various intellectual traditions are sufficiently robust to qualify as unit-ideas à la Lovejoy. Think, for instance, of the 'ghost in the machine' metaphor, or of the various memory metaphors (storage, imprinting, etc.).

The four approaches outlined here are not mutually exclusive. Rather, they all are required for a complete historical analysis of psychology at the level of articulated ideas held by psychologists across the ages. But they do not yet exhaust the province of intellectual history as applicable to our field.

A Model for Intellectual Psychology

Intellectual history, in the narrow sense, constitutes the 'middle layer' in our system of historiographical approaches. As Krieger (1973) noticed, the "terminological distinction between intellectual history and the history of ideas is recent" and goes back, in the Anglo-Saxon world, to the fifties of this century. But Krieger already was able to describe a relatively well-structured demarcation between the two. In the history of ideas, "articulate concepts have themselves been the primary historical agents, with their personal bearers and external relations adduced as conditions of them." Intellectual history "has included inarticulate beliefs as well as formal ideas; and its primary unit of historical concern has not been the set of these notions as such but rather their external relations with the larger life of the people who have borne them" (Krieger, 1973, pp. 500-1).

Thus conceived, intellectual history is apt to serve as a connecting link between social history and the more articulated notions studied at the top

layer. However, so far it has been used more in the historiography of litera-
ture, the arts, and politics than in the history of science, let alone of psychol-
ogy. Perhaps Boring's (1950) use of the *Zeitgeist* concept is an example of the
spontaneous adoption of a viewpoint akin to intellectual history. But the
concept, as used by Boring, was totally amorphous and must be replaced by
more sophisticated analytical tools.

One recent model I find attractive has been put forward by Lindenfeld
(1988). According to him, intellectual history deals with *systems* and *embodi-
ments*. A system is "a complex body of thought related in a coherent fashion."
Systems are ways of "constituting and representing meaning," and from this it
follows that they have to be understood as wholes; the meaning of concepts
within systems will be distorted if studied in isolation. In its social function, a
system is "a particular way in which ideas are shared or communicated."
Systems may vary in scope, tightness, and social support and range from
Kuhnian paradigms to the ideology of the German high-educated subculture
as analyzed by Ringer (1969).

Embodiments are ways of "fixating or condensing a complex of meanings
into a single expression." Thus, they are not articulated, although "their
meaning could easily be spun into a system." They often serve as a "focus of
personal or group identification," and they "are communicated quickly and
easily, because of their relative simplicity and immediacy." In addition,
embodiments have the important property that they may be shared by dif-
ferent systems and thus may "serve as vehicles of communication among the
groups which these systems help to define." Embodiments may be both
abstract and concrete; they occur in mythological as well as in discursive
thought. Again, Lindenfeld draws a parallel to Kuhn's theory in identifying
the 'exemplars' or 'prototye cases' with a particular class of embodiments.

Though like most intellectual historians Lindenfeld draws his examples
mainly from the history of political thought, I think that his analysis can
fruitfully be applied to some problems in the historiography of psychology.
More specifically, I believe that the *Zeitgeist* conjured by Boring finds its
expression in embodiments rather than in systems. Consequently, the iden-
tification and study of embodiments may be an important means to reveal a
hidden unity between psychological theories and systems that on the surface
are very different from each other. Given that embodiments typically have an
ideological function, their study could help us to identify ideological moments
of psychological theories.

An Illustration of the Model

Let me briefly illustrate these claims by a concrete example from the
history of psychology. On the surface of it, *German psychology in the Weimar
period* (1918-33) presented a bewildering multitude of theories or schools.

The diversity was so puzzling that a need was felt for textbooks or manuals enumerating and describing the 'schools' (e.g., Henning, 1931; Messer, 1927; Müller-Freienfels, 1929; Saupe, 1928). These schools certainly were not homogeneous with respect to the systematic coherence of the views held by them; but I think it is fair to identify them with 'systems' in the sense of Lindenfeld. Compared to the Kuhnian paradigm concept, the system concept has the important advantage of allowing for the co-existence of numerous systems at one time. At least some of the psychological systems also had specific 'exemplars' or 'prototype cases' used for initiating students to the technicalities of the craft.

Throughout the Weimar period, an intense polemic was carried out between the adherents of the different schools. There was also the pervasive feeling, not the least fostered by the diversity of schools, that psychology was in a profound state of crisis (see Hildebrandt, 1989, for a diagnosis of the reasons for the "crisis of psychology"). Nevertheless, perusal of the texts (which cannot be documented here, but see Scheerer, 1985) shows that there was also a substantial consensus between the schools. In the present context, one unifying moment is of particular interest. As far as I see, all schools (including those that by all reasonable standards would be considered elementarist and mechanist) claimed to acknowledge the *holistic nature* (the *Ganzheitlichkeit*) of mental life.

Several features should be noted about the all-pervasive *Ganzheit* talk of Weimar psychologists:

First, though the *Ganzheit* concept and its semantic neighbors (such as *Gestalt* and *Struktur*) may have had a reasonably precise meaning *within* a given system, *across* systems they became very diffuse indeed. In fact, most psychologists warned against the indiscriminate use of *Ganzheit* by other psychologists, on account of the danger of the concept becoming totally empty. Nevertheless, they were of course convinced that they themselves did possess the correct approach to *Ganzheit*.

Second, *Ganzheit* talk was not restricted to psychology. When Burkamp (1929) set about to analyze the *Struktur der Ganzheiten*, he could adduce evidence from philosophy, biology, sociology, education, and economics; all these fields, and many more (for instance, geography!) were pervaded by *Ganzheit* doctrines. Nor was *Ganzheit* talk restricted to science. Its use was especially prevalent amongst movements aiming at life reform and among the youth.

Third, insight into the holistic nature of mental life was claimed to be a *specifically German achievement*. The 'forefathers' of holistic thinking were by no means sought where we would look for them today — in the polemics against the psychology of elements — but, as far as possible, back in the German past. Medieval mysticism and German idealism were the main

witnesses to an inherent tendency of the Germans to indulge in holistic thinking (cf. especially Krueger, 1932).

Fourth, *Ganzheit* discourse was often of a *rhetorical nature serving obvious political motivations*. For instance, at the end of an attempt at a systematic analysis of the *Ganzheit* concept Felix Krueger (1932) stated that at present the Germans were despoiled and mistreated everywhere, that they were bloodily persecuted, and that this had opened their eyes for piercing through the fog of Western and Eurasian phrases. The West would fall prey to chaos unless it were reformed from top to bottom, and the Germans were destined to co-operate in this 'new formation of the whole', on account of their special talent for apprehending the inner side of human being and the structured nature of life. Not every Weimar psychologist shared in this at once crude and pretentious rhetoric, but at least Krueger's pronouncements did not provoke the open protest of his colleagues.

Where did this rhetoric come from? To put it briefly, it was a simple continuation of the 'culture chauvinism' of the German professorial elite aimed at supporting the German effort in World War I, an orientation that had been shared by many important psychologists, including Wilhelm Wundt (Scheerer, 1989b). During World War I, the "subordination of parts to the whole" had also assumed a specifically social meaning where parts were individuals and the whole was a biologically conditioned social unit. Thus, when Krueger was indulging in *Ganzheit* talk and stressing the German propensity to holistic thinking, he was in effect conducting political propaganda aimed at the revision of the Versailles treaty and the destruction of the democratic system of the Weimar republic.

To sum up, within Weimar psychology as a whole the *Ganzheit* concept had the functions of an embodiment in Lindenfeld's sense. It served to hold together a fragmented and disunited discipline, it subserved communication between various systems within the discipline and across disciplinary boundaries (remember that polemics, too, is a form of communication), and it allowed at least the sizable majority of German psychologists to identify with the mainstream of antidemocratic thinking (Sontheimer, 1978) in the Weimar republic.

References

Boring, E. G. (1950). *A history of experimental psychology.* 2nd ed. New York: Appleton-Century-Crofts.

Burkamp, W. (1929). *Die Struktur der Ganzheiten.* Berlin: Junker & Dünnhaupt.

Henning, H. (1931). *Psychologie der Gegenwart.* 2nd ed. Leipzig: Kröner.

Hildebrandt, H. (1989). Die wissenschaftsgeschichtlichen Ursprünge der Krise der Psychologie in der Weimarar Republik. In A. Schorr & H. Wehner (Eds.), *Psychologiegeschichte heute*. In press.

Jaroschewski, M. (1975). *Psychologie im 20. Jahrhundert* (Russian original, 2nd ed. (1974). Berlin, DDR: Volk & Wissen.

Kelly, D. (1987). Horizons of intellectual history: Retrospect, circumspect, prospect. *Journal of the History of Ideas, 48*, 143-169.

Klemm, O. (1911). *Geschichte der Psychologie*. Leipzig, Berlin: Teuber.

Krieger, L. (1973). The autonomy of intellectual history. *Journal of the History of Ideas, 34*, 499-516.

Krueger, F. (1932). Das Problem der Ganzheit. Quoted after: F. Krueger, *Zur Philosophie und Psychologie der Ganzheit*, E. Heuss (Ed.). Berlin, Göttingen, Heidelberg: Springer 1953.

Lindenfeld, D. F. (1988). On systems and embodiments as categories for intellectual history. *History and Theory, 27*, 30-50.

Lovejoy, A. O. (1936). *The great chain of being: A study of the history of an idea*. Cambridge, MA: Harvard University Press.

Mandelbaum, M. (1965). History of ideas, intellectual history, and the history of philosophy. *History and Theory, Beiheft 5*, 33-66.

Messer, A. (1927). *Einführung in die Psychologie und die psychologischen Richtungen der Gegenwart*. Leipzig: Meiner.

Müller-Freienfels, R. (1929). *Hauptrichtungen der Psychologie*. Leipzig: Quelle & Meyer.

Richter, M. (1987). *Begriffsgeschichte* and the history of ideas. *Journal of the History of Ideas, 48*, 247-263.

Ringer, F. K. (1969). *The decline of the German mandarins*. Cambridge, MA: Harvard University Press.

Saupe, E. (1928). *Einführung in die neuere Psychologie*, E. Saupe (Ed.), 2nd ed. Osterwieck/Harz: Zickfeldt.

Scheerer, E. (1985). Organische Weltanschauung und Ganzheitspsychologie. In C.F. Graumann (Ed.), *Psychologie im Nationalsozialismus* (pp. 15-53). Berlin, Heidelberg, New York, Tokyo: Springer.

Scheerer, E. (1989a). Psychologie. In K. Gründer, et al. (Eds.), Historisches Wörterbuch der Philosophie (pp. 1599-1653). Bd. 7. Basel, Stuttgart: Schwabe.

Scheerer, E. (1989b). Fighters of the word: The ideology of German psychologists in World War I and its effects on postwar German psychology. Paper presented at the Cheiron conference, Kingston, Ontario, June 1989.

Sontheimer, K. (1987). *Antidemokratisches Denken in der Weimarer Republik*. Müchen: DTV.

THE ORIGINS AND SIGNIFICANCE OF CLARK L. HULL'S THEORY OF VALUE

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SUMMARY: Hull's theory of value is interpreted as an example of the operation of Habermas' system of purposive-rational action. Habermas claims that the system has deeply penetrated the personal and private domains in advanced industrial societies. The author shows that, from the 1890s to the 1920s, American social scientists such as John Dewey created versions of positivism and instrumentalism that precisely fitted the demand that the social sciences should contribute to the material well-being and efficiency of American society. Clark L. Hull is a good test case for the scope and discriminating power of Habermas' theory. By the time that Hull began formulating his theory American social science had become mature. Nevertheless, it continued to operate within the constraints of the purposive-rational system. An aspect of the operation of the system is the provision of a seemingly objective justification for its practices. Hull attempted to provide such a justification and to extend his theory to every aspect of human life, including the sphere of values. The author goes on to assert that Clark L. Hull's theory of value was not deduced from his positivist theory of science but that his theory of science and his moral theory sprang from the same source. Such an analysis suggests that neo-behaviorist theories in general should be assessed not in terms of some supposedly objective, universal standards of truth and rationality, but in terms of their social function at the time of their creation.

Introduction

I wish to demonstrate that Habermas' (1971, 1984, 1987) concept of purposive-rational action can be applied to Clark L. Hull's neo-behaviorist theory (Hull, 1943, 1952). In order to give my paper focus I will concentrate on Hull's theory of value (Hull, 1944). By so doing, I hope to bring home the point that the issue of value lies at the heart of behaviorist theory. The great behaviorists (Watson, Hull, & Skinner) wanted their theories to encompass every aspect of human life. In doing so, they attempted to restructure the cognitive aspects of human knowledge — especially, what constitutes a theory and what it means to be a theorist. It is less apparent, I think, that they also wished to restructure the human value system. Techniques of control devised and tested in the animal laboratory were not just to be used to enhance human life. Those same techniques of control were to be used to change the way people conceived of the goals and purposes of human life and, above all, to

change conceptions of valued objects and processes, such as the person and personal relationships.

An enterprise of such bold scope needs to be evaluated from the perspective of an equally wide-ranging theoretical system. I would like to suggest that Habermas' theory allows us to understand how behaviorist theories like Hull's arose within a particular social milieu and how they were both shaped by the exigencies of that milieu and served particular social ends. After very briefly outlining Habermas' system of purposive-rational action I will show (equally briefly) that this scheme is well-nigh perfectly exemplified in American society from about 1890 to the end of the 1920's, especially in the ideals of the Progressive movement. I will then show that Hull's neo-behaviorist theory can be treated as an extension of the values and beliefs of the Progressives to the abstract task of creating a theory of behavior. I will then deal briefly with Hull's theory of value, suggesting that it should be treated not as a derivation from his general theory to the sphere of value but as the foundation of his entire theoretical enterprise.

A Brief Summary of Habermas' Theory

Habermas maintains that modern science acquired its status during a period when it became possible for mankind to dominate nature by technological means. The possibilities for domination in the natural realm were progressively extended to the human. Rationality, instead of becoming a critique of traditional practices, became, simultaneously and progressively, a justification for a new set of practices that deeply invaded the personal and private domains. Eventually, a technocratic elite arose; its function was to set up ideals of social efficiency, and the attainment of the maximum material gain. The elite could achieve its ends only by co-opting all members of any given society into the enterprise.

Habermas claims that the system of purposive-rational action represents a clean break with historical tradition. In doing so, it protects itself against attack because, unlike other systems of domination, it is not grounded in a scheme that holds out the promise of just interactions. Habermas comments: "For it does not, in the manner of ideology, express a projection of the 'good life' (which even if not identifiable with a bad reality, can at least be brought into virtually satisfactory accord with it)." (1971, p. 111) Unlike previous ideologies, the system of purposive-rational action does not provide people with a means of criticism from within.

The system of purposive-rational action also ensures its own domination via the blurring of the overt signs of class membership that is so characteristic of advanced technological societies. Mass repression on the basis of overt class distinctions has never been an effective means of control because the

difference in power and status between oppressed and oppressor is all too obvious. In a society successfully exemplifying the ideals of purposive-rational action, however, the seeming absence of class barriers merely serves to make the oppressive system more subtle and harder to combat. At the same time, people are co-opted by the system via networks of private rewards. The basis of those rewards is dissociated from the political system, appearing to be the consequence of self-denial, prudence, hard work, intelligence and the other characteristic Western middle-class virtues and traits. That disconnection between the social and political basis for power and status and the means of attaining power and status is highly characteristic of American society.

Early U.S. Social Science

There is a great deal of work demonstrating that the origins of U.S. social science lay in the social reform movements that arose in America from the end of the nineteenth century onwards (Baritz, 1960; Burnham, 1972, 1977; Cravens, 1978; Haskell, 1977; Hofstadter, 1955a, 1955b; White, 1947). It is also clear that the first American social scientists were either reform-minded themselves or worked in close conjunction with reformers. In order to understand the unique characteristics of U.S. social science it is crucial to take note of the ideological features of U.S. Progressivism. First, the Progressives came from a particular stratum of the white anglo-saxon protestant middle class. They tended to have strong religious beliefs or to be the children of parents with strong religious beliefs. All felt called upon to serve their country. All were concerned about America's most pressing needs, which they saw as the problems resulting from rapid urbanization (slums, disease, alcoholism, prostitution, and so on) and the problems associated with mass immigration (the need to induct people of diverse ethnic origins into the American way of life). Given their social origins and their moral style they attempted to impose solutions on those they wished to help rather than attempting to collaborate with the urban masses and generate policies from below. Nor, unlike their socialist counterparts in Europe, did these reformers seriously challenge the right of wealthy industrialists to impose their agenda on America. Although their Puritan consciences were offended by the robber barons' harsh greed for power, the reformers attempted merely to temper and redirect that greed. The reformers and the social scientists who arose from the reform tradition devoted almost all their efforts to discovering means of socializing the urban working class into the mores of the industrialized state. Moreover, in doing so the reformers made no appeal to tradition, community, or to a sense of common purpose. Instead, the solutions they proposed were strictly technological. People were to be moulded to fit the needs of a society exclusively devoted to the attainment of material ends.

With respect to U.S. social science, the consequence was the creation of unique sets of views regarding two crucial concepts — the person and causality (Haskell, 1977). The person was effectively dissolved and became the physical nexus upon which a complex web of social forces impinged. Instead of being seen as an autonomous agent, the person became merely an entity that mirrored the social forces in which he or she was enmeshed. Just as they degraded and devalued the person American social scientists gave no credence to the notion of causal force. Causation was reduced to the study of functional relationships. They believed that the role of the social scientist was to predict and control behavior, and that perfect prediction and control would generate complete understanding. As John B. Watson put it so pungently in his ground-breaking 1913 paper: "The *theoretical* control of psychology is the prediction and control of behavior." (1913, p. 158; emphasis added).

Essentially, I wish to maintain that Hull was an archetypal U.S. social scientist. His theory is not to be understood in some formal or universal sense but by assessing how completely it fits the pattern that controlled the generation of U.S. social theory. So, the equation that is usually made between Hull's theory and logical positivism is a view imposed retrospectively. Instead, Hull's theory should be construed as a version of the forms of positivism unique to the U.S.

Hull as a Theorist

Superficially, Hull's views on theory were the same as the logical positivists, although Smith (1986) has shown that Hull developed his own version of positivism. Just like the logical positivists he believed in the unity of science and that physics was the master science. That meant that true knowledge had to be generated and validated in a manner analogous to the way in which physicists generated and validated knowledge. Once psychologists were pursuing a truly scientific enterprise, Hull believed, disputes between the adherents of competing theories would cease (Hull, 1935, p. 492). Hull also resembled the logical positivists in that he wished to put an end to fruitless and unresolvable controversy. Furthermore, both Hull and the logical positivists believed that the source of much controversy outside physical science lay in arguments about metaphysical issues (Hull, 1930, p. 252; 1943, pp. 23-24). Like the logical positivists again, Hull believed that there was a "symmetry" between explanation and prediction. Hull's interpretation of the symmetry was to say that the conflicting claims of theories purporting to operate in the same domains could be resolved if it could be demonstrated that one theory made more and more precise predictions than any of its competitors.

Even if Hull was a positivist, he was certainly not a logical positivist. Hull tried to derive his postulates directly from experimental data by curve fitting. The logical positivists, who were using already well-established

theories in the physical sciences as their models, would have found that approach very odd. Hull's approach to causation was also simple-minded. He believed that the laws of molar behavior should be uniform and exact and was not prepared to grant that they might be probabilistic.

The logical positivists believed that all metaphysical discourse was nonsense. Although Hull fulminated against metaphysics it is clear that his quarrel was not with metaphysics as such but with what he construed as 'bad' metaphysics — the idealism of Jeans and Eddington and the vitalism of Whitehead and Driesch (Smith, 1986). Others who have written on Hull have asserted that his theory was, at bottom, metaphysical (Carini, 1968; Mills, 1978a, 1978b; Peters & Tajfel, 1958). Smith (1986) believes that Hull resembled Karl Popper in that both believed that the ultimate epistemological distinction was between science and non-science.

Two major differences between Hull and the logical positivists were Hull's obsession with mechanism and with quantification. Smith (1986) shows at length how Hull's fascination for mechanical devices was a part of his thinking before he was a behaviorist and how that fascination exerted a powerful control over his theory. The types of mechanism that interested Hull were those in which a hierarchical control system determines the operation of all the parts of the device. The shift in Hull's interest from particular mechanisms that controlled specific aspects of behavior to abstract devices that could control the complete behavior of living animals was not paralleled by a willingness to consider non-hierarchical types of control. Hull's obsession with quantification has been extensively discussed (Koch, 1954; Hilgard and Bower, 1966; Mills, 1988; Smith, 1986).

Hull's Conception of the Role of a Theorist

Hull's conception of the role of a theorist was curiously unique. First, Hull (1935, p. 496) referred to theorists as "sponsors", strongly suggesting that he saw a theorist as the spokesman or publicist for views that he had not necessarily generated himself. A further indication of Hull's attitude to theory is to be found in a passage from his diary, where he wrote:

> ... people apparently are impressed by the mere external appearance of rigor. This is a factor of considerable importance in the matter of propaganda. I shall certainly heed the evident moral when I write up the system as a whole. (Hull, 1962, p. 858)

That passage suggests that, in some respects at least, Hull distanced himself from his own theory, treating it merely as a means of advancing his own status within the psychological community.

When working on his theory, Hull acted very much like the president of a corporation, who sets the overall goals of the enterprise but who delegates

much of the decision making to his subordinates. Within Hull's group the atmosphere was, up to a point, extremely open and democratic. Hull welcomed criticism of specific theoretical formulations and, while working on *Principles of Behavior*, frequently modified various portions to meet those criticisms. But the overall goals of the enterprise remained strictly in Hull's hands.[1]

A striking example of Hull's delegation appears in the correspondence between Hull and Kenneth Spence (Hull, 1939). As soon as he began full-time work on his theory Hull wrote to Spence suggesting that Edwin Guthrie's principle of reinforcement could be incorporated into the theory. Given the magnitude of the differences between Hull's (1943) and Guthrie's theory of reinforcement the suggestion is very startling.[2] Despite those differences, it is as if Hull were willing to delegate to Guthrie the responsibility of creating the principle of reinforcement needed for his own theory. Hull carried the process of delegation further by suggesting that Spence should provide him with the basis for evaluating Guthrie by writing a review article and that Spence's review should be based on a five-page summary of Guthrie's theory provided by Fred Sheffield.

Although Spence did not write the proposed review and although Hull ultimately revised his own principle of reinforcement the prolonged flirtation with Guthrie suggests that Hull's views on the role of a theorist were highly unconventional. He conceived of his role in a strictly utilitarian manner. The role of the axioms and postulates was strictly pragmatic — to produce quan-

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[1] Smith (1986) comments as follows on Hull's managerial style of theory construction (an approach that had the full support of Mark May, the Director of Yale's Institute of Human Relations): *"May and Hull worked closely together to implement their vision of integrated research at the IHR. Revealed as a division of labor, their notion of scientific cooperation derived from and reflected not only the hierarchical structure of scientific theory itself, but also the method of testing it. If theories were a type of mechanism in Hull's view, so too were the social groups that devised and tested them. Each scientist was like a gear in the machine of science, the output of which was the conceptual machine known as scientific theory. Needless to say, Hull's status in the hierarchy of research suited his ambitions well. Driven by fears for his health, Hull rushed toward a comprehensive theory of psychology, leaving no time for personal involvement in experimentation. The best hope of fulfilling his ambitions lay in his supervision of the most direct possible assault on the integration of psychological theory."* (p.182). For an excellent discussion of Hull's role at the IHR see Morawski (1986).

[2] In the same letter in which he told Spence that he had started work on his theory of behavior Hull wrote: *"Guthrie's general approach (with a very great deal of modification and amplification) would make a really world-beating basis for a system, since the number of postulates would be greatly reduced. What I mean is that on those assumptions I think I could deduce as theorems quite a number of my present postulates, including that of the gradient of reinforcement."* (Hull, 1939). Given the difference between Hull's (1943) and Guthrie's theory the suggestion is very surprising. Guthrie relegated drive to the periphery of his theory, treated repetition in a highly idiosyncratic way, and gave reinforcers an indirect role (Hilgard, 1948).

tifiable predictions of behavior. The theoretical provenance of the postulates did not interest him in the least.[3]

Hull's Theory of Value

Hull (1944) wanted his theory of value to be all-embracing and to cover all aspects of human life. No general theory could hope to be complete unless it addressed issues such as beliefs about the ultimate purpose of human life or the intrinsic worth of goals or ideals, whether of individuals or groups. Hull (1944) did not shrink from the task. He said that there was no agreed definition of value but wrote:

> The chief moral to be drawn from this point is that *definitions are capable of progressive empirical rectification and validation very much as are postulates.* The definition of value which, when incorporated into the relevant postulates of a natural-science theory, mediates a very large number of theorems, each corresponding with precision to the observed outcomes of specified antecedent conditions, is in so far probably sound, valid, or true. By this it is meant that the postulates in question will have a greater probability of mediating all possible remaining theorems relevant to the objectively observable and measurable world than would a set of postulates and definitions which had yielded a smaller number of verified theorems. (Hull, 1944, pp. 127-128; emphasis in original)

Here, Hull was applying the same criterion as he would to any theoretical domain — namely, that a theory is to be assessed solely in terms of its predictive success. Moreover, as in the case of all his other theoretical constructs, he defined values instrumentally. Hull went on to equate value with strivings to reduce needs. The more readily and effectively an object reduces a need, the more it will be valued. In humans, many objects are valued not because they reduce need directly but because they are associated with or lead to need reduction. The chief example of such secondary reinforcers is money, according to Hull.

A particular value that Hull (1944) dealt with himself was truth. He defined truth as the accurate prediction of need reduction. He was, therefore, committed to saying that he would have solved some particular problem once he had reduced his need (anxiety, distress, puzzlement, etc.) to zero. But that is equivalent to saying that the belief that one has arrived at a true solution

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[3] The following passage is a good example of Hull's views on theory: *"The history of scientific practice so far shows that, in the main, the credentials of scientific postulates have consisted in what the postulates can do, rather than in some metaphysical quibble about where they came from. If a set of postulates is really bad it will sooner or later get its user into trouble with experimental results. On the other hand, no matter how bad it looks at first, if a set of postulates consistently yields valid deductions of laboratory results, it must be good. In a word, complete laissez-faire policy should obtain in regard to postulates. Let the psychological theorist begin with neurological postulates, or functional postulates, or organismic postulates, or hormonic postulates, or mechanistic postulates, or dynamic postulates of dialectical materialism, and no questions should be asked about his beginning save those of consistency and the principle of parsimony.* (Hull, 1935, p. 511; emphasis in original).

makes the solution true. Admittedly, it is possible to derive a more sophisticated definition of truth from the theory by saying that, in any given area of knowledge, the theory making the largest number of accurate predictions is the most likely to be true. Even so, we are still operating at a strictly pragmatic level. Even if a theory made perfect predictions we could still ask questions about the basis for those predictions.

I wish to conclude this section by alluding to three issues that I cannot develop fully, given space constraints. First, U.S. social scientists tended to devalue the concept of the person. I believe that there is a symmetry between Hull's dispersal of functions within his associates and the atomization of the person that we see in his behaviorism. It follows from Hull's theory that the self is simply the physical locus of a set of reinforcements. The self, then, is seen as developing passively. There is no notion of inherent, intrinsic control. Indeed, Hull dismissed all such principles as examples of idealist principles or vitalism.

Second, a value that Hull did not deal with explicitly was elegance or beauty. For a theorist such a value is important, since it is commonplace for grand theorists to believe that the aesthetic qualities that they perceive in the world should be reflected in their theories. Elegance, however, had no place in Hull's starkly utilitarian scheme of things.

Finally, another value that should play a role for a psychological theorist is creativity. Again, Hull's position was desolatingly arid. Just as he wanted to quantify his theoretical variables so he wanted to quantify creativity. He who achieves more than the next man is automatically more creative. If that is our stance, then we do not have to make judgments about the intrinsic worth of what has been achieved.

In general, what one discovers in the archival material available is that Hull had a strangely combative attitude towards his chosen subject matter. He would frequently tell his closest confidant Kenneth W. Spence that he was going to "take a problem to the mat". In part this need to throw down, pin, and conquer a problem arose from his fear that he had but a short time in which to reach his goals. But one also has a sense that conquest and subjugation comprised all that Hull wished to achieve. That is, one has no sense that he loved his subject matter for its own sake. One certainly has no evidence that his work brought him joy or a sense of abiding peace. A grim-jawed, grinding conquest of problem after problem, until the appointed number had yielded to attack, that is all.

Conclusion

In conclusion, I want to make a general point about behaviorism. Behaviorist theories, such as Hull's and Skinner's, represent an attempt to create a comprehensively technocratic picture of human life. Their theories claim to

give causal accounts of every possible form of human activity. At the same time, they also provide justification for limiting themselves to certain types of causal explanation. The theories and their context of justification form what Habermas (1984, 1987) calls a life-world. To live within the same life-world as someone else is to share more or less different background convictions that are not subjected to analysis or discussion and which form the foundation for those beliefs and practices that are openly discussed.

A central feature of the behaviorist life-world is that human beliefs and actions are to be evaluated solely in functional terms — that is, in terms of their usefulness in reaching and maintaining goals concerned with material well-being. Included in beliefs and actions are theoretical statements and the steps required to create and justify theories. As I have shown, Hull had a strictly functional and utilitarian approach to theory. The same can certainly be said for Skinner. Hull's and Skinner's instrumentalism commits them to an instrumental approach to questions of value. Goals, purposes, and meanings are to be assessed solely in terms of their practical utility. That belief, supposedly, is the direct outcome of empirical studies, especially studies in the animal laboratory, that provide the justification for fully generalizable laws of behavior.

The basis for those laws becomes suspect when one realizes that the animal studies themselves are 'perfused with value'. The animal of the Hullian or Skinnerian laboratory was a Benthamite beast that, so the behaviorists believed, could be forced to achieve goals set for it by the experimenter. Those goals were derived from the value system from which the theory was created in the first place. So, we should assess behaviorist theories in terms of a theory of value, not by assessing the extent to which the laboratory work of the behaviorists supported their theories.

References

Baritz, L. (1960). *The Servants of Power: A History of Social Science in American Industry*. Middletown, CT: Wesleyan University Press.

Burnham, J. C. (1972). The new psychology: From narcissism to social control. In J. Braeman, R. H. Bremner, & D. Brody (Eds.), *Change and continuity in 20th century America: The 1920s* (pp. 351-398). Columbus, OH: Ohio State University Press.

Burnham, J. C. (1977). Essay. In J. D. Buenker, J. Burnham, & R. M. Crunden (Eds.), *Progressivism* (pp. 3-29). Cambridge, MA: Schenkman Books.

Carini, L. (1968). The Aristotelian basis of Hull's behavior theory. *Journal of the History of the Behavioral Sciences, 4*, 109-118.

Cravens H. (1978). *The triumph of evolution: American scientists and the heredity-environment controversy*. Philadelphia, PA: University of Pennsylvania Press.

Habermas, J. (1971). Technology and science as "ideology". In J. Habermas (Ed.), *Toward a rational society: Student protest, science, and politics,* (pp. 81-122). (J. J. Shapiro, Trans.) (Original work published in 1968 and 1969). London: Heinemann.

Habermas, J. (1984). *The theory of communicative action. Vol. 1. Reason and the rationalization of society,* (T. McCarthy, Trans.) (Original work published in 1981). Boston: Beacon Press.

Habermas, J. (1987). *The theory of communicative action. Vol. 2. Lifeworld and system: A critique of functionalist reason,* (T. McCarthy, Trans.) (Original work published in 1985). Boston: Beacon Press.

Haskell, T. H. (1977). *Emergence of a professional social science: The American Social Science Association and the nineteenth century crisis of authority.* Urbana, IL: University of Illinois Press.

Hilgard, E. R. (1948). *Theories of learning.* New York: Appleton-Century-Crofts.

Hilgard, E. R., & Bower, G. H. (1966). *Theories of learning.* (3rd ed.). New York: Appleton-Century-Crofts.

Hofstadter, R. (1955a). *The age of reform: From Bryan to F. D. R.* New York: Knopf.

Hofstadter, R. (1955b). *Social Darwinism in American thought* (rev. ed.). New York: Braziller.

Hull, C. L. (1935). The conflicting psychologies of learning — A way out. *Psychological Review, 42,* 491-516.

Hull, C. L. (1939). Letter to Kenneth W. Spence. Archives of the History of American Psychology, Akron, OH.

Hull, C. L. (1943). *Principles of behavior: An introduction to behavior theory.* New York: Appleton-Century-Crofts.

Hull, C. L. (1944). Value, valuation, and natural-science methodology. *Philosophy of Science, 11,* 125-141.

Hull, C. L. (1952). *A behavior system.* New Haven, CT: Yale University Press.

Hull, C. L. (1962). Psychology of a scientist: IV. Hull's idea books. *Perceptual and Motor Skills, 15,* 807-882.

Koch, S. (1954). Clark L. Hull. In W. K. Estes, et al. (Eds.), *Modern learning theory* (pp. 1-154). New York: Appleton-Century-Crofts.

Mills, J. A. (1978a). Hull's theory of learning as a philosophical system: I. An outline of the theory. *Canadian Psychological Review, 19,* 27-40.

Mills, J. A. (1978b). Hull's theory of learning: II. A criticism of the theory and its relationship to the history of psychological thought. *Canadian Psychological Review, 19,* 116-127.

Mills, J. A. (1988). The genesis of Hull's *Principles of Behavior. Journal of the History of the Behavioral Sciences, 24,* 392-401.

Morawski, J. (1986). Organizing knowledge and behavior at Yale's Institute of Human Relations. *Isis, 77,* 219-242.

Peters, R. S., & Tajfel, H. (1958). Hobbes and Hull — metaphysicians of behavior. *British Journal for the Philosophy of Science, 8,* 30-44.

Smith, L. D. (1986). *Behaviorism and logical positivism: A reassessment of the alliance.* Stanford, CA: Stanford University Press.

Watson, J. B. (1913). Psychology as the behaviorist views it. *Psychological Review, 20,* 158-177.

White, M. G. (1947). *Social thought in America: The revolt against formalism.* New York: Viking. (Reprinted by Oxford University Press, 1976, with a new foreword by the author)

Author Note

INTERACTIONIST THEORY AND DISCIPLINARY INTERACTIONS: PSYCHOLOGY, SOCIOLOGY AND SOCIAL PSYCHOLOGY IN FRANCE

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SUMMARY: After a discussion of certain current difficulties in describing an interactionist social psychological perspective, we examine turn-of-the-century French social science. Gabriel Tarde had created several social psychological theoretical formulations, with his last being an interactive 'inter-psychology' which dealt with a range of analyses of interpersonal influence processes, public opinion, communication, economic exchanges, and so forth. In examining why this theory never had widespread acceptance in France, a multilevel analysis is suggested: logic of science for conceptual or theoretical evolution; social psychology of science for the social processes among idea submitters and idea accepters; and sociology of science for community-wide, institutional support and long-term promulgation of the ideas. The conceptual difficulties faced by social psychological theories, in general, and by interactionist theories, in particular, are discussed in relation to psychological and sociological formulations. An analysis is made of the lack of institutional support and the reactions of the evolving disciplines of sociology and psychology towards this interactionist social psychological theory.

Some Current Difficulties About Interactionist Social Psychology

Dissecting the theoretical content of interactionist theories in social psychology, whether at the turn of the century, or in modern times, presents great difficulty for social psychological researchers seeking new language to express what occurs between individuals or between individuals and collectivities. It is far too easy to fall back into, for example, behaviorist categories which strip interaction of its dynamic, contextual, systemic, and historical (developmental) qualities. Rather, language must be sought to stress the mutuality of the communications and influences occurring *within* interpersonal and small group contexts. I will argue that the focus of what would be a truly autonomous social psychology — as distinguished from a psychological social psychology tacked onto psychology or a sociological social psychology appended to sociology — lies in an interactive conceptualizing of the bridging processes or mechanisms that bind, link or interrelate persons with other persons or groups. Such processes develop *between* persons, and might

include the dialectical give-and-take of conversation, development and persistence of family ties, peer-group influence, leadership and power relationships, various socialization processes (including formation of various roles, selves and identities), a deepening friendship, an escalating violent feud, and so forth. I suggest that an autonomous social psychology might choose to focus specifically on the interaction occurring in the relational space *between* people, rather than relying, in psychological fashion, on a strong anchorage of social psychological phenomena back within the individual, or, passing over to the sociological side and becoming enmeshed in social structures.

Other participants at this Conference on Theoretical Psychology seem also to be wrestling with similar concerns about 'interaction': Apfelbaum (1979) talks of power as a relationship, occurring between individuals and groups; Smedslund (1988) approaches three main types of interaction (cooperation, consultation and coercion) from a structured psychologics, rather than directly from the psychological level; M. Gergen (this volume) talks about 'relational units'; and Shotter (1984) also tackles the complex task of repainting the individual/social dialectic (especially for 'selfhood'). K. Gergen faced parallel difficulties in talking about "the self as abstracted relationship," when he suggested:

> ... that relationship precedes the ontology of the individual; prior to relationships there is little sense in which there is a concept of the individual — the individual self.... Can we develop a language of understanding in which there are not powerful, helpful, intelligent or depressed selves, for example, but in which these characterizations are derivative from more essential forms of relationship? (K. Gergen, 1984, pp. 15-16).

I believe that questions concerning the self as defined in relationship, or identity formed as part of a Baldwinian 'social dialectic', or a focussing on interactive processes between individuals, *per se*, are all parts of a more autonomous social psychological perspective, not the 'add-on-component' subdisciplines which evolved within psychology and sociology. If we think of interactive processes as forming the social bridge between an individual and one or more other individuals, can we not conceptually center our analyses on just that bridge itself? Can we not examine the two-way flow of cars or communicative and influence processes over it, without having our attention always diverted to the anchoring points at the psychological or sociological ends? Can the individual-social interactive bridge not be studied as a connective, dynamic, developing relational system, and should this not be the central task of social psychology?

Historical Flashback on the Problems of Interactionism

Such modern concerns have been mirrored in the past, and we shall briefly examine the thwarted development of one form of interactionist social psychology. Much of the current interest in interactionist perspectives in

social psychology, sociology, and to a lesser degree in psychology, is generally traceable to the development of the symbolic interactionist perspective, and more particularly to the writings of George Herbert Mead, many of which were posthumously published (e.g., Mead, 1934). Less visible were the efforts made in France by Gabriel Tarde at developing social psychological frameworks from 1880 until his death in 1904. His last theoretical formulation of an 'inter-psychology' involved a broad range of analyses of interpersonal influence processes, communication, public opinion, economic exchange, and so forth, and it is this theory of inter-mental processes which appears extremely 'interactionist' to a presentist eye. Of course, prior to Tarde's interactionist formulation, a number of alternative conceptions of 'social psychology', and earlier 'proto-social psychologies' had appeared on the French scene (Apfelbaum, 1981, 1986, 1988; Apfelbaum & Lubek, 1983; Apfelbaum & McGuire, 1986; Lubek, 1981; Lubek & Apfelbaum, 1988; van Ginneken, 1989). Each attempted to describe systematically the processes and relations that occur between, or link, the individual to the 'social' or collective. Unfortunately all these exercises were ephemeral.

The purpose of this paper is not to create another "origin myth" (Samelson, 1974) about the French roots of interactionist social psychology, but rather simply to examine historically the fate of an 'interesting' theoretical formulation and to try to uncover some of the reasons for the 'subterranean' development (Apfelbaum, 1981) of social psychological idea systems in France. Do such ideas 'get lost', as some historians of psychology might claim, because they have perhaps emerged precociously within a context of ideas or 'Zeitgeist' which cannot yet sustain them? Or has it been a traditional theoretical 'War of the Words' where the existence of competing paradigmatic formulations blocked their progress and prevented them from gaining institutional backing for their dissemination? These ideas may then have faded for lack of an extended scientific forum for reflection, discussion, debate, correction, testing, and so forth.

Concepts, Persons and Institutions: A Multilevel Approach to Theory Development

In order to assess how a scientific discipline or set of ideas becomes visible, accepted as legitimate, and/or anchored, I would argue for a multilevel analysis. At the level of the logic of science, for example, the analysis may focus on conceptual/theoretical evolution and the creation of research models or 'paradigm/exemplars'. At the level of social psychology of science, we may focus more on how interpersonal and small group interactions affect scientific outcomes in, for example, the communication/influence processes observable between idea 'proposers' (e.g., paper or grant submitters, academic job applicants, graduate students) who 'negotiate' with idea 'accepters'. At the broader level of sociology of science, we might examine more closely the

'paradigm/community's' mechanisms for promoting and disseminating ideas which are legitimated as 'scientific', and for providing institutional support to maintain such an intellectual enterprise's productivity and longevity, or to thwart, if necessary, 'false' science (see Figure 1).

I would suggest that a conceptual framework or paradigm/exemplar such as Tarde's "inter-psychology" (or perhaps a more ambitious conceptual project of delineating an autonomous discipline of social psychology) cannot assure itself of any sort of continuity or existence *per se*, if consideration is given solely to the criteria operative at a particular moment within the logic of science. Rather, the fate of any intellectual formulation will also depend on factors at the level of social psychology of science, where, for example, a series of interactions involving asymmetric power relations may lead either to research promotion or to gatekeeping. Finally, at the level of sociology of science it will be necessary to track the degree of institutional support from the 'paradigm/community' in order for there to be an expansion of the ideas and the correlative research enterprise (Lubek, 1980, 1986; Lubek & Apfelbaum, 1979, 1987). In the analysis of the fate of inter-psychology as a Fench social psychological formulation, the *theoretical content* of the evolving idea system will only be briefly sketched (see, e.g., Clark, 1969; Lubek, 1981). Elsewhere, I have detailed Tarde's relations with editors, hiring committees, social psychologists, and so forth, and so will make little mention here of the level of "social psychology of science" (Lubek, 1981, 1984). Rather, I shall put extra emphasis on the more macroscopic 'sociology of science' level in this paper, examining the institutional and disciplinary supports (or lack thereof) for the 'inter-psychology' theoretical system.

Social Psychology and Inter-psychology: Conceptual Difficulties

Chroniclers of social psychology often have trouble ontologically locating its conceptual center, and it is difficult to find authors who convincingly argue that social psychology is an autonomous research field, either in its historical evolution or current status. Karpf (1932, 1952), followed by many textbook writers, offered the hypothesis of a "two discipline" origin and development and traces the (often independent) psychological and sociological roots. In France, social psychological analyses describing how the individual and the social context mutually affected each other could be found at the end of the 19th Century, but as far as academia was concerned, these remained largely 'subterranean', with no autonomous disciplinary rooting taking place in academia until after World War II. And the epistemological fate of certain social psychological formulations — especially those of the *interactionist* variety which deal with processes occurring *between* individuals and groups — has been described as falling into an intellectual 'void' or 'no-person's land' between the idea systems of psychology and sociology

The approach of "SOCIAL PSYCHOLOGY OF SCIENCE" suggests that the contents, direction, and speed of diffusion of scientific knowledge can be affected by interpersonal and small group processes.

SOCIOLOGICAL, INSTITUTIONAL LEVEL: PARADIGM/COMMUNITY- Hierarchically organized scientific community sharing a problem area, supporting/diffusing research, e.g., members of a professional organization.

Research administrators
(Tenure, hiring, promotion committees, deans, laboratory directors)

→

Researchers/teachers seeking career stability

Established research supervisors deciding on apprentices' progress

→

Student researchers (e.g., PhD candidates seeking formal entrance to the scientific enterprise

Grant funding panel awarding research money

→

Researchers submitting grant applications for proposed research projects

Editors deciding on fates of manuscripts

→

Authors submitting reports of their scientific work

Quality control, gate-keeping decisions about additions to knowledge base by those (s)elected by the community

Largely one-way communications (e.g., authoritative decisions without appeal), involving asymmetric power relations between individuals

Submissions by those wishing and competing to add new knowledge (e.g., normal sciences, anomalies, false science, etc.)

MEDIATING SOCIAL PSYCHOLOGICAL PROCESSES

LOGIC OF SCIENCE LEVEL:

PARADIGM/EXEMPLAR - Cumulative research sharing the same "model" and research-strategy decisions; e.g., the articles in a journal; the rule-governed generation of scientific knowledge (theory, data).

Figure 1: A MULTI-LEVEL ANALYSIS OF SCIENTIFIC KNOWLEDGE-GENERATION: THE ROLE OF SOCIAL PSYCHOLOGICAL PROCESSES

(Apfelbaum, 1981, 1986; Lubek, 1981; Lubek & Apfelbaum, 1988). There is also some debate about whether social psychology is best described as being a bridge, rather than a crossroad intersection between psychology and sociology (Maisonneuve, 1973). In France, neither social psychology in general nor particular perspectives such as Tardean interactionism have had much success. As Maisonneuve (1973) noted, social psychology's progress as a social science discipline with specific content yet no autonomy was "disrupted for a long time by the imperialist and reductionist tendencies shown by psychologists and sociologists alike" (p. 14).

The fate of interpsychological theorizing must be viewed against this more general canvas of institutional and conceptual difficulties facing all social psychological perspectives. Thus, Lubek and Apfelbaum suggest:

> The development and dispersion of French social psychological ideas, on the one hand, and social psychologists on the other, have been affected both by the perilous epistemological position of its subject matter — located in the interactive space *between* the individual and the collective — and by the ... constant necessity of disciplinary tight-rope-walking between psychology and sociology (as these two disciplines strove for their own legitimacy and career opportunities as viable social sciences). (Lubek & Apfelbaum, 1988, p. 3).

During the critical period of the 1890s when social psychological ideas were being frequently formulated by social scientists, publicists, and social movement leaders, there was neither a dominant, clear, conceptual formulation, universally agreeable to the small evolving French community of social scientists, nor was there any sort of disciplinary foundation or support for such ideas. Tarde in the 1890s began evolving his interpsychological ideas away from the over-reliance on psychologism which had characterized his earlier works. Thus, his *Laws of Imitation* (Tarde, 1890/1903) — originally conceived as the first of a two-volume social psychology — and subsequent works were harshly criticized by some sociologists as being aristocratic, individualistic, person-blame, and elitist. At a time when his thought was gradually evolving into a more interactionist conception of social life, Tarde nonetheless found himself being backed into a psychological corner, especially in his debates with Emile Durkheim and his disagreements with some of the *Année Sociologique* critics of his work. As the 19th Century drew to a close and the Dreyfus Affair unfolded, French intellectuals also found themselves polarized along political lines. Sociological formulations offered by Durkheimians seemed increasingly more in tune with the rising forms of collectivist thought which, until the outbreak of World War I, were daily gaining more adherents (solidarism, syndicalism, co-operatism, socialism, communism, collective anarchism, etc.), and in eclipse were the older individualist political philosophies, with which certain psychological theories (including Tarde's imitation theory) resonated (Lubek, 1981).

But underlying these political, theoretical and conceptual disagreements, there were also highly contentious philosophical issues which lay just

beneath the surface of these attempts to create discourses to explain social behaviors. Strong differences about epistemology and philosophy of science arose among the competing 'social psychological' formulations, as well as between psychologists and sociologists debating their disciplinary boundaries. Benrubi (1933) notes that little synthesis was possible among the positions independently evolving from such a variety of traditions as positivism, idealism, neo-criticism, scholastic realism, materialism, and probabilism. Durkheim's ideas had in fact evolved from an empiricist, Comtean positivist tradition, and Tarde's from a stream of epistemological and critical idealism leading into a critique of science (following Renouvier, Claude Bernard, & Cournot). They were bound to have basic disagreements. In their last public debate (Worms, 1904), Tarde sought to differentiate his 'nominalism' from Durkheim's 'scholastic realism' leading to metaphysics. Elsewhere, as well, Durkheim chastised Tarde's antipositivist approach as a reaction against science. Tarde accepted accidents and probabilities, spontaneous individuals who invented, and interactive influence processes for communicating, imitating, opposing, and so forth, while Durkheim's analyses of social behavior may have looked more for the discovery of social facts and the imposition of Cartesian order and constraints by collectivities. At this time as well, some writers were not convinced whether it was to be science or history which would provide the more appropriate analytical framework for the study of real world social problems: much of Durkheim's writing emulated the evolving (social) scientific model, while Tarde's was more historical (Benrubi, 1933; Clark, 1968, 1969; Lubek, 1981; Milet, 1970). Durkehim's approach came to dominate, became firmly rooted in France, was carried on long after his death in 1917 by his *Année Sociologique* colleagues, students, and so forth, and remains to this day highly visible. (Lubek, 1984 found 4,255 citations to Durkheim's work in the *Social Sciences Citation Index*, 1966-1980, compared to 168 for Tarde).

Tarde's inter-mental psychology, which he later renamed inter-psychology, was an interactionist perspective which focused on what occurred *between* the individual and the other(s) — *those interpersonal, communication and influence processes that required the mutual influence or dialectic relationship of at least two persons*. Such a formulation was not compatible with how Durkheimian sociology was constructing 'social facts' and structures nor with how psychology was trying to construct the individual. Each of these disciplines seemed reluctant to welcome aboard with open arms a point of view that concerned itself with the realm of the interpersonal and, in attempting to bridge the individual with the collective, focused perhaps too much attention on the bridge itself.

Lack of Institutional Support for Inter-psychology and Social Psychology

Sociologist Durkheim and psychologist Binet, for example, each managed to find institutional bases at the university and they founded scientific journals for the dissemination of their ideas and the support of their evolving disciplines. The importance of these journals in providing a forum for the growth and dissemination of a set of ideas of a dominant figure and a gioup of like-minded assoicates can be seen by examining the two journals for the strong degree of 'presence', both direct and indirect, of Durkheim, Binet (and later Piéron). There was no equivalent centralized forum for the evolving interactionist social psychological position of Tarde. Tarde *did* give lectures on social psychology and inter-psychology at a number of *non-degree-awarding* institutions, the most prestigious of which was Collège de France (1900-1904). However, he and Théodule Ribot were unsuccessful in their attempt in 1899 to have the chair renamed from 'modern philosophy' to 'sociological psychology'. Tarde died in 1904, before he could complete publication of a planned interactionist social psychology, although portions of his ideas appearded in his *Psychologie Economique* (Tarde, 1902), in several articles, and in unpublished Collège de France lecture notes kept in the Tarde archives (Lubek, 1981).

Working outside the university system, Tarde had no students to carry on his work. A short-lived attempt to create a journal to carry on Tardean ideas, *La Revue de Psychologie Sociale* (1907-1908), was unsuccessful in presenting a coherent interactionist social psychological orientation for its authors, many of whom did not have a background in the social sciences (Lubek, 1981). However, psychologist Georges Dumas (1920) did make an independent attempt to revive Tarde's inter-psychology. Publication of his article (Dumas, 1920), which appears to have been written earlier, was delayed no doubt because of World War I. Dumas' ideas reappeared as a chapter (Dumas, 1924) in his influential two-volume *Traité de Psychologie* (Handbook of Psychology). Charles Blondel's (1928) work on collective psychology also has traces of Tarde's influence.

Overall, although social psychological concepts were being formulated from the 1890s onwards in France (and there was a developing English-language literature, some of which was translated into French), within the French academic context, there were only sporadic informal attempts to discuss social psychology between the two world wars, compared to its slow steady development in North America at this time (Lubek & Apfelbaum, 1989). Generally, one can say that the ideas about an interactionist inter-psychology (and, for that matter, any sort of social psychology) were not being disseminated in the French university classroom, perhaps simply due to the absence of its formal place in the curriculum until after World War II, when chairs were created in "psychologie de la vie sociale" at the Universities of

Paris, Strasbourg and Bordeaux. The chair created at the Sorbonne, with a laboratory attached, was given to Daniel Lagache, a Normalien philosopher, a central figure in the psychoanalytic movement, and a contemporary of Jean-Paul Sartre, Paul Nizan, and Maurice Merleau-Ponty. Moving from the chair in Bordeaux, Jean Stoetzel took over Lagache's chair at the Sorbonne in 1955, which was then renamed 'Social Psychology' (Lubek & Apfelbaum, 1988). So after 60 years of dragging its heels and with a very tardy academic entry, by the time social psychology was finally introduced to a wider public, the interactionist inter-psychology had all but disappeared; many of the new social psychological ideas which were being taught and were the subject of research in the 1950s were imported directly from the United States.

The demise of inter-psychology may be partially due to the lack of development and reworking of the ideas by other social scientists. It faded from view in most social science publications. The eight volumes of the *Nouveau Traité de Psychologie*, edited by Dumas, appeared between 1931 and 1948, with the last volume interrupted by the Second World War. In volume 8, Dumas was supposed to supply the updated chapter on inter-psychology and sociologist G. Davy a chapter on 'sociology and social psychology'. Neither chapter appeared and Dumas died in 1946. The next handbook revision, the *Traité de Psychologie Experimentale*, edited by Paul Fraisse and Jean Piaget (1963-1966), contains a volume on social psychology and a chapter on the history of psychology, but in the 2,050 pages there is no reference to Tarde or inter-psychology (Lubek, 1981). In the post World War II period, various interpersonal and interactionist theories, along with other social psychological formulations, have been brought into social psychology from English language sources (e.g., symbolic interactionism). But the indigenous French interactionist theory of Tarde and Dumas found neither conceptual nor institutional support from psychologists and sociologists alike, did not give rise to an autonomous discipline of social psychology, nor was it exported to other countries.

When dealing with the interactionist "interpsychology" or other lost and/or forgotten theoretical formulations — those which were initially weak *per se*, those which were epistemologically difficult or misunderstood, those losing a theoretical confrontation, those marginalized because they challenged the paradigmatic *status quo*, or those lacking sufficient institutional backing — it is suggested that a multilevel analytical approach stressing conceptual, social, and institutional elements may be helpful in determining the reception and fate of a developing theory.

References

Apfelbaum, E. (1979). Relations of domination and movements for liberation: an analysis of power between groups. In W. G. Austin & S. Worchel (Eds.), *The social psychology of intergroup relations* (pp. 188-204). Belmont: Brooks/Cole.

Apfelbaum, E. (1981). Origines de la psychologie sociale en France: développements souterrains et discipline méconnue. *Revue française de Sociologie, 22,* 397-408.

Apfelbaum, E. (1986). Prolegomena for a history of social psychology: Some hypotheses concerning its emergence in the 20th century and its raison d'être. In K. Larsen (Ed.), *Dialectics and ideology in psychology* (pp. 3-13). Norwood, NJ: Ablex.

Apfelbaum, E. (1988). Les enjeux d'une histoire de la psychologie sociale. *Revue de Synthèse, 4,* 499-510.

Apfelbaum, E., & Lubek, I. (1983). Le point de vue critique des écrits psycho-sociologiques (1889-1905) de Augustin Hamon. In S. Bem, H. van Rappard, & W. van Hoorn (Eds.), *Studies in the history of psychology and the social sciences* (Vol. 1, pp. 31-60). Leiden: University of Leiden Press.

Apfelbaum, E., & McGuire, G. (1986). Models of suggestive influence and the disqualification of the social crowd. In C. Graumann & S. Moscovici (Eds.), *Changing conceptions of crowd mind and behaviour* (pp. 27-50). New York: Springer-Verlag.

Benrubi, J. (1933). *Les sources et les courants de la philosophie contemporaine en France* (2 vol.). Paris: Alcan.

Blondel, C. (1928). *Introduction à la psychologie collective.* Paris: A. Colin.

Clark, T. N. (1968). Tarde, Gabriel. In D. Sills (Ed.), *International encyclopedia of the social sciences* (Vol. 15, pp. 509-514). New York: MacMillan & Free Press.

Clark, T. N. (1969). *Gabriel Tarde on communication and social influence: Selected papers.* Chicago: University of Chicago Press.

Dumas, G. (1920). L'interpsychologie. *Journal de Psychologie normale et pathologique, 17,* 515-537.

Dumas, G. (1924). l'Interpsychologie. In G. Dumas (Ed.) *Traité de psychologie* (Tome 2, pp. 739-764). Paris: Alcan.

Dumas, G. (Ed.) (1931-1948). *Nouveau traité de psychologie* (8 Tomes). Paris: Presses Universitaires de France.

Fraisse, P., & Piaget, J. (Eds.) (1963-1966). *Traité de psychologie expérimentale* (9 Tomes). Paris: Presses Universitaires de France.

Gergen, K. J. (1984). Toward self as relationship. Paper presented at the International Conference on Self and Identity, Cardiff, Wales, July 9-13, 1984.

Ginneken, J. van (1989). *Crowds, psychology and politics: 1871-1899.* Doctoral Dissertation. Universiteit van Amsterdam.

Karpf, F. B. (1932). *American social psychology: Its origins, development, and European background.* Reprinted, New York: Russell & Russell, 1972.

Karpf, F. B. (1952). American social psychology - 1951. *American Journal of Sociology, 53,* 187-193.

Lubek, I. (1980). The psychological establishment: Pressures to preserve paradigms, publish rather than perish, win funds and influence students. In K. Larsen (Ed.), *Social psychology: Crisis or failure?* (pp. 129-157). Monmouth, Oregon: Institute for Theoretical History.

Lubek, I. (1981). Histoire des psychologies sociales perdues: le cas de Gabriel Tarde. *Revue française de Sociologie, 22,* 361-395.

Lubek, I. (1984). Politics and psychology: Anarchist social psychology in fin-de-siècle France. Paper presented at the annual meetings of the American Psychological Association, Toronto, Canada, August 25.

Lubek, I. (1986). Fifty years of frustration and aggression: Some historical notes on a long-lived hypothesis. In K. Larsen (Ed.), *Dialectics and ideology in psychology* (pp. 30-84). Norwood, NJ: Ablex.

Lubek, I. & Apfelbaum, E. (1979). Analyse psycho-sociologique et historique de l'emprise d'un paradigme: l'apprentissage S-R, l'hypothèse frustration-agression, et l'effet Garcia. *Recherche de Psychologie Sociale, 1,* 112-120; 123-149.

Lubek, I., & Apfelbaum, E. (1987). Neo-behaviorism and the Garcia effect: A social psychology of science approach to the history of a paradigm clash. In M. Ash & W. Woodward (Eds), *Psychology in twentieth century thought and society* (pp. 59-91). Cambridge: Cambridge University Press.

Lubek, I., & Apfelbaum, E. (1988). The development of social psychology in France: some socio-historical and institutional factors. Paper presented at the 24th International Congress of Psychology, Sydney, Australia, August 31, 1988).

Lubek, I., & Apfelbaum, E. (1989). French and American social psychology between the wars: A comparative exploration. Paper presented at the Annual Meetings of the British Psychological Society's Section on History and Philosophy, Lincoln, England, March 28-30, 1989.

Maisonneuve, J. (1973). *Introduction à la psychosociologie.* Paris: Presses Universitaires de France.

Mead, G. H. (1934). *Mind, self and society.* Chicago: University of Chicago Press.

Milet, J. (1970). *Gabriel Tarde et la philosophie de l'histoire.* Paris: Vrin.

Samelson, F. (1974). History, origin, myth, and ideology: The "discovery" of social psychology by Auguste Comte. Reprinted in: K. S. Larsen (Ed.), *Dialectics and ideology in psychology* (pp. 14-29). Norwood, NJ: Ablex, 1986.

Shotter, J. (1984). *Social accountability and selfhood*. Oxford: Blackwell.

Smedslund, J. (1988). *Psychologics*. New York: Springer-Verlag.

Tarde, G. (1890/1903). *Les lois d'imitation. Etude sociologique*. Paris: Alcan. Translated by E. C. Parsons, as *The laws of imitation*. New York.

Tarde, G. (1902). *Psychologie économique* (2 Tomes). Paris: Alcan.

Worms, R. (1904). La sociologie et les sciences sociales (Compte rendu des conférences de Durkheim et Tarde, Décembre, 1903). *Revue Internationale de Sociologie, 12*, 83-87.

A COGNITIVE REVOLUTION IN INFANCY RESEARCH?

Ed Elbers

SUMMARY: The behaviorist program of infancy research has stagnated over the last decades. Ethologists and cognitivists were successful in opening up new areas for infancy research. Therefore, many authors speak of a cognitive revolution in infancy research. Nonetheless, behaviorists have convincingly demonstrated that the operant conditioning of infants is possible. As a reaction, cognitive and ethological researchers began performing conditioning research themselves and attempted to integrate this field of research into their theories. The picture of a revolution in infancy research has, thus, to be nuanced. The two camps still reject each other's basic assumptions and approach. In this respect there is discontinuity. Continuity is visible, however, in conditioning research in which we find rapprochement and co-operation between behaviorists and researchers belonging to the cognitive and ethological program.

Introduction

Many authors, reflecting upon the recent history of psychology, have seen discontinuity: they consider the transition from behaviorism to cognitive psychology as a revolution (e.g., Palermo, 1971; Overton, 1984). The term 'cognitive revolution' for denoting this transition has found wide acceptance. I shall present a case study from the recent history of psychology to show that successful science produces not only discontinuity and competition, but sometimes also rapprochement and even co-operation between scientists from differing traditions. I will discuss modern infancy research and, without denying that the psychological study of infancy has gone through a major conceptual shift over the past decades, I shall argue that this episode has also created continuity.

Although infancy research has a respectable tradition in psychology (the studies of Preyer and Baldwin around the turn of the century, of Gesell and Piaget in the thirties), the dominant programs of the forties and fifties (psychoanalysis and behaviorism) had not much to offer for the empirical study of infants. Despite their differences, psychoanalysis and behaviorism concur in viewing infants as totally dependent on their environment, incompetent and asocial (see Schaffer, 1971). Hampered by these presuppositions, these programs had little to offer to infancy research. A revival of interest in the psychological development of infants occurred around 1955, due to the emergence of new research programs — cognitive psychology and ethology. These programs advocated a radical revision of the image of infancy. Infants were considered active, competent in many respects, oriented towards their

environment and motivated for social interaction. Whereas behaviorists had estimated the innate repertoire of responses in babies as small, cognitivists and ethologists claimed that infants come into the world with a baggage of behavioral patterns and perceptual preferences which prepare them for human living. Instead of explaining infant development by conditioning, ethologists and cognitivists saw development as a continuous process of adaptation to the environment, a process in which the internal structures underlying behavior and perception are differentiated and reorganized. Their programs were extremely successful and opened up new areas of infancy research, such as mother-child interaction, behavioral states, perceptual preferences, and imitation.

Behaviorist researchers have played only a minor role in modern infancy studies. John Watson's conditioning studies with infants (mostly between 1916-1920), however pioneering, were less influential than Gesell's or Piaget's work: they failed to instigate an extensive research interest in infant development within behaviorism. At the beginning of the sixties, however, a group of Skinnerian psychologists became particularly interested in the development of infants. In this article I shall deal mainly with these investigators (the most important of whom is J. L. Gewirtz) and the vicissitudes of their theories. The program for their research was presented in publications by Bijou and Baer (1965) and Gewirtz (e.g., 1969). These theorists consider the development of infants as the product of conditioning processes, in which respondent and operant behaviors are changed under the influence of the environment. In keeping with Skinner's behaviorism, however, the conditioning of respondent behavior is deemed of little importance. More attention is given to the conditioning of operant behavior. Therefore, their research was carried out to establish which environmental stimuli can function as reinforcers of operant behavior and how the application of these reinforcers ('reinforcement') leads to the formation of behavior in a socially desirable direction.

With this program as a point of departure, behaviorists performed infancy research in the sixties and seventies. Although the quantity of their research was modest in comparison with that of cognitivists and ethologists, Gewirtz and his colleagues have succeeded in continuously attacking the opposing party with relevant arguments and original empirical studies. As a result, there have been some sharp debates on the development of infants. It is easy to show that there is a defensive aspect to the behaviorist research: the behaviorists reacted to the successes of the cognitivists and ethologists and tried to fit the discoveries of these programs with hindsight into their own theoretical frame. Nonetheless, as I shall show, this behaviorist counter-offensive has had an influence on modern infancy research.

The Debate on Infant Conditioning

Behaviorists achieved their objective and demonstrated that infants can be conditioned: they have shown that responses such as smiling, vocalizing, kicking, head movements, and sucking increase in frequency when they are systematically reinforced. The outcome of this research, however, did not come up to their expectations in every respect (Sameroff, 1972; Millar, 1976; Lipsitt, 1982). Conditioning research has made it clear that the possibilities for conditioning infants are restricted. Some forms of behavior can be changed more easily than others and, moreover, the nature of the reinforcing stimuli poses limitations on whether conditioning is possible.

Successful conditioning studies with newborn children have used behavioral patterns which are functional for the adaptation to natural circumstances. Behaviors of newborn children which are connected to drinking, such as sucking, head turning and opening the mouth, can easily be changed. Adaptation of these responses to the environment is essential for surviving. It is vital that the child can learn to develop the most effective sucking movements. Social responses which play a role in social interactions, such as smiling and vocalizing, can also be conditioned easily.

Not all reinforcers, however, are equally effective in conditioning these responses. It is easy to teach an infant to turn its head to get something to drink, but it is extremely hard to reinforce the same response with a visual stimulus, at least in children under six months of age.

These results fitted in well with the concept of "constraints on learning" proposed by Seligman in his article on learning research on animals (Seligman, 1970). Seligman attacked the idea of the generality of the behaviorist laws of learning. He argued that the behaviorist presupposition that any form of behavior can be changed by any positive reinforcer does not hold true in the light of animal research. It is easy enough to train a pigeon in a Skinner box to peck at a light, if this act results in the appearance of a food pellet. But it is practically impossible to teach pigeons to peck at the light in order to prevent an electric shock. Seligman explained these results by placing them in an evolutionary perspective; natural selection has prepared organisms to learn stimulus-response relationships which are vitally important. The animal is neutral or even 'contraprepared' to other relationships. For pigeons, pecking is a natural way to find food, and it is, therefore, understandable that they can learn to peck at a light to get food. But these animals respond to danger by flying away and not by pecking. To continue pecking would be dangerous, so it is plausible that pigeons are contraprepared to peck in order to evade a shock.

Seligman's theory of preparedness and constraints on learning has been discussed extensively among students of infant conditioning. Behaviorists, in these discussions, have made a conspicuous change in their judgement of the

results of the conditioning studies. Initially, they opposed the idea of preparedness by using a standard argument. They argued that many responses in infants only seem to be innate. In reality they are the outcome of previous conditioning processes (Gewirtz, 1977). They argued that similarity of behavioral patterns in infants of different cultures does not prove the idea of a biological preparation. The extreme helplessness of newborn infants evokes the same regime of caring and thus the same conditions of reinforcement, regardless of the culture. The development and experience of infants, therefore, are largely independent of culture, and this gives the impression that these response patterns have not been learned. Gewirtz reacted to the concept of constraints on learning by arguing that this term has been used too often as an excuse to neglect environmental conditions of learning. A child can also be prepared for new learning experiences by previous learning experiences (Gewirtz & Petrovich, 1982).

Nonetheless, behaviorists have given up this argument and finally acknowledge that infants are prepared to learn certain behaviors in certain contexts and that this is a matter of biological determination. This means that they have given up the idea that infants are born with a minimal repertoire of innate behaviors. This change can be seen in a number of publications by behaviorist investigators of infancy (see, i.e., Millar, 1976; Gewirtz & Petrovich, 1982). These behaviorists still maintain that development is the result of conditioning, but they no longer brandish the argument that the appearance of new behavior must be ascribed to influences from the environment. Thus, behaviorist investigators of infancy have moved in the direction of their opponents. They now acknowledge that the idea of early behaviorism, that any behavior can be connected to any positive stimulus, has proven to be unwarranted.

These recent publications show that behaviorist students of infancy are steadily abandoning their extreme nurture position. In recent articles by Gewirtz, a mass of ethological findings is presented, and he candidly acknowledges that every animal species possesses typical abilities for and restrictions on learning. The development of human infants, Gewirtz and Petrovich (1982) write, is not only the result of certain learning experiences, but is also caused by biological, genetically programmed processes. This does not mean, however, that these authors have given up their belief in the importance of conditioning.

It is only natural that behaviorists should give up their extreme nurture position, but not their belief in the omnipresence of the principle of conditioning. Behaviorists have, in fact, always accepted that an infant is born with a repertoire of reflexes and responses; processes of conditioning interlock with these reflexes and responses. What they are now gradually giving up is the most elastic part of the behaviorist view — their presupposition that this repertoire is minimal.

How did cognitivist and ethological researchers react to the findings on infant conditioning? When it became clear from behaviorist research that infants can only be conditioned under certain circumstances and that these results can only be understood when the biological adaptation of infants is taken into account, this field of research became interesting to cognitivist and ethological psychologists of infancy. This outcome of behaviorist research fitted nicely into their programs. Hence, nonbehaviorist investigators began performing conditioning research. They did so without giving up their global rejection of behaviorism. Initially, cognitivists explained the successes of the behaviorist conditioning studies by elicitation. They responded, for instance, to the Rheingold, Gewirtz and Ross (1959) experiment, in which vocal responses of infants were successfully conditioned by social behavior of the experimenter, by pointing to the fact that social behavior elicits vocal sounds in infants. The increased vocalizing, therefore, is not caused by reinforcement. On the contrary, the infant's vocalizations are a spontaneous answer to the social stimuli provided. This cognitivist claim could be demonstrated in a study by Bloom and Esposito (1975) who made suitable experimental controls to distinguish between the effects of elicitation and the effects of conditioning.

Some cognitive and ethological researchers reject the idea of conditioning in human beings (e.g., Brewer, 1974). Nonetheless, many cognitivist and ethological researchers have accepted that conditioning occurs in infant development. This does not mean that they have accepted basic presuppositions of the behaviorist research program, or even that they feel committed to the idea that conditioning is a paramount force in infant development. One of them, Sameroff, is not only an expert in the field of conditioning sucking behavior in newborn infants, but is also one of the fiercest critics of behaviorism in developmental psychology (Sameroff, 1972, 1983). Another example is the well-known English developmental psychologist H. R. Schaffer, who, in his widely read book from 1971 (Schaffer, 1971), fulminates against behaviorism and especially against the idea that the social development of infants can be explained with conditioning, but who performed conditioning experiments himself (Millar & Schaffer, 1972).

Operant conditioning techniques have been widely used in cognitive research. For example, Bower (1966) studied shape and size constancy in infants. First he conditioned his subjects to turn the head when a visual stimulus was presented (in the case of size constancy he presented a cube); the appearance of the mother was used as a reinforcing stimulus. Then cubes of different sizes and at varying distances were presented. In each case, the amount of headturning was taken as an indication of the infant's visual skills.

In purely technical respects no differences exist between the conditioning experiments of cognitivists and the conditioning research of behaviorists. Investigators from both groups agree on technical research matters — which procedures are the most successful, how undesired effect can be controlled,

and even where elicitation effects can possibly be apparent. As to the description and the technical evaluation of the experiments, no essential difference can be found between review articles of a cognitivist (such as Sameroff) or of behaviorists (like Rovie-Collier, 1983). If competing research programs are characterized by different methodologies, we have found an exception here. But differences emerge immediately when the research results must be interpreted in a more encompassing way. Can the results best be interpreted in a conditioning approach or are they the expression of the infant's adaptation to the environment? Sameroff, in his conclusions, is considerably more hesitant and less inclined to admit that conditioning has been proved than his behaviorist colleagues (such as Rovie-Collier, 1983). Although he accepts conditioning as a mechanism, Sameroff relativizes its importance by maintaining that the successes scored in the conditioning studies are rooted in biological and cognitive processes more fundamental and pervasive than conditioning (cf. Sameroff & Cavanagh, 1979).

The manner in which some cognitivist and ethological students of infancy allow for conditioning as a factor in infant development is nicely illustrated in the recent review study by Kaye (1982). According to Kaye, operant conditioning, next to cognitive mechanisms, plays a part in the earliest development of infants, but the effects of conditioning are soon restricted by temporal conditions regarding the relationship between response and reinforcing stimulus. Kaye refers to an investigation by Millar and Watson (1979) who found that, to be effective, the stimulus reinforcing operant behavior in newborn infants must follow the behavior in less than three seconds, a restriction which still holds for children of six to eight months of age. In Kaye's opinion, this limitation only exists for short-term memory in perceiving contingencies in conditioning: memory processes which are involved in other developmental mechanisms, like imitation, do not suffer from the same restriction. Consequently, Kaye argues, imitation soon becomes a more important instrument in the development of infants than operant conditioning. Adults will, after a while, start to demonstrate desired behavior instead of reinforcing it. Operant conditioning, in Kaye's account, is still possible, but it loses importance, because other, more effective cognitive mechanisms become available.

Kaye's view demonstrates that ethologists and cognitivists do not simply add conditioning to their own principles of explanation. In that case, conditioning would only be an alien element in their theories. Just as Seligman's concept of preparedness allowed establishing the biological conditions of conditioning, Kaye attempts to discover the cognitive conditions of conditioning (especially the restrictions of short-term memory in infants). At the same time it is clear that Kaye attributes the leading role to cognitive processes and argues that conditioning is given only a marginal position.

Conclusions

I have compared the results of the behaviorist program of infancy research with investigations originating from ethological and cognitivist orientations. The outcome of this evaluation is not favorable for behaviorism. The behaviorist program has shown a limited heuristic power; apart from insisting on the importance of conditioning, behaviorists have restricted themselves to a defensive reaction by trying, with hindsight, to fit the new discoveries of ethologists and cognitivists into their own theoretical frameworks. Behaviorist investigators of infancy reacted to the successes of ethologists and cognitivists by revising their theories in the direction of their opponents' views.

We cannot, however, merely conclude that in the rivalry between two research programs the behaviorist program was the loser and that the image of a discontinuous development in psychology has been confirmed. In reality, continuity can also be found in this episode of modern psychology. Behaviorist developmental psychologists and developmentalists with a cognitivist or ethological orientation have come closer to each other on one point: the conditioning of infants. Behaviorist investigators of infancy have demonstrated that infants can be conditioned. But since not all of their predictions turned out as they had expected, they inevitably moved in the direction of their opponents' viewpoints. Cognitivists and ethologists were able to incorporate some of the results of behaviorist infancy research and began performing conditioning research themselves, which would never have occurred to them without the behaviorist research. Hence, some cognitivist and ethological investigators accept the idea that processes of conditioning form a part of infant development. They can assimilate conditioning with their theories by specifying under which biological and cognitive circumstances conditioning can occur. Because they have taken over a field of research which was initially very much characteristic of behaviorism and because they perform this research with basically the same methods as their behaviorist colleagues, continuity in psychology is visible here. This continuity, however, only exists when we look at conditioning research, which means at a local level. Globally speaking, the differences between the two groups of researchers have not disappeared: at that level we see a stagnation of the behaviorist program, and, therefore, discontinuity. In conclusion, neither Kuhn's idea of a scientific revolution nor Lakatos' notion of discontinuity across research programs is sufficiently nuanced to picture the development of theories and methods in the psychology of infancy. Simply applying these conceptions of the history of science blinds us to the complicated relationships in practice between researchers belonging to opposing research programs — relationships ranging from neglect and critical discussions to rapprochement and co-operation.

References

Bijou, S. W., & Baer, D. M. (1965). *Child development. Volume II. Universal stage of infancy.* New York: Appleton-Century-Crofts.

Bloom, K., & Esposito, A. (1975). Social conditioning and its proper control procedures. *Journal of Experimental Child Psychology, 19*, 209-222.

Bower, T. G. R. (1966). The visual world of infants. *Scientific American, 215*, 80-92.

Brewer, W. F. (1974). There is no convincing evidence for operant or classical conditioning in adult humans. In W. B. Weimer & D. S. Palermo (Eds.), *Cognition and the symbolic processes* (pp. 1-42). Hillsdale: Erlbaum.

Gewirtz, J. L. (1969). Mechanisms of social learning: Some roles of stimulation and behavior in early human development. In D. A. Goslin (Ed.), *Handbook of socialization theory and research* (pp. 57-112). Chicago: Rand McNally.

Gewirtz, J. L. (1977). Maternal responding and the conditioning of infant crying: Directions of influence within the attachment-acquisition process. In B. C. Etzel, J. M. LeBlanc, & D. M. Baer, (Eds.), *New developments in behavioral research. Theory, method and application* (pp. 31-57). Hillsdale: Erlbaum.

Gewirtz, J. L., & Petrovich, S. B. (1982). Early social and attachment learning in the frame of organic and cultural evolution. In T. M. Field, A. Huston, H. C. Quay, L. Troll, & G. E. Finley (Eds.), *Review of human development* (pp. 3-19). New York: Wiley.

Kaye, K. (1982). *The mental and social life of babies. How parents create persons.* Brighton: Harvester.

Lipsitt, L. P. (1982). Infant learning. In T. M. Field, A. Huston, H. C. Quay, L. Troll, & G. E. Finley (Eds.), *Review of human development* (pp. 189-203). New York: Wiley.

Millar, W. S. (1976). Operant acquisition of social behaviors in infancy: Basic problems and constraints. In H. W. Reese (Ed.), *Advances in child development and behavior. Volume 11* (pp. 107-140). New York: Academic Press.

Millar, W. S., & Schaffer, H. R. (1972). The influence of spatially displaced feedback on infant operant conditioning. *Journal of Experimental Child Psychology, 14*, 442-453.

Millar, W. S., & Watson, J. S. (1979). The effect of delayed feedback on infant learning reexamined. *Child Development, 50*, 747-751.

Overton, W. F. (1984). World views and their influence on psychological theory and research: Kuhn, Lakatos, Laudan. In H. W. Reese (Ed.), *Advances in child development and behavior. Volume 18* (pp. 191-226). New York: Academic Press.

Palermo, D. S. (1971). Is a scientific revolution taking place in psychology? *Science Studies, 1,* 135-155.

Rheingold, H. R., Gewirtz, J. L., & Ross, H. W. (1959). Social conditioning of vocalization in the infant. *Journal of Comparative and Physiological Psychology, 52,* 68-73.

Rovie-Collier, C. K. (1983). Infants as problem-solvers: A psychobiological perspective. In M. D. Zeiler & P. Harzem (Eds.), Biological factors in learning (pp. 63-101), Chichester: Wiley.

Sameroff, A. J. (1972). Learning and adaptation in infancy: A comparison of models. In H. W. Reese (Ed.), *Advances in Child Development and Behavior. Volume 7* (pp. 169-214). New York: Academic Press.

Sameroff, A. J. (1983). Developmental systems: Contexts and evolution. In W. Kessen (Ed.), *Handbook of child psychology. Volume I. History, theory, and methods* (pp. 237-294). New York: Wiley.

Sameroff, A. J., & Cavanagh, P. J. (1979). Learning in infancy: A developmental perspective. In J. D. Osofsky (Ed.), *Handbook of infant development* (pp. 344-392). New York: Wiley.

Schaffer, H. R. (1971). *The growth of sociability.* Harmondsworth: Penguin Books.

Seligman, M. E. P. (1970). On the generality of the laws of learning. *Psychological Review, 77,* 406-418.

PIAGET, VYGOTSKY, AND THE DEVELOPMENT OF CONSCIOUSNESS

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SUMMARY: This paper presents a comparative analysis of the early works of Piaget and Vygotsky and argues that an understanding of human consciousness was a central issue in their works. Appreciating the importance of this issue helps explicate certain themes within each theory, as well as points of contrast between them. Further, the problem of consciousness has become a contemporary concern for those interested in proposing a computational theory of mind and, while there is a stark contrast between Piaget's and Vygotsky's theoretical orientation to consciousness, the debate between them is a discourse that is markedly different from what is presently being discussed, offering a fresh perspective on this very traditional problem.

In recent years there has been considerable effort given to comparative studies of the writings of Jean Piaget and Lev Semovich Vygotsky. Such efforts typically focus on Piaget's and Vygotsky's contribution to the developmental literature. However, it is important to remember that both Piaget and Vygotsky had broader agendas that involved theories of mind, and only implemented developmental methodologies. Keeping this in mind not only allows a fuller comparison of their theoretical orientations but also makes such a comparison relevant to a wider range of topics than is typically assumed. This paper contends that within Piaget's genetic epistemology, and within Vygotsky's attempt to establish a Marxist psychology, the problem of consciousness was a central concern. Appreciating the importance of this problem can help explicate certain themes within each of their respective theories as well as points of contrast between them. Further, the problem of consciousness has become a contemporary concern for those interested in proposing a computational theory of mind (Jackendoff, 1987; Johnson-Laird, 1988); and, while there is a stark contrast between Piaget's and Vygotsky's theoretical orientation to consciousness, the debate between them is a discourse that is markedly different from what is presently being discussed, offering a fresh perspective on this very traditional problem.

Piaget's Early Theory of Consciousness

Throughout his work, it could be said that Piaget was never intrested in the mere effects of mind and their efficient causes. Rather, his object of study was the structures of mind, emphasizing their systematic nature and self-regulation. Such structures were never explained as a mere outcome of antecedent conditions, but rather, were described as total systems of elements related to each other in specific ways and explained in terms of a range of

possible interconnections. Piaget's method for studying such structures came from traditions in biological science. This method involves three phases: the first is taxonomic, in which the phenomenon of interest is described and differentiated into working classifications; the second phase is explanatory to the extent that the facts used in the descriptive phase are co-ordinated into functional relationships; the final phase involves the construction of a model of the functional relationships composed of deductive laws, which allows an approximation of a causal explanation (Chapman, 1988). This orientation can be seen in Piaget's earliest works (1923, 1924), which proposed an account of the development of consciousness.

Piaget's (1923) first volume begins with a taxonomy of children's use of language, distinguishing between the social functions of speed and the non-social functions. As is well known, he concluded that children's language does not exclusively serve communicative purposes. Children's language also reflects aspects of subjective states. Further, he suggested that children's thought, as expressed in speech, is egocentric in nature, that is, "halfway between autistic thought which is undirected...and directed intelligence" (p. 151). At the end of this volume, the question remained as to whether egocentric speech is characteristic of immature language usage, or if egocentrism constitutes the basic form of children's conscious experience. This question is taken up in *Judgment and Reasoning in the Child* (1924), in which Piaget proposed a theory of the development of consciousness.

In this second volume, Piaget found that egocentrism is not merely restricted to children's linguistic expressions but is the constitutive form of children's thought. The studies reported in this volume revealed that egocentrism extends to children's understanding of certain logical connectives, the comprehension of relational terms, and the ability to introspect about simple reasoning problems. For Piaget, the diffuse state of egocentrism, evident in children's speech and thought, is explained as a result of children being unconscious of themselves and their actions. Piaget asked, "Does his (the child) egocentrism go hand in hand with a certain degree of unconsciousness?" (p. 210). He concluded that egocentrism "... consists of a series of discontinuous judgments which determine one another extrinsically and not intrinsically, or to put it differently, which entail one another like unconscious acts, not like conscious judgment ..." (p. 212). Piaget argued that the decline in egocentrism, and the establishment of adaptive intelligence, is a function of the development of consciousness awareness, which develops according to two laws. The first is the 'law of conscious realization' which states that the more we make use of a relation, the less conscious we are of it; and we become conscious in proportion to our dysadaption or conflict. Piaget writes:

> For in so far as he is thinking for himself, the child has no need to be aware
> of the mechanisms of his reasoning. His attention is wholly turned towards
> the external world, towards actions, in no way directed towards thought as a

> medium interposed between the world and himself. In so far, on the other
> hand, as the child seeks to adapt himself to others, he creates between himself
> and them a new order of reality, a new place of thought, where speech and
> argument will hence forth hold their sway, and upon which operations and
> relations which till then have been the work of action alone will now be
> handled by imagination and by words. The child will therefore have to
> become conscious to the same extent of these operations and relations which
> till then had remained unconscious because they were sufficient for the
> purpose of action (Piaget, 1924, p. 198).

Thus, through conflict, and the need to verify and justify actions to
others, arises conscious awareness; and with the development of this aware-
ness there is a corresponding decline in egocentrism. However, Piaget
pointed out that while the 'law of conscious realization' helps explain the
motivation for the development of consciousness, it does not explain the
means by which conscious awareness comes about.

The second law required to explain the development of conscious aware-
ness was referred to as the law of 'shifting' (or decalage). Piaget stated:

> For to become conscious of an operation is to make it pass from the plane
> of action to that of language: it is therefore to reinvent it in imagination in
> order to express it in words (Piaget, 1924, pp. 213-214).

This second law points to the means for achieving awareness. Language plays
a fundamental role in this development, as perhaps any semiotic system might.
Piaget concluded this volume by elucidating a model of the characteristics of
the young child's egocentric form of consciousness (or unconsciousness), and
the limitations that would have to be overcome with the development of
awareness.

Piaget (1929, 1930) extended his studies of egocentrism in two other
works in which he reported that the child's thought develops from a state of
diffuse realism, where the child's internal and external experiences are not
differentiated and all thoughts are directed towards immediate perception,
towards an appreciation of the relativity of perspectives, an understanding of
reciprocal relationships, and an openness towards an objective comprehen-
sion of reality. This course of development essentially mirrors the child's
development from unconscious egocentrism to a state of conscious aware-
ness. Piaget writes:

> ... as the child becomes conscious of his subjectivity, he rids himself of his
> egocentricity. For after all, it is in so far as we fail to realize the personal
> nature of our own point of view that we regard this point of view as absolute
> and shared by all. Whereas, in so far as we discover the purely individual
> character, we learn to distinguish our own from the objective point of view.
> Egocentricity, in a word, diminishes as we become conscious of our own
> subjectivity (Piaget, 1930, pp. 246-247).

Vygotsky's Theory of Consciousness

Throughout Vygotsky's brief career the problem of consciousness was clearly a central concern. In his earliest writings he argued that a scientific psychology could not ignore human consciouness, "To put it simply, a human is always thinking about himself; this process is never without some influence on his behavior" (1925, p. 7). Vygotsky rejected the stimulus-response framework of his time but also criticized attempts to directly study consciousness through introspection. Instead, he proposed that an understanding of consciousness must be sought at a level different from consciousness itself; for Vygotsky, this was the level of socially meaningful activity. Social activity was proposed to be the generator of consciousness. Vygotsky writes, "The mechanism of social behavior and the mechanism of consciousness are the same ... We are aware of ourselves as we are aware of others; this is as it is because in relation to ourselves we are in the same position as others are to us" (cited in Kozulin, 1987, p. 35). Methodologically, Vygotsky focused on structures of higher mental processes, using what he referred to as the 'experimental genetic' method. This method is dependent on detailed descriptions which trace the historical process of a psychological structure (Vygotsky, 1978).

In Vygotsky's approach to consciousness, the role of 'internalization' is crucial. Internalization is the way in which social activities are transformed from external processes to aspects of subjective experience. This is believed to be achieved through the relationship between individual, spontaneous activities and socially meaningful, symbolic activities. An explication of this process is worked out in Vygotsky's classic text, *Thought and Language* (Vygotsky 1934).

In this volume, Vygotsky's objective was to examine the interfunctional aspects of human consciousness. What becomes most important is not merely the internalization of any particular social activity, but the establishment of internal relations between different functions. In his account of child development, as is well known, Vygotsky argued that the first several years of life involve the establishment of the interfunctional relationships between the personal, spontaneous functions of thought, and the social, symbolic functions of language. Initially, the two functions are separate, and only gradually come together, each maintaining an autonomous, though reciprocal, existence.

Vygotsky also proposed a similar argument for concept formation. Children's awareness of concepts was seen as another example of a change in the interfunctional relationship between different types of conceptual functions. Vygotsky distinguished between spontaneous concepts, derived from the 'bottom up' in the child's everyday activities, and scientific concepts, derived from the 'top down' through social encounters in school. Spontaneous concepts are rich in concrete manifestations and weakest in sys-

tematic organization; scientific concepts are well organized, but weakest in terms of clear exemplars. Vygotsky explained the awareness of concepts as a result of the interfunctional relationship between these two types of concepts. Spontaneous concepts, moving upward towards organization are influenced by 'top down' scientific concepts. Likewise, scientific concepts are facilitated by the concrete exemplars of spontaneous 'bottom up' concepts. With regard to the development of conscious awareness, what is most important is the influence of socially meaningful, scientific concepts. Vygotsky concluded that, "Reflective consciousness comes to the child through the portals of scientific concepts" (p. 171).

A Comparative View

In Piaget's early theory the development of consciousness proceeds from an undifferentiated state, where neither the subjective nor the objective dimensions of experience are clearly defined, to a condition in which the child is aware of his subjectivity, and can openly respond to the possibility of objective experiences. While Piaget's early theory of the development of consciousness may be vague at points, the themes of conflict and reconstruction are clearly underscored. These two themes were important enough to be carried through to Piaget's very last works; and at the time of his early writings, they were the focus of Vygotsky's criticism. Vygotsky charged that Piaget's theory of the development of consciousness presupposes the importance of individual experience at the expense of social reality, and that the themes of conflict and reconstruction are, at most, only descriptive with no explanatory power. In contrast, Vygotsky argued that the development of conscious awareness is the result of the interfunctional relationship between socially meaningful activities and private, subjective experiences.

Despite Vygotsky's criticism, it may appear that the positions of Piaget and Vygotsky are similar in several respects. Piaget stressed the importance of conflict, particularly from within a social context; Vygotsky saw socially meaningful activity as the generator of consciousness. For Piaget, shifting or reconstructing an action on another plane is essential to the development of consciousness; for Vygotsky the interfunctional relationship between two psychological processes such as spontaneous concepts and scientific concepts, results in the emergence of conciousness. Thus, it could be said that the issue of conflict and reconstruction are important to both theoretical orientations.

However, while there may be common ground between the two theories to the extent that they both share a Hegelian orientation to mind, there are also subtle and important differences between them. For Piaget, the emphasis was on social conflict as the motivation for consciousness, while Vygotsky underscored dialogue (or instructions) rather than disagreement. Also, while Piaget spoke of shifting, and the importance of reconstructing action on the representational plane of operations, Vygotsky spoke of interfunctional rela-

tions, and stressed the importance of the types of social symbols that are involved. In particular, functions that involve formal, socially meaningful symbols (i.e., language, scientific concepts, writing) are of central importance in redefining the 'everyday' functions of the subjects. Vygotsky viewed the internalization of such socially meaningful activities as critical to the development of consciousness. For Piaget, the individual is central in the development of consciousness. Social conflict may establish the need for awareness, but it is through the individual's interiorization of such conflicts that consciousness is achieved. Thus, for Piaget, social activity lays the path for the development of consciousness, while for Vygotsky the development of consciousness is social activity.

In this comparative study, Vygotsky's criticism and position pulls at the vagueness of Piaget's early writings, and highlights what seems neglected. However, Vygotsky's approach to the development of consciousness is not without its own question begging assertions. Vygotsky characterized socially meaningful activity as the generator of consciousness, but the notion of socially meaningful activity is presented as a fait accompli. There is no discussion of how social activity becomes meaningful to the individual. In discussing language, Vygotsky stressed its social origins and organizing functions but did not consider the influence of the self-generative activities of the individual on linguistic expression.

In discussing concept formation, Vygotsky pointed out that the teacher-student dialogue is instrumental in the internalization of scientific concepts. Yet dialogue between teacher and student is not easily, nor always, achieved. Further, the notion of internalization is itself vague and descriptive. Finally, in describing the importance of 'top down' concepts, Vygotsky noted that they bear the 'imprint' of children's spontaneous activities, yet this point never receives elaboration. In fact, the entire development of spontaneous concepts is neglected. Thus, it is not clear how, and under what conditons, 'top down' concepts are acquired. These criticisms of Vygotsky's position would seem to suggest that an account of the development of consciousness necessarily requires a consideration of the nature of subjective experience.

According to Piaget, the impact of socially meaningful activity requires that the child be amendable to such encounters. An examination of the conditions that make such encounters possible became the focus of Piaget's work throughout the 1970s. As he formulated the problem, "when is a subject fully conscious of a situation? How is this consciousness acquired? In other words, what constitutes the dawn of consciousness?" (1974, p. iii). This latter work can be understood in direct continuity with Piaget's earlier writings.

The law of conscious realization, emphasizing the importance of conflict as a motivation for awareness, is pursued in Piaget's elaboration of his most recent equilibration model (1975). Here he viewed consciousness as developing through a variety of activities (both successes as well as failures), which

give rise to a sense of dissonance. Piaget attempted to define the different types of dissonance that can be experienced by the subject, which can serve as motivation for the development of an awareness that transcends the mere performance of an action to a level of understanding the action. In this new model, dissonance is not described as arising from external sources (such as social encounters) but rather as a result of deficiencies in an already formed subject-object mode of interaction.

The law of shifting has been elaborated in Piaget's more recent discussion of reflective abstraction: empirical abstraction involving the abstraction of physical qualities, such as color and weight, and more importantly reflective abstraction, in which what is abstracted is a knowledge of one's own actions on objects or in events. Further, Piaget contended that the development of consciousness, when reconstructed on the plane of conceptualization, proceeds from the 'periphery to the center,' that is, from the most general awareness of the point of subject-object contact to an awareness of the central co-ordination of the mechanisms of action and the properties of objects. Space does not permit a more elaborate account of these latter developments in Piaget's theory; however, it seems that a full understanding requires an appreciation of their position relative to Piaget's earliest works.

References

Chapman, M. (1988). *Constructive evolution*. NY: Cambridge University Press.

Jackendoff, R. (1987). *Consciousness and the computational mind*. Cambridge, MA: The MIT Press.

Johnson-Laird, H. (1988). *Computational theory of mind*. Bradford, MA: The MIT Press.

Kozulin, A. (1987). Vygotsky in context. In L. S. Vygotsky's *Thought and Language*. Cambridge, MA: The MIT Press.

Piaget, J. (1923). *The language and thought of the child*. (Trans., M. Gabain). NY: World Publishing.

Piaget, J. (1924). *Judgment and reasoning of the child*. (Trans., M. Warden). Totowa, NJ: Littlefield, Adams & Co.

Piaget, J. (1929). *The child's conception of the world*. (Trans., J. & A. Tomlinson). Totowa, NJ: Littlefield, Adams & Co.

Piaget, J. (1930). *The child's conception of physical causality*. (Trans., M. Gabain). Totowa, NJ: Littlefield, Adams & Co.

Piaget, J. (1974). *The grasp of consciousness*. (Trans., S. Wegwood). Cambridge, MA: Harvard University Press.

Piaget, J. (1975). *The equilibration of cognitive structures*. (Trans., T. Brown & K. J. Thampy). Chicago, IL: Chicago University Press.

Vygotsky, L. S. (1925). Consciousness as a problem in the psychology of behavior. *Soviet Psychology, 17*, 3-35.

Vygotsky, L. S. (1934). *Thought and language.* (Trans., A. Kozulin). Cambridge, MA: The MIT Press.

Vygotsky, L. S. (1978). *Mind and Society.* Cambridge, MA: Harvard University Press.

THE SIGNIFICANCE OF BÜHLER'S 'AXIOMATIC' AND VYGOTSKY'S 'GENERAL PSYCHOLOGY' FOR THEORETICAL PSYCHOLOGY AND ITS PERSISTENT MONISM-PLURALISM-DEBATE

Wolfgang Maiers

Summary: One of the issues plaguing contemporary theoretical psychology concerns the question whether our discipline should proceed on a pluralistic or monistic epistemic base. Two historical analyses of a crisis in psychology, those of Bühler and Vygotsky, are discussed because they seem to apply as prototypes to this strategic alternative. A comparative review shows that this common suggestion is untenable. Both views, albeit differing in other respects, share the goal of a non-eclectic unification of psychology in non-empiricist terms. The relevance of Vygotsky's 'General' and Bühler's 'Axiomatic Psychology' to modern attempts at a conceptual and methodological refoundation of psychology is considered.

Introduction

A prominent issue in 'theoretical psychology' is the question whether scientific progress is bound to monistic unification of psychology (e.g., Giorgi, 1976; Maiers & Markard, 1987; Tolman, 1988) or to its acknowledgment as an aggregation of varied studies (Koch, 1976) — or whether eclecticism is the very road to integration (Plaum, 1988). Those who plead for a non-competitive pluralism, while assuming a network of valid domain-specific research programs or theories, disguise the true problematic, which is an unsettled rivalry of incompatible or indefinite conceptualizations of one and the same domain. 'Theoretical pluralism' dignifies this scientific indeterminacy that is at the core of psychology's crisis. It corresponds to the relativism of the currently favored constructionist alternative to the *prima facie* discredited claims of realism. A renewal of a 'unitary science' project of psychology *à la* logical empiricism is certainly out of the question. Its rebuttal, however, does not preclude the possibility of an anti-scientism which is both, monistic and realistic. Theoretical monism, with its quest for general, lawful statements, need not distort the manifold aspects of reality and rule out different points of view.

The controversy is not new, but follows chronic debates on psychology's unity, basically about its *a priori* duality with reference to the sciences and the humanities. The historical settlement of this discussion proved to be a schismatic Pyrrhic victory. The never-muted critique that the objectivism of

behavioral psychology has, at most, caught only 'half' of the subject matter, gained momentum with psychology's reception of action theory and its model of human agency and subjectivity. With us again is the ancient issue of the reconcilability of the intentional and the causal, the idiographic and the nomothetical modes of thinking (cf. Herrmann, 1987). Theoretical psychology with its monism-pluralism dissent mirrors at a meta-level this conceptual and methodological incoherence of psychology. In such circumstances historical research becomes especially relevant as a potential guide (cf. Maiers, 1985; 1989).

In the present paper I wish to examine two works from 1927: Lev Vygotsky's *The Crisis of Psychology in its Historical Significance* (1985a) and Karl Bühler's *The Crisis of Psychology* (1978).[1] They seem to take opposite sides in the above issue: in Western psychology, Bühler's approach has typically been received as a positive historical model for antimonistic criticism and pluralist synthesis (e.g. Wellek, 1982; Allport, 1966). Conversely, advocates of a consistently materialist psychology judged that Bühler's compromise of conflicting approaches within a single system inevitably resulted in a wedding of insufficiencies. As Yaroshevsky put it, "'The foundation of psychology has to be reconstructed', the Soviet psychologist Vygotsky stated 1932, contrary to Bühler." (1975, p. 235; also see Rubinstein, 1979)

In the following account I shall not contrast Bühler's views as rooted in German idealism with Vygotsky's materialist tradition, but rather seek for common or bridging ideas that may run counter to the prejudice of Bühler's "systematic eclecticism" as opposed to Vygotsky's stance.

Axiomatic Reorientation of Psychology as a 'Life Science'

Bühler regarded all new currents around the turn of the century as critical responses to classical association psychology with its postulates of experiential subjectivism, atomism, sensualism, or mechanism. While the Würzburg School and psychoanalysis left untouched the "prerogative of inner perception" (Bühler, 1978, p. 17), psychology's crisis came to a head only with two variants of an objective approach: the Anglo-American turn towards behaviorism and the German *geisteswissenschaftliche Psychologie*, exploring individual mental states in relation to transindividual historical products of the objective mind.

Bühler recognized such polarizations of seemingly incommensurate systems not as a "crisis of decay" [*Zerfallskrise*], but rather one "of construction" [*Aufbaukrise*], an "*embarras de richesse*" (p. 1). Resolution would come

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[1] On the history of Vygotsky's then unpublished text, cf. Luria, 1979, and Leontiev, 1985; besides, cf. van IJzendoorn & van den Veer, 1984, and Rückriem, 1986. A detailed comparison with Bühler's analysis is given by Maiers, 1988a. Apparently, Vygotsky was unaware of his colleague's treatise despite an earlier version published in 1926. I do not know of any later reference.

from clarifying the relations between "experience", "conduct", and "products of the objective mind" (p. 29). "I assert that each of the three aspects is possible and none dispensable in the single science of psychology.... It then becomes a philosophical problem whether and to which unity, not yet named, these three initial objects [*Ausgangsgegenstände*] belong or lead to as constituents." (p. 29) The "philosophical attempts at a reorientation of psychology", he wrote (p. 65), should be directed to a homogeneous "conceptual system (...) in which eventually the data of all three aspects could be entered according to a clear procedure of translation".

In later treatises (e.g., 1965) Bühler explains more generally his idea of a "philosophy of science". Unlike the formally oriented neopositivist logic of research or neo-Kantian typologies of science, he aimed at "a theory of science" that explicated in a historical-critical manner the "axiom system" [*Axiomatik*] of specified disciplines. This meant reviewing the existing categories, regressively uncovering the principles that underlie the conceptual framework and empirical investigations, and ascertaining the basic "induction ideas" that regulate the constitution of a scientific subject matter. The essence of his proposal was to determine the presuppositions by which a class of phenomena, that could be viewed from various angles, is delimited as "a singular subject, a uniform research domain" (1932, p. 95).

In his 1927 review Bühler (1978) inspected the heterogeneous tenets accordingly. He showed that the three aspects (experience, conduct, and mental objectifications) were interrelated by demonstrating exemplarily (pp. 29-62), that "language", for example, cannot be grasped within the solipsistic frame of an individual-centered psychology of consciousness. The origin of semantics — and hence of language — is to be found in communal life requiring interindividual behavioral co-ordination and, to that end, particular media of communication. This is the first axiom. While behaviorism permits psychology to determine publicly observable communicative intercourse as elements, thus avoiding undue mentalistic suppositions, it cannot ultimately detect psychologically relevant units and organization in the totality of perceptible behaviors without appealing to goals and subject-relatedness [*Subjektsbezogenheit*]. This second axiom calls experiential psychology to the scene. Finally, and this is the third axiom, the viewpoint of the *geisteswissenschaftliche Psychologie* becomes indispensable because human language, beyond the (infrahuman) semantic functions of expression [*Ausdruck*] and appeal [*Auslösung*], comprises a unique dimension, namely that of communication as a tool to represent [*darstellen*] objective meanings.

To evaluate Bühler's triad of aspects/axioms for its contents does not fall within the scope of my paper. Let me briefly comment, however, that the Marxist critics mentioned above, in blaming Bühler for summing up the defects of the behaviorist, experiential, and *geisteswissenschaftliche* definitions of the psychological, apparently misjudged what he intended to synthesize.

For example, Rubinstein's (1979) critique of the mechanicist distortion of activity in the behaviorist anti-mentalism does not hit Bühler insofar as Bühler referred to the "behaviorism of the animal psychologists," that is, rationally understood: the comparative research of psychophylogenesis. It achieves methodical objectivity by banning anthropomorphic reifications of human consciousness, not by scientifically denying psychic processes altogether. (It does not alter this intention that Bühler's reference mistook the factual circumstances of American behavioral psychology at his time. *De facto*, the supposed triumph of evolutionary thinking had long been killed in psychology's history [see Tolman, 1987; Maiers, 1988b] — and this, as will be made clear below, affected Bühler's own approach too.) Rubinstein's (op. cit.) legitimate criticism that psychology's preoccupation with a fictitious consciousness disconnected from practice leads to phenomenalism cannot hit Bühler either, as Bühler demanded that the "solipsistic observational domain [*Schaubereich*] of experiential psychology" (1933, p. 41) be transcended. (This, if anything, came close to Vygotsky's exploration of consciousness as a social-historical system or structure of meaning.) Finally, Rubinstein's (op. cit.) reservation ignores the congenial objections that Bühler made to emphatic hypostases of an autonomous mind: Bühler's judgment that the objective cast of the *geisteswissenschaftlich* — psychological investigation of superindividual mental structures in itself does not allow for the individual subject, and hence demands a psychology of experience and action, is, in my opinion, sensible — on the foil that the *geisteswissenschaftliche Psychologie* mystified the material [*sinnlich-gegenständliche*] contents of consciousness as ideal entities independent from human activity. Furthermore, those critics failed to discuss that Bühler acknowledged a need not only for recognizing each of the three aspects but also for a unifying theory in homogeneous terms.

In Bühler's mind was the scheme of an empirical psychology as a "life science" [*lebenswissenschaftliche Psychologie*], reminiscent of Aristotle's conception of *psyché*, and based upon the findings of modern biology (cf. 1969). Arguing against the Cartesian notion of the extramundane character of psychic processes, which he regarded as prevalent in traditional-psychological views of the relationship between mind, body, and the world outside, it was Bühler's "intention to work out systematically the biological model-ideas [*Modellgedanken*] of psychology" (p. 181) as a preferable starting-point for "the new formation of theoretical psychology" (ibid.). Bühler assumed that organisms of whatever complexity, in adapting themselves to their respective environments, are purposively active — and in this sense psychic [*seelenhafte*] systems. Life is generally to be understood as a "whole-regulated [*ganzheitsgeregelt*] and meaningful [*sinnvoll*] process" (1978, p. 65) — pertinent key concepts being "mutual guidance" [*Steuerung*] of (intra-/inter-) organismic functions, "sign-character" [*Zeichenhaftigkeit*] of the physical and social world, and, with respect to experiences, "intentionality", for example, the directedness (reference) of such unique inward phenomena to objects

(contents) of the external reality (pp. 65-67). From this it follows that human psychology in particular, focuses on individuals acting in their natural-social life-space [*Lebensraum*] "according to inner needs and outer circumstance" (1969, p. 201). The scientific challenge is to give this dynamic harmony a psychological explanation on the basis of a biological system theory. It is by taking the "teleological view" (1978, p. 65) inherent in this approach, that, according to Bühler, the need for a consistent theoretical integration of the aspects of experience, meaningful behavior, and objective mental products is best met.

Consequently, in a systematic examination of "Spranger's new dualism" (1926), Bühler (1978, pp. 68-82, pp. 106-137, & pp. 141-161) disapproved of Spranger's antitheses between causal and intentional, explanatory and under-standing [*verstehende*], inductive und intuitive [*einsichtige*], elementary and structural, sense-indifferent [*sinnfreie*] and sense-related [*sinnbezogene*] psychology. Spranger unduly claimed the principle of structure, restricted to structures of value, for the exclusive perspective of a *geisteswissenschaftliche Psychologie*. By contrast, he attached the term *naturwissenschaftliche Psychologie* to a defunct physicalism. This dissection ignored a sense-related, teleological mode of thought that had been dominant for a long time already in biology as it starts out from the functionality of all organic wholes (struc-tures) (pp. 70-71).

In other words: as a "life scientist" Bühler rejected any methodological dichotomy — whether within or between scientific disciplines. In his "sketch" of a "new axiomatic system" (1969) psychology was bilaterally connected within the corpus of science: it completes the life sciences by determining the properties of human nature, and thus lays the foundation of the humanities.

Vygotsky's 'General-Scientific' Foundation of Psychology as a 'Real Science'

Vygotsky stated that the psychology of the normal adult, abnormal psychology, and animal psychology "compete for the rank of a basic theoreti-cal psychology that is central for a number of special disciplines" (1985a, p. 58). This rivalry is matched by the contrarieties of subjective psychology, psychoanalysis and behaviorism/reflexology. With their "primary abstrac-tions" of "immediate experience", the "unconscious", and "behaviour", these approaches categorize psychological matter disparately (p. 69). This methodological crisis of a spontaneously growing science with no distinct position between sociology and biology (p. 180) sets the cardinal task of a "General Psychology" (p. 57): that is, to develop an integrating concept of the common object of research and a binding principle of how to generalize knowledge of different branches (p. 67), and to overcome dilettantistic lin-guistic usage by creating a germane scientific terminology (p. 170).

Vygotsky rejected the mistake of psychology's heterogeneity as an excess of secondary disagreements within a basically realized unity of 'empirical' science. There is no hope for an easy integration *via* differential analyses of "purely empirical systems", as these do not exist and every psychology is grounded in some "metapsychology". According to Vygotsky, "empirical psychology" originated from an idealistic basis. Under the circumstances of urgent practical demands of society, it split into a descriptive, introspectionist psychology and a natural-scientific, explanatory approach, mainly represented by applied psychology (p. 199).

For Vygotsky this bifurcation of "two different, incompatible types of science ... existing and operating behind all conflicting currents" (p. 192) manifested an inescapable epistemological polarity. Thus, Husserl's thesis that, opposite to physical nature, "there is no distinction between appearance and essence in the psychic sphere" (1965, p. 35), precisely presents the formula of psychological idealism. Following Feuerbach (1971, p.127), by contrast, the epistemological problem is set, within the ontological frame of materialist monism, also for the psychic realm. Only such a transcendence from immediate appearance to objective process and substance of experience leads to scientific psychology.

To back up this central thesis, Vygotsky demonstrated that 'third way' positions such as Gestalt psychology or Stern's Personalism are governed (though not unequivocally) by either side of the essential polarity (p. 212), and that the emerging Marxist psychology has been a wasted effort to reconcile both conflicting guidelines (p. 247). "The unity of a science is determined by the unity of the standpoint with respect to the subject matter."(p. 160). The necessary general psychology will have to take sides with the materialists, rejecting the reification of introspectable immediate experience. This excludes eclectic synthesis. Under this cover Vygotsky comprehended the crude annexation of elements of a foreign system irrespective of its key idea, as well as the more sophisticated, yet arbitrary operations towards an epistemological and conceptual convergence that homogenize disparate systems through distorting adaptations of their respective methods and contents. As a case in the latter point Vygotsky referred to Luria's (1925) Freudo-Marxist synthesis, which simply dismissed the irreconcilable pan-sexualism as allegedly irrelevant — and hence deprived Freud's theory constitutive element. In order to appropriate a foreign system critically, one has to exceed it and develop proper principles and concepts of one's own (p. 158).

Vygotsky criticized the Russian Marxist eclecticists, who compensated for the lack of a germane psychological methodology by abstractly utilizing basic tenets of Marxist philosophy. A well-conceived "dependency" on dialectics as "universal science" (p. 252), however, involves a concrete connexion of the philosophical theory with the particularities of different scientific branches. Vygotsky called this mediation a "philosophy of the special dis-

ciplines" (p. 84), as it elaborates the foundational concepts and principles that delimit a scientific field as a distinctive category of existence and hence deals with ontological universals. It does so *via* a critique of the historical forms of scientific knowing. General science thus proceeds with the methodological (self-) reflexion of the sub-/disciplines and generalizes their findings in relation to other sub- and neighbouring disciplines. As a "theory of psychological materialism" the "general science" of psychology determines the basic dimensions of "psychological" problems. This is a prerequisite to specify empirically objective regularities of the phenomena which the controverted doctrines dealt with, and, hence, to evaluate the relative significance of the latter (pp. 104-129). For Vygotsky it was out of the question to construe the 'general' and the 'concrete' psychology as differing essentially with respect to the object, intention and methods of research. General psychology, aptly labeled as the "dialectics of psychology" (p. 252), opens up at the same time a dialectic comprehension of psychic processes. Vygotsky regarded this as analogous to the unity of a critique of science and positive research realised in Marx's *Capital*, where the dialectical method is applied to both the empirical data of economic development and the history of political economy (as factual and meta-factual levels of scientific study).

Vygotsky's text contains few methodological hints at the concrete psychology which later on took shape in his "cultural-historical approach". As his polemics against the substitution of intuiting essence [*Wesensschau*] for inductive analysis (pp. 129-153, pp. 224-257) indicates, however, his methodical views were unhampered by a restrictive understanding of analytical procedures (that is, a schematic image of the form of experimental science, that had been and was then being projected upon academic psychology by neo-/positivistic philosophy of science). This and his exposition of the role of interpretation in the sciences and the humanities alike implied, besides its anti-empiricist direction, a strong anti-dualistic argument for psychology as a coherent "natural", that is, "real science" [*Wissenschaft vom Realen*].

Two Metascientific Argumentations for Monistic Unification

Vygotsky as well as Bühler were devoted to the obviously unfinished task of overcoming the disorienting idealistic and mechanistic doctrines by means of a basic revision in the cognitive structure of psychology. Both traced the heterogeneous psychological views to the central philosophical problem of the psycho-physical relationship — and both agreed in blaming the Descartes-Locke conception of mind for modern psychology's being captured in the impasse of psycho-physical interactionism, parallelism, or identity conceptions. By contrast, both these critics resolutely dismissed this handicap and, instead continued Feuerbach's paving the way from German idealism to Marx (cf. Vygotsky, 1985b) or Aristotle's materialistic, "biological" tendency (cf. Bühler, 1969).

As approaches of a field-specific meta-science adhering to empirical cognition, Vygotsky's "General" as well as Bühler's "Axiomatic psychology" outlined a nonempiricist program of empirical science. Their conceptions of a permeable hierarchy of research levels: from philosophy down to practice-related psychological theorization, mediated by the definition of its categories and methodological principles meet the topics of the present moment. Particularly Vygotsky's positions on the interrelation between theory and empirical data, on the fictitious objectivity of knowledge anchored in pure observation, and on the constructivity of cognition as activity of reflexion impress as an anticipation of the later (self) criticism of neopositivism. Disregarding for our purpose the difference between dialectical-materialistic and critical-realistic epistemology, Vygotsky's and Bühler's arguments deserve attention as sound refutations of both the traditional mechanicism of reflexion with its naive 'correspondence' theory of truth and the solipsism of modern constructionism reducing truth to consensus.

A key to both foundational works is their 'interdisciplinary' orientation. This bore no similarity to the vogue of dissolving a unique 'psychological' cognition either in an indifferent mishmash or *via* a reduction to some vulgar physiologism or sociologism or, more prominent these days, to computer metaphors. Rather they pursued a transformation of the genuinely 'psychological' key concepts respecting the findings of other sciences that are fundamental for the understanding of psychic developmental processes.

As regards the pluralism-monism issue, they were in agreement. Assuming that the traditional doctrines have hypostatized and, subsequently, distorted ultimate moments of a psychophysical "complex-reality" [*Komplexrealität*], Bühler aimed at a critical reinterpretation of such antinomic concepts — a transformation, that would lead to a conceptually homogeneous "final object" [*Endgegenstand*]. Bühler's complementarity of obligatory aspects differed basically from both a naive syncretism of heterogeneous theoretical elements and a sophisticated 'anything-goes liberalism' that is content with giving conventionalistic reasons for optional ways of looking and technical languages. Moreover, it cannot be identified with, for example, Koch's proposal (1976) to treat the manifold psychological phenomena in various "psychological studies". As Koch remains silent about "sublating" such irreducible aspects and hence about the connection of viewpoints which, by their very nature, cannot fuse, his position, *nolens volens*, favors the doctrine of incommensurability. By contrast, Bühler's idea of unity as "unitas multiplex" (1932, pp. 95-96) is to be legitimized from the commitment of materialist monism to counteract the absolutizing of isolated aspects by accounting for the qualitative multifariousness and historical layers of reality in their interrelations.

Formally, Vygotsky's incrimination of any sort of theoretical "bilingualism" [*Doppelsprachigkeit*] as an "indication of equivocality in thinking"

and his demand for a unitary standpoint had its counterpart in Bühler's teleological perspective as the supposed unifying principle and remedy for defective methodological antitheses.

Bühler's attempt to conceptualize in homogeneous terms the correlation of structure [*Gebilde*] and act(ion) [*Handeln*], tackled a central topic, that mainstream-psychology had either dismissed altogether or missed in its methodology of variable-analysis. Admittedly, this interdependency remained an open problem in Bühler's own theory of purposive action embedded in an overall objective semantics. Yet, his formulation of the problem appears up-to-date, certainly more sensible than many present-day variants of a 'telic psychology', as it bears some resemblance to the position of integrating causes and reasons within one conceptual framework instead of playing off causalism against intentionalism. That position is assumed in the contemporary discussion of analytical philosophy and hermeneutics about a theory of human action, and it is fully justified viewed from dialectic-materialistic determinism.

Let me touch upon a central intrinsic limitation of Bühler's approach. Bühler opposed to the monadic individualism of traditional psychology a systemtheoretical view of the individuals' psychophysical correspondence to their life-space. Lacking a historical method, however, he could explicate this irreducible subject-object unity of psychological analysis only from an abstract-functional, holistic viewpoint, that is, a "psychophysical Gestalt principle" (1960, p. 84). He missed an integrative developmental conception of the unity of consciousness and activity, of individual subjectivity and trans-individual objectivity, and hence an understanding of the societal mediation of consciousness. Bühler's very intent to prevent a dualistic disintegration of the definition of "the psychological" by incorporating the specific "meaning-fulness" [*Sinnhaltigkeit*] of human experience in the objective teleonomy of life, requires, materialistically, a methodology that connects system analysis and developmental thinking. This was the original element in Vygotsky's causal explanation of psychic processes in the overall developmental context of natural- and social history. Philosophically, his position combined the notion of the unity of the world, grounded in its materiality, and the principle of reflexion, and thus opened a monist solution to the psychophysical problem with its immanent psychophysiological, epistemological and practical aspects.

Vygotsky's as well as Bühler's analyses of crisis are qualified as what Madsen (1987, p. 165) established as the concern of theoretical psychology: that is, as "metascientific study of psychological theories and theory-problems". This is one of the eminent historical lessons for theoretical psychology to learn from both scholars: that Marxist and non-Marxist positions alike, relying on specialty knowledge as well as on a coherent philosophical world view, encourage the development of non-dogmatic,

testable alternatives of a monistic foundation for our science to both a reductionist *pars pro toto* universalism and eclecticistic theoretical pluralism.

References

Allport, G. W. (1966). An Appreciation of Die Krise der Psychologie by Karl Bühler. In J.F.T. Bugental (Ed.), Symposium on Karl Bühler's Contributions to Psychology [Special issue]. *Journal of Genetic Psychology, 75*, 201-204.

Bühler, K. (1932). Das Ganze der Sprachtheorie, ihr Aufbau und ihre Teile. *Bericht über den XII. Kongreß der DGfP in Hamburg 1931* (pp. 95-122). Jena: Fischer.

Bühler, K. (1933). Die Axiomatik der Sprachwissenschaften. *Kant-Studien, 38*, 19-90.

Bühler, K. (1960). *Das Gestaltprinzip im Leben der Menschen und der Tiere*. Bern, Stuttgart: Huber.

Bühler, K. (1965). *Sprachtheorie. Die Darstellungsfunktion der Sprache* (2nd ed.). Stuttgart: Fischer. (Original edition 1934).

Bühler, K. (1969). Der Modellgedanke in der Psychologie. In K. Bühler, *Die Uhren der Lebewesen und Fragmente aus dem Nachlaß* (G. Lebzeltern, Ed., pp. 169-220). Wien: Kommissionsverlag der Österreichischen Akademie der Wissenschaften.

Bühler, K. (1978). *Die Krise der Psychologie*. Frankfurt/M., Berlin: Ullstein. (Original edition 1927).

Feuerbach, L. (1971). Wider den Dualismus von Leib und Seele, Fleisch und Geist. In *Gesammelte Werke*. (W. Schuffenhauer, Ed., Vol. 10, pp. 122-150) Berlin/GDR: Akademie. (Original edition 1846).

Giorgi, A. (1976). Phenomenology and the Foundations of Psychology. In W. Arnold (Ed.), *Nebraska Symposium on Motivation 1975* (Vol. 23, pp. 281-348). Lincoln, London: University of Nebraska Press.

Herrmann, Th. (1987). Die nomologische Psychologie und das intentio-nale Denkmuster. In W. Maiers & M. Markard (Eds.), *Kritische Psychologie als Subjektwissenschaft. Klaus Holzkamp zum 60. Geburtstag* (pp. 106-119). Frankfurt/M.: Campus.

Husserl, E. (1965). *Philosophie als strenge Wissenschaft*. Frankfurt/M.: Klostermann. (Original edition 1910/11).

IJzendoorn, M. H. van, & Veer, R. van der (1984). *Main Currents of Critical Psychology. Vygotsky, Holzkamp, Riegel*. New York: Irvington.

Koch, S. (1976). Language Communities, Search Cells, and the Psychological Studies. In W. Arnold (Ed.), *Nebraska Symposium on Motivation 1975* (Vol. 23, pp. 477-559). Lincoln, London: University of Nebraska Press.

Leontiev, A. N. (1985). Einleitung: Der Schaffensweg Wygotskis. In L.S. Wygotski, *Ausgewählte Schriften* (Vol. 1, pp. 9-55). Köln: Pahl-Rugenstein.

Luria, A. R. (1979). *The Making of Mind. A Personal Account of Soviet Psychology* (M. Cole & Sh. Cole, Eds.), Cambridge, MA, London.

Madsen, K. (1987). Theoretical Psychology: a Definition and Systematic Classification. In W.J. Baker, M.E. Hyland, H.V.Rappard, & A.W.Staats (Eds.), *Current Issues in Theoretical Psychology* (pp. 165-174). Amsterdam: North Holland.

Maiers, W. (1985). Zur Erkenntnisfunktion wissenschaftshistorischer Analyse für die Gegenstandsbestimmung in der Psychologie. In K. H. Braun & K. Holzkamp (Eds.), *Subjektivität als Problem psychologischer Methodik* (pp. 315-363). Frankfurt/M.: Campus.

Maiers, W. (1988a). Sechzig Jahre Krise der Psychologie. *Forum Kritische Psychologie, 21*, 23-82.

Maiers, W. (1988b). Has Psychology Exaggerated its 'Natural Scientific Character'? Remarks Concerning an Empirical Topic and a Methodological Desideratum of 'Theoretical Psychology'. In W. J. Baker, L. P. Mos, H. V. Rappard, & H. J. Stam (Eds), *Recent Trends in Theoretical Psychology* (pp. 133-143). New York: Springer-Verlag.

Maiers, W. (1989). Historisch-materialistische Erkenntniskritik und positive Weiterentwicklung der Psychologie. Zur Funktion der Psychologiegeschichte. In A. Schorr & G. Wehner (Eds.), *Bericht über die 1. Fachtagung für Geschichte der Psychologie, Eichstätt 1988*. Göttingen: Hogrefe (in print).

Maiers, W., & Markard, M. (Eds.) (1987). *Kritische Psychologie als Subjektwissenschaft. Klaus Holzkamp zum 60. Geburtstag.* Frankfurt/M.: Campus.

Rubinstein, S. L. (1979). Probleme der Psychologie in den Arbeiten von Karl Marx. In S.L. Rubinstein, *Probleme der Allgemeinen Psychologie* (pp. 11-32). Berlin/GDR. (Original edition 1934).

Rückriem, G. (1986) Rezeption der Tätigkeitstheorie in der Bundesrepublik Deutschland und Berlin (West). In *Survey. 1. Internationaler Kongreß zur Tätigkeitstheorie, Berlin (West) 1986* (pp. 205-259). Berlin/W.: System Druck.

Spranger, E. (1926). Die Frage nach der Einheit der Psychologie. *Sitzungsberichte der Preußischen Akademie der Wissenschaften, 26*, 172-199.

Tolman, C. W. (1987). Zur Vorgeschichte der historischen Herangehensweise in der bürgerlichen Psychologie. In W. Maiers & M. Markard (Eds.), *Kritische Psychologie als Subjektwissenschaft. Klaus Holzkamp zum 60. Geburtstag* (pp. 228-240). Frankfurt/M.: Campus.

Tolman, C. W. (1988). Theoretical Unification in Psychology: A Materialist Perspective. In W.J. Baker, L. P. Mos, H. V. Rappard, & H. J. Stam (Eds.), *Recent Trends in Theoretical Psychology* (pp. 29-36). New York: Springer.

Vygotsky, L. (1985a). Die Krise der Psychologie in ihrer historischen Bedeutung. In L. Wygotski, *Ausgewählte Schriften* (Vol. 1, pp. 57-278). Köln: Pahl-Rugenstein.

Vygotsky, L. (1985b). Spinoza und seine Lehre von den Gefühlen im Lichte der heutigen Psychoneurologie. In L. Wygotski, *Ausgewählte Schriften* (Vol. 1, pp. 363-382). Köln: Pahl-Rugenstein. (Original edition 1933).

Wellek, A. (1982). Der Rückfall in die Methodenkrise der Psychologie und ihre Überwindung. In H. Balmer (Ed.), *Geschichte der Psychologie* [Kindlers 'Psychologie des 20. Jahrhunderts'] (Vol. 1, pp. 17-42). Weinheim, Basel: Beltz. (Original edition 1958).

Yaroshevsky, M. G. (1975). *Psychologie im 20. Jahrhundert*. Berlin/GDR: Volk und Wissen.

DEMYSTIFYING VYGOTSKY'S CONCEPT OF THE ZONE OF PROXIMAL DEVELOPMENT

René van der Veer

SUMMARY: In this paper it is shown that the Soviet psychologist Vygotsky's well-known concept of the zone of proximal development arose in the practice of intelligence testing. More specifically, Vygotsky used the concept to explain the phenomenon of regression towards the mean of IQ scores. It is claimed that this way of thinking about the zone of proximal development contradicts its current Western interpretations. In addition, it is argued that Vygotsky's original interpretation had several unfortunate implications that conflicted with his own larger body of writings as well.

In the final years of his life Vygotsky returned to the problems of teaching in school, now — unlike in his earlier writings (Vygotsky, 1922, 1926) — focussing on the problem of the relation between school teaching and cognitive development. His approach of this problem was deeply rooted in the paedological writings of the time which had evolved while lecturing at the Herzen Pedagogical Institute in Leningrad. The key concept in this period became the 'zone of proximal development'.

Western researchers have analyzed this concept making use of Vygotsky (1962) and Vygotsky (1978). Unfortunately, the first book only gives a rather global discussion of the idea, while the latter one is an unfortunate compilation in which all references to the historical backgrounds of the concept have been omitted (see van der Veer & Valsiner, in press). None of the current western or Soviet interpretations of the concept of the zone of proximal development (e.g., Rogoff & Wertsch, 1984) are based on an analysis of Vygotsky's original writings. Meanwhile, a careful reading of Vygotsky's talks on this subject — published in Vygotsky (1935) — sheds a rather new light on his interpretation of the concept. Historically the concept of the zone of proximal development was tightly connected with the practice of intelligence testing and contradicted various aspects of Vygotsky's general theoretical framework.

The Zone of Proximal Development

Vygotsky's most detailed description of the concept of the zone of proximal development can be found in the stenogram of a lecture delivered at the Bubnov Pedagogical Institute on December 23, 1933 (posthumously published in Vygotsky, 1935). Vygotsky mentioned that in former times researchers used to think that one cannot start teaching children unless they have reached a certain level of development. Much effort went into estab-

lishing the lowest possible boundaries from which the teaching of various school subjects might be started. The way to establish these lower boundaries was to ask the child to independently solve some specified task or test. We now know, however, Vygotsky argued, that there is also an upper boundary, that is, we know that optimal periods exist for the learning of an intellectual skill. Paedological research had demonstrated that we cannot wait forever until the required intellectual functions have matured enough for successful teaching to take place. The mother tongue, for example, is best learned at a very early age, while mathematics, probably, should be learned considerably later. Is there a way to establish the optimal periods for learning various intellectual skills? Can we establish a child's teachability in a certain domain? To answer these questions Vygotsky turned to the domain of intelligence testing and the concept of the zone of proximal development.

He discussed this concept in the context of intelligence testing at the entrance of elementary school and against the background of the often observed phenomenon of 'regression towards the mean' (to put it anachronistically). Vygotsky reminded his audience of the general practice to test all children before they entered elementary school. He mentioned the fact that IQ scores had been shown to predict performance in school with high accuracy and was rather positive about the practice of referring children to different categories on the basis of their IQ scores:

> This rule is now used by the school all over the world, it contains the fundamental wisdom of all paedological investigations carried through at the entrance of school (Vygotsky, 1935, p. 37).

Unfortunately, research done by Terman (1919), Burt (1921), and Blonsky (1927) had pointed out a mysterious phenomenon: children with high initial IQ tended to lose and children with low initial IQ tend to gain IQ points in the school period, leaving their rank order unchanged. How should we interpret this phenomenon? Vygotsky was inclined to explain these findings by suggesting that children with low entrance IQ scores profited more from schooling than children with high entrance IQ scores: relatively speaking, then, the elementary school was more successful for the first group. But why would this be the case? Do children with high initial IQ scores gain little, because school is badly adjusted to their wants? To answer these questions Vygotsky brought in the concept of the zone of proximal development.

> In the investigation of the cognitive development of the child it is accepted to think that indicative of the child's intellect is only that, which the child can do himself. We give the child a series of tests, a series of tasks of varying difficulty, and by the way and the degree of difficulty up to which the child can solve the task we judge about the greater or lesser development of his intellect. It is accepted to think that indicative of the degree of development of the child's intellect is the independent, unassisted solving of the task by the child. If we would ask him leading questions or demonstrated him how to solve the task and the child after the demonstration solved the task, or if the teacher started to solve the task and the child finished it or solved it in cooperation with other children, in short, if the child diverged however so

much from the independent solving of the task, then such a solution would
already not be indicative of the development of his intellect (Vygotsky, 1935,
p. 41).

At least, this is what researchers tended to think for years, Vygotsky
argued. He, evidently, did not agree and proposed exactly to give the child
hints and prompts to see how far this would lead the child. He mentioned that
'various researchers' had used different ways to do this. In this way it had been
found that children with the same mental age — as established in the tradi-
tional, independent way — were able to solve problems up to different mental
age levels. We, therefore, have little reason to say that they have the same
mental age after all: using the hints and prompts some children solved tasks
four years above their independent performance, while others hardly profited
from the help offered. The difference between independent performance and
aided performance, thus, seems to be characteristic of the child.

The zone of proximal development of the child is the distance between his
actual development, determined with the help of independently solved tasks,
and the level of the potential development of the child, determined with the
help of tasks solved by the child under the guidance of adults and in
cooperation with his more intelligent partners (Vygotsky, 1935, p. 42).

The level of actual, independent development, Vygotsky maintained, was
characteristic of the intellectual skills the child already mastered: it repre-
sented the already matured functions, the results of yesterday. The perfor-
mance of children co-operating with more knowledgeable others, however,
was characteristic of their future development: it revealed the results of
tomorrow. To substantiate this claim he referred to the results found by 'the
American investigator MacCarthy' with regard to the preschool age period.
MacCarthy had shown that 3-5 year old children can perform some tasks
independently and some others only under the guidance or in co-operation
with some other person. These latter tasks the children were able to perform
independently when they were 5-7 years old. Therefore, Vygotsky concluded,

... we can say what will happen with this child between 5 and 7 years, other
conditions of development staying the same ... In this way the investigation
of the zone of proximal development became one of the strongest instru-
ments of paedological investigations, allowing [us] to considerably enhance
the effectivity, utility, and fruitfulness, the application of diagnostics of the
intellectual development to the solution of the tasks raised by pedagogics,
[and] the school (Vygotsky, 1935, p. 43).

Having briefly mentioned MacCarthy's findings Vygotsky returned to the
problem of the relative degree of success of different IQ groups in school.
Suppose, he argued, we have one group of children with high IQ scores and
another with low scores. Suppose, further, that these groups can be sub-
divided into two subgroups with a proximal zone of, respectively, two or three
years of mental age. We, then, have four possible combinations: high IQ, large
zone; high IQ, small zone; low IQ, large zone; and low IQ, small zone (see
Table 1).

Table 1

Children with Different IQ Scores and Different Zones of Proximal Development

1. High IQ	Large Zone
2. High IQ	Small Zone
3. Low IQ	Large Zone
4. Low IQ	Small Zone

Vygotsky claimed to have found in a large scale empirical investigation that the dynamics of intellectual development and the degree of relative success are comparable for the first and third, and for the second and fourth groups. This probably means that his findings indicated that children with similar zones of proximal development gained or lost similar quantities of IQ points. The zone of proximal development, therefore, was more important for and predictive of the child's intellectual development than the IQ score as traditionally established.

To show the intricacy of the phenomena, Vygotsky brought in yet another complicating factor. Suppose, he reasoned, that we have a group A of either illiterate children forming part of a group of illiterate children or literate children forming part of a group of literate children. Further, suppose we have another group B of either literate children forming part of a bigger group of illiterate children or illiterate children forming part of a group of literate children (the problem of illiterate children was a very real problem in the Soviet Union at the time). These children can have various IQ scores, which leads us to Table 2.

Which children are most comparable with regard to the dynamics of intellectual development and the relative school success? Vygotsky again referred to empirical investigations performed under his guidance and stated that

> The investigation shows, and this time much more significant and telling than in the case of the zone of proximal development, that the similarity appears considerably greater between the first and third, second and fourth, than between the first and second and the third and fourth groups. This means that for the dynamics of the intellectual development in school and for the progress of the child in the course of school instruction [the] determining [factor] is not so much the size of the IQ in itself, that is, the level of development of the present day, as the relation of the level of preparation and development of the child to the level of the demands made by the school. This last quantity — the level of demands made by the school — in paedology one has now proposed to call the ideal mental age (Vygotsky, 1935, p. 46).

Table 2

Children with Different IQ Scores and Different Literacy Background

1. High IQ	A (homogeneous (il)literacy)
2. High IQ	B (mixed literacy)
3. Low IQ	A (homogeneous (il)literacy)
4. Low IQ	B (mixed literacy)

Vygotsky was inclined to consider this concept of 'the ideal mental age' very important. He mentioned that different researchers had tried to establish the ideal mental age for various school classes. Presumably, then, these researchers tried to deduce from the demands made in a specific class which mental age was required for successful performance in this class. This required mental age had to stand in some optimal relation to the various mental ages of the children attending the class. Vygotsky mentioned that the relation of the ideal mental age of a given class to the real mental age of the children in that class was the most sensitive measure established by paedologists at the time. If these respective levels differed too much — as in the case of an illiterate child forming part of a literate class or a literate child forming part of an illiterate class — children were expected to gain little. The same held when the divergence was too small: instruction should call into life, drag behind itself, organize development. But how was the optimal distance between real and ideal mental age to be established, what are the optimal conditions for intellectual progress? Vygotsky mentioned that various attempts — using units for the child's mental age, program materials, and school years — had been made, but that to him most convincing were some small, individual case studies. These investigations — carried out by his collaborators — demonstrated that the optimal difference between ideal mental age level and real mental age level completely coincided with the zone of proximal development of the child (Vygotsky, 1935, p. 48). If the child has a zone of proximal development of two mental age years, then the ideal mental age of his class should be two years above the child's mental age as independently measured.

> In this way the analysis of the zone of proximal development becomes not only a magnificent means for the prognosis of the fate of the intellectual development and the dynamics of the relative success in school, but also a fine means for the composition of classes ... the level of intellectual development of the child, his zone of proximal development, the ideal [mental] age of the class, and the relation between the ideal [mental] age of the class and

> the zone of proximal development ... [form] the best means to solve the
> problem of the composition of classes (Vygotsky, 1935, p. 49).

Explaining Regression Towards the Mean

Having described the intricacies of class composition and the use of the concept of the zone of proximal development in this domain, Vygotsky returned to the problem that formed the focus of his talk at the Bubnov Pedagogical Institute. How can we, then, explain the phenomenon of 'regression towards the mean'? Is it a general law that children with high initial IQ tend to lose, while children with high initial IQ tend to boost their scores? To this question Vygotsky answered in the negative arguing that we should take into consideration the composition of the school class. But why do we still find the phenomenon as a statistical law? To explain this he first remarked that the IQ is a rather opaque instrument, it is "a symptom, an indication". The problem is that we do not know what an IQ score indicates and how it evolved. To stick to symptoms in medicine might prove lethal, Vygotsky told his audience in a rather personal passage: some coughs indicate influenza, others tuberculosis! It would be wrong, therefore, to formulate the general law that coughs should be treated in such and such a way. The same holds true for IQ scores: they reflect very different backgrounds.

Why, then, do the children with high initial IQ scores tend to lose IQ points in the four years of elementary school? The explanation was, according to Vygotsky, that the majority of the children coming to school with high initial IQ were not really more gifted, but grew up under more favorable circumstances (Vygotsky, 1935, p. 51). The reason they excelled was that they came from a privileged background. They had plenty of books and toys at their disposal, their parents read stories to them, and so forth. The Binet tests in use, Vygotsky remarked, were in essence designed to test knowledge resulting from favorable home circumstances. It was no wonder, therefore, that these children obtained high scores. However, they tended to lose their lead soon, because

> ... they get them at the cost of the zone of proximal development, that is,
> they run through their zone of proximal development earlier, and, therefore,
> they are left with a relatively small zone of development, as they have to some
> extent already used it. According to the data of my investigation in two
> schools there were more than 57% of these children (Vygotsky, 1935, p. 52).

In essence, then, Vygotsky explained the phenomenon of 'regression towards the mean' by the equalizing, levelling effect of schooling. Because the circumstances at school are more equal children from disadvantaged home backgrounds will gain, while those from privileged homes will tend to lose.

Implications

It is quite clear from several of Vygotsky's remarks — for example, his claim that the zone of proximal development is "revealing the results of tomorrow"; that establishing this zone is "a magnificent means of prognosis"; and that "we can say what will happen with this child ... other conditions ... staying the same" — that he considered the measurement of the zone of proximal development to be a means to predict the child's future IQ development. In essence, he suggested two quantities be measured — independent performance and aided, joint performance — and claimed that the future development of the former was fully determined by the latter. Children were able to profit from the jointly performed tasks, because of their singular ability to imitate the activities of their more able partners. Referring to MacCarthy, Vygotsky maintained that activities that can be imitated by the child will be independently performed in the near future: "Research shows the strictly genetic lawfulness between that which the child can imitate and his mental development" (Vygotsky, 1934, p. 264). To Vygotsky, then, the dynamics of the child's independently reached IQ scores were fully predictable on the basis of the jointly reached IQ scores. This peculiar view can be pictured in the following way (see Figure 1).

To be able to predict the child's future cognitive development the investigator should (a) establish the child's independently reached IQ score. In Figure 1 the child is 4 years old and reaches an independent score of 4.5 mental age years (measurement A). The child, therefore, is scoring slightly above the average performance of his age group. The next step is to (b) establish the child's score in joint performance, that is, while the child can make use of various hints, prompts, is shown part of the solution, and so forth. Under these circumstances the child in our example is able to solve the tasks up to a mental age of 7 years old (measurement B). The child, thus, has a zone of proximal development of 2.5 mental age years. We now can predict, according to Vygotsky, that in the next 2.5 chronological years our child's independent performance will become progressively better until it has reached the level of the joint performance measured at the chronological age of 4. This level will be reached after 2 1/2 years have passed.

The resulting view of cognitive development is rather odd for several, interconnected reasons. First, because Vygotsky at least suggested that cognitive development proceeds in a linear fashion. A difference of two mental age years between independent and joint performance was expected to have disappeared after two chronological years. This view would be in sharp contradiction with many of Vygotsky's own statements about the dialectics of child development. Second, the dynamics of the child's IQ development were pictured by Vygotsky against the background of a static environment. The environment was brought in in the form of the measurement of the aided or joint performance at one specific point of time and then was disregarded.

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     7.0     ●B      ●       ●       ●       ●       ●

     6.5                                             ●

Mental   6.0                                 ●

 Age     5.5                         ●

     5.0             ●

     4.5     ●A

          ───────────────────────────────────────────────
            4.0     4.5     5.0     5.5     6.0     6.5
                         Chronological Age
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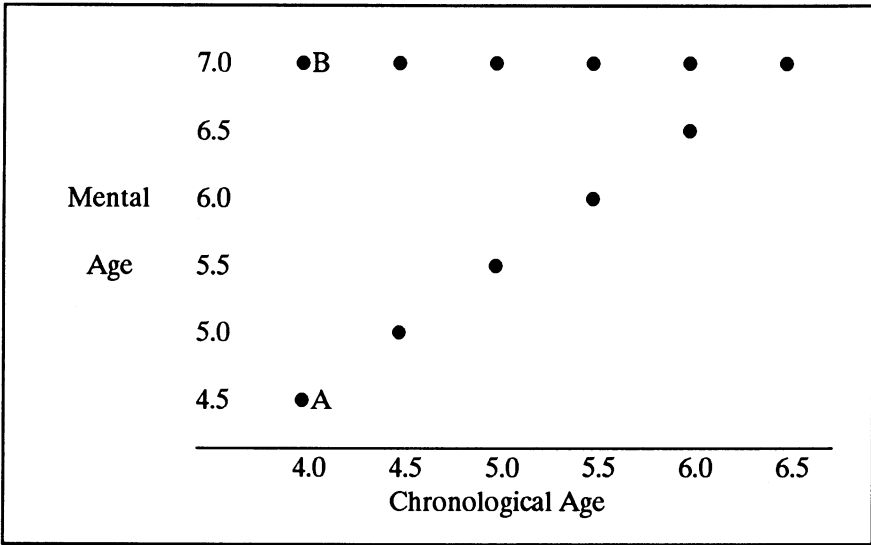
**Figure 1. Predicting the child's future IQ scores on the basis
of the measurement of independent and joint performance.**

There is, of course, no reason to believe that the child's aided performance at
the chronological age of 5 years old would be the same as the one established
at age 4. There is no reason, therefore, to believe that some children will have
'spent' their zone of proximal development, as Vygotsky clearly suggested.
On the contrary, because the environment is still present — in the form of
adults playing with the child and tutoring him — there is every reason to
believe that a second measurement at age 5 would give a higher joint perfor-
mance score. This new zone of proximal development would then possibly
predict another independent IQ score for the child at age 7. The examples
Vygotsky gave to demonstrate the use of the zone of proximal development
suggest that he conceived of the environment as a static background of the
dynamically developing child. This, again, was in sharp contradiction with the
views he espoused in his various other publications. Third, Vygotsky seemed
to suggest that the independent performance of a child will have as its 'ceiling'
the joint performance. This may be plausible in the case of intelligence tests
presented to very young children, but formulated as a general rule it suggests
the unfortunate idea that children can never outperform their adult partners,
or — to put it even more generally — that the next generation can never
transcend the cognitive possibilities of the former one. In itself, this idea
looms large in any conception that like cultural historical theory emphasizes
the transfer of cultural knowledge from one generation to the next, but in this
particular case it is conspicuously present.

Conclusions

Vygotsky's own view of the zone of proximal development was unfortunate and at variance with the general flavor of his thinking. The concept arose in the narrow practice of intelligence testing on the basis of the research done in the field of paedology and was only later considered to be a general law describing the relation between instruction and cognitive development. The historical background of the concept of the zone of proximal development is little known (see Van der Veer & Valsiner, in press) and deserves the attention of all researchers seriously devoted to the study of the Soviet scientist's work.

References

Blonsky, P. P. (1927). *Psikhologicheskie ocherki.* Moscow: Novaya Moskva.

Burt, C. (1921). *Mental and scholastic tests.* London: Staples.

Rogoff, B., & Wertsch, J. V. (Eds.) (1984). *Children's learning in "the zone of proximal development".* San Francisco: Jossey-Bass.

Terman, L. M. (1919). *The intelligence of school children.* Boston: Houghton Mifflin.

Veer, R. van der, & Valsiner, J. (in press). *Lev Vygotsky. His life and work.* Oxford: Basil Blackwell.

Vygotsky, L. S. (1922). *O metodakh prepodovanija khudozhestvennoj literatury v shkolakh II stupeni.* Unpublished manuscript.

Vygotsky, L. S. (1926). *Pedagogicheskaja psikhologija.* Moscow: Rabotnik Prosveshchenija.

Vygotsky, L. S. (1934). Problema vozrasta. In L. S. Vygotskij (Ed.), *Sobranie sochinenij. Tom 4. Detskaja psikhologija* (pp. 244-268). Moscow: Pedagogika.

Vygotsky, L. S. (1935). *Umstvennoe razvitie detej v processe obuchenija.* Moscow-Leningrad: Uchpedgiz.

Vygotsky, L. S. (1962). *Thought and language.* Cambridge, MA: The M.I.T. Press.

Vygotsky, L. S. (1978). *Mind in society.* Cambridge, MA: Harvard University Press.

PERSONAL AND SOCIAL PRECONCEPTIONS IN THE FORMATION OF PSYCHO/SOCIOLOGICAL THEORY: FREUD'S SEDUCTION HYPOTHESIS AND THE CASE OF CHILD SEXUAL ABUSE

J.Hans Droste

SUMMARY: In this paper I will argue that we, as scientists, are influenced by various factors which lie outside the paradigm of objectivity. Personal as well as social factors influence the formation of psycho/sociological theory. To illustrate some of these influences I will discuss the possible factors which contributed to Freud's rejection of the seduction hypothesis. His ideas at the time of their presentation were unacceptable, but now, almost a century later, changes have occurred within society which have made a more open discussion possible. The factors which might have contributed to this openness and which in effect have influenced our insights concerning child sexual abuse will be reviewed.

In 1896 Sigmund Freud presented a controversial hypothesis on the origins of hysteria. He postulated that hysterical symptoms were the product of a traumatic sexual experience in early childhood. The memories associated with sexual abuse had been "repressed" and these "unconscious memories" were capable of creating and maintaining the various symptoms later in life. Sexuality, according to Freud, preceded hysteria and the sexual elements so often found among hysterical patients were not grounded in some hereditary female disease, but were the result of sexual abuse in childhood. Hysteria was in other words a symptom of child sexual abuse (Freud, 1896). Freud presented these ideas on April 21, 1896, to his colleagues at the University of Vienna. More than a year later, Freud renounced his ideas in a letter addressed to Wilhelm Fliess (Masson, 1985, letter, September 21, 1897). It was not until 1906 that he made this change public.

Several examples of externalist explanations for his renouncement can be found in the literature. Masson (1984), the author of *The assault on truth* and well-known for his allegation that Freud intentionally had suppressed the seduction hypothesis, compares this period in the life of Freud with his cocaine period when he was rejected by the academic profession. Now, with his new ideas concerning hysteria and sexuality, Freud again faced isolation. It was only Wilhelm Fliess, Freud's close friend, who listened to Freud's 'extraordinary ideas'. From this point of view it is not surprising that Freud eventually abandoned his seduction hypothesis. When one is constantly faced with rejection and solitude within the academic profession, such as Freud was, one eventually starts to doubt the validity of one's ideas, not because they are incorrect, but because cognition is not 'all rational'. Scientists also have

feelings and emotions which interact with their scientific work. A good example is the publication of the seduction hypothesis. When Freud presented his ideas in a lecture to his colleagues, he was not planning to publish these ideas in the short term. After his ideas received a cold reception, he wrote to Fliess that "they can go to hell!" (Masson, 1985, letter, April 26, 1896). One week later he wrote: "I am as isolated as you could wish me to be: the word has been given out to abandon me, and a void is forming around me" (Masson, 1985, letter, May 4, 1896). Ten days after this letter, the *Wiener klinische Wochenschrift*, the university journal which normally gave a summary or discussion of the lecture, only mentioned the title of Freud's lecture (Masson, 1984, p.6). Freud wrote to Fliess: "In defiance of my colleagues I have written down in full my lecture on the aetiology of hysteria" (Masson, 1985, letter, May 30, 1896). What this example shows is that one can be urged to publish one's ideas or even reject them because hopes, fears and even anger influence the course of one's actions and ideas.

Another personal factor which influenced Freud's scientific judgement and made it impossible for him to uphold the seduction hypothesis, was his incapability to accuse his own father of sexual abuse. In the famous letter to Fliess in which Freud writes that he no longer believes in the seduction hypothesis, Freud writes: "Then the surprise that in all cases, the father, not excluding my own, had to be accused of being perverse" (Masson, 1985, letter, September 21, 1897). What Freud is actually saying is that he is *unable* to accuse his father of sexual abuse. Just a few months earlier he wrote to Fliess that he was convinced that his brothers and sisters had been sexually abused by his own father (Masson, 1985, letter, February 8, 1897). Why did he exclude himself? Is there a connection between this accusation towards his father and the trauma Freud experienced with his death? Krüll (1979), in her study on Freud's childhood relationship with his father and the void he experienced later in life, argues that this is the reason why Freud was never able to bring his self-analysis to an end. If he would have completed his self-analysis, he might have realized that he too, just as all of his brothers and sisters, had been sexually abused (Krüll, 1979, p. 74). Rejecting the seduction hypothesis offers a good 'unconscious' solution to acquit his father of sexual abuse and overcome the sorrow of his death. Rush (1980), who made a study on the history of child sexual abuse, writes: "One must remember that when Freud arrived at the seduction hypothesis, he did so by listening carefully and intently to his female patients; when he arrived at his Oedipal theory, he did so by listening carefully and intently to himself... His conflicts about his own father may have caused him anguish and guilt, but does this exonerate other fathers?" (Rush, 1980, p. 95)

The above leads us to an important social factor which influenced Freud's thinking. According to Miller (1981) a taboo exists within society that condemns children to a lifetime of emotional attachment to their parents. Children in our society are, according to Miller, raised under the Fourth

Commandment "Thou shall honour thy father and mother". But actually, as she claims, the message is "you are not allowed to become aware of what your parents have done to you". Miller considers this rule of obedience as embedded in individual life as well as in psychotherapy. Many analysts and therapists are themselves victims of what she calls "Schwarze Pedagogik"; they too are victims of suppression and manipulation. Unconsciously, in the therapeutic setting, they use the same authoritarian attitude towards their patients. If a therapist tells the patient that his or her parents are only human and should be forgiven, then unconsciously the Fourth Commandment is enforced on the patient. It will never be possible to totally reject one's parents in order to overcome one's traumatic past (Miller, 1981, p. 23). Miller (1981, p. 146) furthermore argues that because Freud was caught in the patriarchal system and not able to overcome the Fourth Commandment, he had to reject his seduction hypothesis. With her theory, Miller is killing two birds with one stone. She not only explains the reason why Freud rejected his seduction hypothesis, but also why he was unable to accuse his own father of sexual abuse. Here we see that social and personal factors are closely intertwined.

Another important factor in the rejection of the seduction hypothesis, and which is closely connected to the first one (isolation from the scientific community), is that his ideas conflicted with fundamental conceptions or prejudices within society. Conceptions of hysteria, sexuality, mental life, even the values of the society and the family were at stake. Lasch (1977) argues that in the last 150 years a myth has evolved concerning family and society. The myth is that the family is idealized as "a haven in a heartless world." With his hypothesis, Freud implicitly broke with this myth. He started what Parton (1985) calls a "moral panic"; one can be 'deaf' to possible explanations or have doubts as to the sense of reality of the scientist. This explains why Freud's ideas had not been heard; they just *could not* be heard. It also explains why Krafft-Ebing addressed Freud after his lecture with the comment that "it sounds like a scientific fairy tale" (Masson, 1985, letter, April 26, 1896). And no wonder his lecture was not mentioned in the university journal. It was a matter of convenience and even a necessity to keep up the biological explanation for hysteria instead of accepting a socio/psychological one with all of its consequences. If society were to uphold their idealization of the family, one had to repudiate Freud's observations. Not only the family, the profession of scientist was also idealized. The acceptance of Freud's ideas would in fact mean that the men of science were a laughing stock. What they had been claiming for centuries was not true, and it was one man, Sigmund Freud, who had found, what he himself called, the "Caput Nili", the source of the Nile (Masson, 1985, letter, April 26, 1896). If society was to uphold this idealization of scientific integrity, one had to dismiss all implicit accusations and, therefore, repudiate Freud's observations.

If societal norms and deeply rooted beliefs are at stake, one cannot expect the scientific profession to exclude itself from these 'prejudices'. They

too are a part of society and it is almost impossible for a scientist to become a-historical and transcend existing beliefs. This is why science cannot be seen as an independent way to gather information about its subject, based on rational criteria, constantly evolving in the one way direction of scientific truth. As Mills (this volume) argues, it might well be that certain theories should be regarded in terms of their social and moral function at the time of their creation. For it was Freud's rejection of the seduction hypothesis that eventually lead to the birth of psychoanalysis. This in turn is largely based on the acceptance of the Oedipal theory. It actually suppresses the reality of child sexual abuse and clears the father of any blame. It, furthermore, anticipates the myth of the seductive child and to male-female role expectations. Society was unable and unprepared to face 'the truth of incest', and the Oedipal theory coincided with it.

Freud's ideas were, at the time of their presentation, unacceptable. But now, almost a century later, changes have occurred within society that have made a more open discussion possible. Much research still has to be done, but some factors might be distinguished. First of all, there is psychoanalysis itself. Although psychoanalysis has been only interested in the psychological reality, side-effects on society have, in the long term, been more openness and understanding towards sexuality. It may be seen as a paradox, that psychoanalysis, which believed that the traumatic sexual experiences in childhood were fantasized, eventually contributed to the realization that sexual abuse is *not* a fantasy but an existing reality. But even today some are unwilling to see the reality of the seduction hypothesis and are blind in their own cause. Gay (1988), the latest biographer on Freud's life, frenetically states in his book *Freud. A life of our time*:

> The seduction theory in all its uncompromising sweep seems inherently implausible; only a fantasist like Fliess could have accepted and applauded it. What is astonishing is not that Freud eventually abandoned the idea, but that he adopted it in the first place (Gay, 1988, p. 91).

The result of this growing tolerance towards sexuality eventually led to the emergence of the first biographies of women who had been sexually abused in childhood. In the late seventies they were the first to tell their story of how it is to be condemned to a life of silence (Armstrong, 1978; Brownmiller, 1976; Butler, 1978; Forward, 1979; Rijnaarts, 1979; Vale Allen, 1980). To publish the intimate story of ones traumatic past was facilitated by the feminist movement. These biographers created a change in sex-role expectations and tried to redefine sexuality. Most important, they accepted the reality of incest which in turn inspired others to tell their stories (Butler, 1978; Rush, 1980; Herman, 1981).

Another factor which contributed to breaking the 'silence of incest' was the media. Newspapers and popular journals came with almost 'pornography like' stories of women who had been sexually abused as a child. In the search for new kinds of sensation, incest is just another phenomenon to focus the

camera on. Although the first interviews on television with incest victims were shadows and silhouettes of women who were afraid of social rejection and retaliation, now, since two or three years, women are starting to overcome there fears and angers. They are openly accusing their brothers, uncles and fathers...

Although external factors influenced Freud's ideas, internal factors also may have contributed to the rejection of the seduction hypothesis (Droste, 1989). Freud himself mentions some of these factors (Masson, 1985, letter, September 21, 1897). But for the purpose of this paper it suffices to conclude that Freud was *also* influenced by factors which lie outside the paradigm of objectivity. Personal as well as social factors influence the formation of psycho/sociological theory. Most of us would like to discard the existence of these factors in order to stay trustworthy as 'objective' scientists. It is only after one has become famous (and usually dead) that others, mainly historians, try to reconstruct the context in which the theory has emerged. This fact alone shows a belief that theories are influenced and molded by the pressures of society and the burdens of our past. Knowledge, therefore, cannot be based solely on an internal 'scientific' discussion, but has to take external 'unscientific' factors into consideration.

REFERENCES

Armstrong, L. (1978). *Kiss daddy goodnight: A speak-out on incest.* New York: Hawthorne.

Brownmiller, S. (1976). *Against our will: Men, women and rape.* Harmonsworth: Penguin.

Butler, S. (1978). *Conspiracy of silence.* San Francisco: Volcano Press.

Droste, H. (1989). *Verwerping en ontkenning.* Unpublished: Leiden University.

Forward, S. (1979). *Betrayal of innocence: Incest and its devastation.* Harmondsworth: Penguin.

Freud, S. (1896). *The aetiology of hysteria.* In Masson (1984, p. 259-290).

Gay, P. (1988). *Freud: A life of our time.* London: Dent.

Herman, J. (1981). *Father-daughter incest.* Cambridge: Harvard University Press.

Krüll, M. (1979). *Freud und sein Vater.* München: Beck.

Lasch, C. (1977). *Haven in a heartless world; the family besieged.* New York: Basic Books.

Masson, J. (1984). *The assault on truth. Freud's supression of the seduction theory.* Harmondsworth: Penguin.

Masson, J. (1985). *The complete letters of Sigmund Freud to Wilhelm Fliess 1887-1904.* Cambridge: Belknap Press.

Miller, A. (1981). *Du sollst nicht merken.* Frankfurt: Suhrkamp.

Parton, N. (1985). *The politics of child abuse.* London: MacMillan Education.

Rush, F. (1980). *The best kept secret. Sexual abuse of children.* New York: Mcgraw-Hill.

Rijnaarts, J. (1979). Was Noreen Winchester een uitzondering? *Sociaal Feministishe Teksten 3,* 73-100.

Vale Allen, C. (1980). *Daddy's girl: A memoir.* Hemel Hempstead, UK: Simon & Schuster.

FREUD'S DOCTOR'S BAG:
ON HIS HEURISTIC RESOURCES

Geert E. M. Panhuysen

SUMMARY: This paper illustrates how heuristics affect theory construction through the use of a case study of the early development of psychoanalysis. Freud borrowed the heuristic starting points of his research program for the neuroses directly from traditional and recent biomedical thought, but not without adapting these biomedical search schemes for the purpose of theory construction at a psychological level. He abandoned the requirement of a specific pathological anatomical change for each disease and introduced the requirement to look for a specific psychic pathological mechanism for each psychoneurosis. This new criterion led him to a mechanization of the mind and the construction of a general theory of the psychic apparatus.

Introduction

"Relics of the past still survive", Freud wrote once to Fliess. Much of psychoanalysis is an elaboration of this theme. Likewise it can be seen as a dictum that relates to the development of Freud's theories. The years of his biomedical education and neurological research left traces which affected the formation of his ideas. In most of his theoretical steps at a psychological level he made use of conceptual instruments from his doctor's bag. Freud borrowed his intellectual resources (heuristics, search schemes) from traditional and recent biomedical thought, adapting them for the purpose of psychological theory construction.

A case in point is his view about the etiology (the causes) of the psychoneuroses, a remarkable feature of which is the biphasic realization of the psychoneuroses. First, the outcome of early psychosexual development leaves weak points in the mental equipment of the person; second, these weak points remain without any symptomatic manifestation until the person, as adolescent or adult, is provoked by the requirements of mature sexuality. How did Freud think of this view? Freud had specialist's knowledge of children's palsies (paralyses and anesthesias) and adapted one of the etiological schemes he had applied in that domain to contrive etiological insights for hysteria and the other psychoneuroses. In his monograph on double-sided children's palsies (Freud, 1893b) he described several cases that satisfy the following scheme: the development of some part of the cerebrum has been disturbed, for example, by a birth trauma. Then, after a period of time without any symptoms at all, contractions or convulsions occur at the moment that a (psycho)motoric function arises (when the child tries to walk, to speak or to write) addressing the damaged part of the cerebrum. This scheme of deferred

action gives an answer to a particular type of question: how is it possible that grave symptoms make their appearance without the immediate presence of provoking agents that are compatible with the gravity of the symptoms? The scheme requires a newly arisen functional claim and a disturbed (brain) development in the past. Freud only extended the scope of this scheme from neurological affections to psychoneuroses.

My thesis is that a similar explanation can be reached for most of the development of Freud's psychoanalytic theory. Here I will show, in particular, that the heuristic starting points of his research program for the neuroses of the early nineties were driving him forward in the development of a general psychological theory. This kind of understanding is not merely of historical significance, but can also give us insight into the usefulness of his intellectual resources for psychological theory-construction now.

My approach supposes a distinct theory about the growth of knowledge. How do scientific theories originate? Theories are the products of problem solving activities, brought to systematization by heuristic views or habits of thought. These heuristics define the problems and the work to be done, direct the search processes and pose the requirements the solutions must fulfil. The richer the heuristics, the more systematic the search.

Why have I chosen to study Freud? The development of psychoanalytic theory by Freud is a very interesting case in which to study heuristics at work, because the letters he wrote to Fliess give us an unique opportunity to follow his theoretical steps, the course of his thoughts, very closely.

Freud's Nosographic Program of 1894 and its First Results

Freud sent in 1894 to Fliess a table of contents for a textbook about the neuroses (Masson, 1985, pp. 76-78). His collaboration with Breuer had given him an initial success in this domain, their well-known theory of hysteria (Freud, with Breuer, 1893a) — hysterical symptoms are the expression of unconscious, undigested traumatic experiences — and Freud considered his own contribution (the idea of pathological defense) to be a concept with great promise in relation to the explanation of the other psychoneuroses. Hence this sketch, which implies a nosographic[1] program and a summary of the directives Freud chose to follow when he set foot in this field.

To the traditional core of this program belong the assumptions that it is possible, first, to reduce every phenomenon of illness to some unit of disease and, second, to order these units into a taxonomy, *'more botanico'*. The nosographic program has a long history (Diepgen, 1951); and its directives changed every time when the general conceptions about the causes and nature

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[1] Nosography is the description, identification and classification of diseases.

of illness changed. What is more, this development of the nosographic program influenced the development of the concept of neurosis rather directly (Lopez Pinero, 1983).

Sydenham has the first peak of the program to his name (Sydenham, 1682); he established the requirement that every unit of disease should be founded on a fixed pattern of symptoms, a specific complex of symptoms in constant conjunction.

Since the rise of the anatomoclinical conception of disease (Morgagni, 1761) symptoms of disease were viewed as manifestations of an underlying pathological state; all phenomena of disease should be reduced to the site, extension and nature of organic lesions. This view led to a second requirement for a nosographic unit: every unit of disease should also be associated with a specific underlying pathology, namely a specific structural change of some organ.

When physiological medicine flourished ("Disease is life under changed conditions"; Virchow, 1858), the underlying pathology should be sought in altered function. Knowledge of altered structure did not suffice any longer; and the possibility of disease without any structural lesion was opened.

At last, the successes of the germ theory, of the hunt for microbes by Pasteur[2] and Koch (Koch, 1882), generated the demand to look for a specific etiology, the third nosographic requirement: each disease should have its own 'microbe'. So noso*graphy* was transformed into noso*logy*[3]; its objectives, description and classification, were supplemented with explanation.

When Freud, who was a man with ambitions, sketched his own nosographic program for the study of the neuroses, he assimilated all of the above criteria into his sketch, which had surprising consequences. First and foremost he was interpreting these biomedical ideas in psychological terms: the etiological factors and pathological mechanisms acquired a mental status. Second, the search for underlying pathology shifted into the search for psychological theory. As abnormal mental life should be viewed as mental life in modified conditions, a theory of mental dysfunctioning is only possible on the basis of a theory of mental functioning in general.

Freud's nosographic inspiration had already expressed itself in a contribution about hysteria in 1888 (Freud, 1888b). In this article he supported the position of Charcot, his Paris master, that the phenomena of hysteria display orderliness and regularity. This position was fiercely contested by many of Charcot's predecessors and contemporaries in neurology and psychiatry; one viewed the fickleness of hysterical phenomena as a good reason to consider

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[2] Pasteur published in 1877 papers on anthrax and chicken cholera (Pasteur, 1922-1929).

[3] Nosology as the knowledge of the diagnostics, etiology, pathology, prognosis, and therapeutics of the different diseases.

hysteria not as a serious matter or a grave illness. Freud dissented. Although the pathological mechanism was not clear to him at that moment, in the typical case hysterical attacks, hysterogenic zones of the body, and disturbances in sensoric and motoric functions are fixedly conjoined. For him hysteria satisfied at least this basic requirement of a disease unit.

In one of his first neuropathological projects he tried to attain a clarification of the distinction between on the one side hysterical and on the other side organic paralyses and anesthesias (Freud, 1893c). His conclusion was that the spreading and delimitation of symptoms over the body in the case of organic affections tallies with the distribution and ramification of the nerves over the body, thus with scientific neuroanatomical knowledge. But in the case of hysteria the symptoms appear to be ignorant of neuroanatomical matters: their extension and limits concur rather with 'folk or laic anatomy', that is, with popular ideas about the construction of the nervous system. (A famous example is glove anesthesia.) For Freud this demonstrated the psychogenic character of hysteria in contrast with organic paralyses and anesthesias: hysterical symptoms are produced by ideas. Patterns of symptoms, differing to such a degree, must also have very different underlying pathologies.

The years round about 1894 witnessed an outburst of Freud's nosographic activities. The directives of his nosographic program made him unhappy with certain aspects of the trauma theory of hysteria as formulated in 1893. The underlying pathology was not clear enough for him. Breuer and he had suggested three possible mechanisms for hysteria: retention (circumstances hindering an adequate reaction), a hypnoid state (leading to the splitting off of an impression or idea) and pathological defense. Apart from that, their ideas about the etiology of hysteria had remained rather vague and divergent. He made considerable progress, in relation to his nosographic requirements in an article of his own, *"The neuro-psychoses of defence. An attempt at a psychological theory of acquired hysteria, of many phobias and obsessions and of certain hallucinatory psychoses."* (Freud, 1894a). Hysteria would be produced by a specific form of pathological defense: conversion. What is more, he associated the different forms of 'neuropsychosis' with different kinds of pathological defense: obsessions and phobias with substitution/transposition (displacement) and hallucinatory confusion with (partial) detachment from reality.

In *"Further remarks on the neuro-psychoses of defence"* (Freud, 1896b) he fulfilled his task by giving a specific etiology for each particular neurosis of defense. No neurosis occurs in the case of a normal sexual life, as only psychosexual problems provoke pathological defense. The specific condition for hysteria is a sexual trauma in very early childhood, at a time when this child was necessarily passive. The specific condition for obsessional neurosis is a sexual trauma somewhat later, at a time the child could be active. In the same article he described the specific defense mechanism involved in paranoia,

projection, which has as a specific condition active sexual experience in later childhood. Here he also exploited the idea of the biphasic realization of the psychoneuroses (the deferred action of early traumas) to reduce both retention and hypnoid state to particular aspects of pathological defense.

The letters to Fliess (Masson, 1985) show the amount of work he did in these years to find satisfactory specifications of the etiology of the different psychoneuroses: he took into consideration the age at the time the child was seduced, the age at which the repression occurred, whether the traumatic experience was laid down in a verbal or a visual memory, the degree of maturation of the cognitive and perceptual capacities at the time of the trauma, the precise nature of the sexual experience (like passivity or activity), the number of repetitions of such traumas, and so forth. All these hectic activities show that Freud would only be satisfied when he succeeded in specifying tenable correlations between differences in symptom, pathological mechanism and etiology.

Meanwhile, Freud had published his proposal to detach anxiety neurosis as a particular syndrome from neurasthenia (Freud, 1895b). What was known as neurasthenia amongst psychiatrists was in his view not to be considered as a separate disease unit, but rather as a whole cluster of clinical pictures. The way he argued for the introduction of anxiety neurosis as a particular syndrome was very characteristic of the directives of his nosographic program. He had observed the following symptoms in constant conjunction: headaches, nerve pains (in the back) and gastric weakness with flatulence and constipation. This was the clinical picture of neurasthenia *stricto sensu*. The symptoms concurring in anxiety neurosis were very different: over-sensitiveness (mainly of hearing) resulting in insomnia; free floating anxiety resulting in hypochondriac moods or qualms of conscience; attacks of anxiety of all sorts: heart palpitations, arrhythmia, slowing down of the heart-beat, dyspnoea, trembling and quivering, sudden cold sweat, starting from one's sleep at night, dizziness, canine hunger and diarrhoea, and phobic phenomena (but different from those in the case of obsessional neurosis).

The pathological state Freud connected with anxiety neurosis was an excess of accumulated excitation. The etiology matching this state should be sexual abstinence or unsatisfied physical sexual stimulation. For women, sexual practices such as coitus interruptus and sexual shortcomings of men like ejaculatio praecox, would lead to 'frustrated' excitation. Freud connected neurasthenia with a state of excessive excitability. Due to the weakness of the nervous system the least excitation is already too much for the over-excitable nervous system, and the main etiological factor here should be masturbation. Notice that both neuroses, according to Freud, are effects of abnormal conditions disturbing the normal, general function of the nervous system.

Neurasthenia as well as anxiety neurosis are products of a purely physiological disturbance: the symptoms are the expression of quantities of

excitation the nervous system is not able to discharge, whether by weakness or by an excess of excitation, but psychological processing mechanisms do not play a part in these neuroses. This in contradistinction with the psychoneuroses, in which the psychic apparatus is reacting to psychic conflicts with pathological defense: in this case the physiological disturbance in the discharge of excitation really does result from psychological processing mechanisms. Not only is there a fundamental difference in underlying pathology, but also in etiology. Neurasthenia and anxiety neurosis have a simple etiology, whereas the etiology of the psychoneuroses is complex. Neurasthenia and anxiety neurosis belong to the group of *actual* neuroses, caused by sexual problems in the present. The more complicated causation of the psychoneuroses, however, proceeds always in two steps: earlier psychic development has left weak spots in the armorial bearings of the psychic apparatus and afterwards it is not able to cope with attacks upon these points. Notice that the psychoneuroses are effects of disturbed psychic development and abnormal life conditions on the normal defense mechanisms of the mind.

These nosographic maneuvers display some of Freud's core assumptions. Firstly, the basic function accomplished by a living organism is the discharge of excitation, whether from an external or internal source. The nervous system controls this function and in the highest organisms the nervous system forms the basis for a psychic apparatus, by which the discharge of excitation can be optimized in the long run. Secondly, dysfunctioning is always possible; if the organism is not able to discharge excitation adequately, the excess of excitation ventilates itself by symptom formation. Dysfunctioning can occur on two levels. Either the lower, purely physiological, functions of the nervous system are disturbed, or the higher functions are out of order, in which case (relatively autonomous) psychic mechanisms are involved. Thirdly, organisms are developing beings, and on this basis the causes of dysfunctioning can be distinguished into factors interfering with development on the one side and actual factors deranging a well-developed organism on the other side.

Given these core assumptions Freud had to ask and answer the following questions: What is the composition of the psychic apparatus? What are its workings? What is the course of development of this apparatus? and What are the conditions, by which the workings and the development of the psychic apparatus can be disturbed? These are exactly the questions he tried to answer in the famous seventh chapter of *The interpretation of dreams* (Freud, 1900a) and in *Three essays on the theory of sexuality* (1905d). Freud started with a relatively modest, nosographic program, but the tasks this program implied resulted in the elaboration of an all-embracing, explanatory psychological theory; the internal logic of his program led to a change of figure and background in his thinking. But he did not give up his nosographic pursuits, and still less the directives of his nosographic program in his later scientific career.

Some Conclusions

How was it possible that Freud's approach — although in many respects connected with the biomedical tradition and applying its resources — resulted in a general psychological theory? To answer this question, I will make a comparison between the nosographic programs of Freud and of Kraepelin. The handbooks of Emil Kraepelin (Kraepelin, 1910-1920) dominated the psychiatry in the first decades of this century. His work, like that of Freud, was at the beginning mainly directed to immediate nosographic tasks: the identification and classification of mental diseases. But, in contrast with Freud, this direct nosographical orientation has continued to dominate the whole of his scientific career. This had something to do with his priorities: a reliable psychiatric diagnosis was one of his first aims. But it had much more to do with the particular directives of his nosographic program. The Dutch psychiatrist H. C. Rümke has left us the following concise summary of Kraepelin's program: "The ideal of the seeker of disease units was: the same cause, the same somatic and psychic clinical picture, the same terminal state, the same pathological anatomical substratum." (Rümke, 1954, p. 20)

In the case of diseases without known cause or without known bodily substratum one has to restrict one's attention mainly to the clinical picture, the course of the disease and the terminal state. If this restricted program succeeds a reliable diagnosis remains possible, as well as does the prediction of the course of the disease, but a 'causal' therapy vanishes from sight.

Summarizing Freud's program likewise in this manner, I suggest the following formula:

> 'the same etiology, the same somatic and psychic clinical
> picture, the same physiological or psychic pathological
> mechanism'.

The obvious similarity between these two summaries is the traditional morphological criterion: every unit of (mental) disease has to be grounded on a specific constant conjunction of (somatic and psychic) symptoms and a specific course of disease. But the differences are considerable. Firstly, although Kraepelin, like Freud, required in principle a specific etiology and a specific underlying pathology for each disease unit, his practical priorities confined his program to the clinical picture and so to the traditional criteria. Freud kept the principles in the foreground, and thus the requirements of a specific etiology and pathology incited him to causal explanations and a general theory of normal and abnormal functioning. Secondly, in the view of Kraepelin the search for underlying pathology always implied the identification of alterations in anatomical structure. Not so for Freud: in his view there could be change of function without change of structure, and change in psychological functioning without change in physiological functioning. Thus his attempt to specify the disease mechanisms for the psychoneuroses could

result in a general *psychological* theory. Freud was impelled into psychology both by what he left out and what he filled in.

The approach I have used here has not only significance for theoretical psychology, but also for the history of science. Explanation of scientific developments by means of heuristics is far more economical than by means of the invocation of all kinds of sources from which scientists might have borrowed.

References

Note: The alphabetical subscripts for Freud's works within the same year are adopted from the Bibliography of his publications in the final volume of *The Standard Edition, 24,* 47-82 (Strachey, 1953-1974).

Diepgen, P. (1951). *Geschichte der Medizin. 2.* Berlin: De Gruyter Verlag.

Freud, S. (1888b). Hysteria. In *Standard Edition, 1,* 39-57.

Freud, S., with Breuer, J. (1893a). On the psychical mechanism of hysterical phenomena. (Preliminary communication.) In *Standard Edition, 2,* 3-17.

Freud, S. (1893b). Zur Kenntnis der cerebalen Diplegien des Kinderalters (im Anschluss an die Little'sche Krankheit. In Max Kassowitz (Ed.), *Beiträge zur Kinderheilkunde.* Heft III, Neue Folge. Vienna: Moritz Perles.

Freud, S. (1893c). Some points for a comparative study of organic and hysterical motor paralyses. In *Standard Edition, 1,* 157-172.

Freud, S. (1894a). The neuro-psychoses of defence: an attempt at a psychological theory of acquired hysteria, of many phobias and obsessions and of certain hallucinatory psychoses. In *Standard Edition, 3,* 43-61.

Freud, S. (1895b). On the grounds for detaching a particular syndrome from neurasthenia under the description 'anxiety neurosis'. In Standard Edition, 3, 87-115.

Freud, S. (1896b). Further remarks on the neuro-psychoses of defence. In *Standard Edition, 3,* 159-185.

Freud, S. (1900a). The Interpretation of Dreams. In *Standard Edition, 4-5.*

Freud, S. (1905d). Three essays on the theory of sexuality. In *Standard Edition, 7,* 3-122.

Koch, R. (1882). Die Aetiologie der Tuberculose. *Berliner klinische Wochenschrift, 19,* 221.

Kraepelin, E. (1910-1920). *Psychiatrie. (Ein Lehrbuch für Studierende und Aerzte),* (8th ed.). Four volumes. Leipzig: J. A. Barth Verlag.

Lopez Pinero, J. M. (1983). *Historical origins of the concept of neurosis.* Cambridge, NY: Cambridge University Press.

Masson, J. M. (Ed.) (1985). *The Complete Letters of Sigmund Freud to Wilhelm Fliess (1887-1904)*. Cambridge, MA & London: The Belknap Press of Harvard University Press.

Morgagni, G. B. (1761). *De sedibus et causis morborum per anatomen indagitis*. Five volumes. Venice.

Pasteur, L. (1922-1929). *Oeuvres de Pasteur*. Seven volumes. Paris: Masson.

Rümke, H. C. (1954). *Psychiatrie, 1*. Amsterdam: Scheltema & Holkema.

Strachey, J. (Ed.) (1953-1974). *The Standard Edition of the Complete Psychological Works of Sigmund Freud*. Twenty-four volumes. London: Hogarth Press & The Institute of Psycho-Analysis.

Sydenham, T. (1682). *Dissertatio epistolaris ad G. Cole de observationis nuperis circa curationem variolarum confluentium, necnon de affectione hysterica*. London: Kettilby.

Virchow, R. (1858). *Die Cellularpathologie in ihrer Begründung auf physiologische und pathologische Gewebelehre*. Berlin: Hirschwald.

ON THE FUNCTION OF FOLK PSYCHOLOGY IN THE THEORY AND HISTORY OF PSYCHOLOGY

Helmut Hildebrandt[1]

SUMMARY: In modern cognitive science the relation of experimental psychology to folk psychology is controversial. But there are some propositions about the explanations folk psychology gives that are generally shared. The main emphasis is laid on a rationalistic account of the structure of mental processes. This view of folk psychology is new and its consequences for cognitive science are challenged by historical forerunners of folk psychology. The example of a hermeneutical psychology discussed heavily in German psychology between the two world wars shows very different opinions on folk psychology. During this period the main points of interest were a theory of individuality and personality that includes emotionality and irrationality. Therefore, one can draw some conclusions for the debate between proponents and opponents of a strong relation of folk psychology to cognitive science. First, folk psychology relies very much on global historical situations and traditions. Second, folk psychology as a social phenomenon may serve as a point of referral for a historical examination of the development of psychology and then offers a possibility to discuss externalistic influences on scientific psychology. But in this case, the inner theoretical reference is abandoned.

The abandonment of the behavioristic research program in psychology was not only due to purely internal theoretical developments. It can be attributed to obvious technological developments in, for example, computer and information technology, as well as to less apparent social developments. With respect to the latter Stich (1983) adopted the expression "Two Cultures" which have been formed as a consequence of the dominance of behaviorism in the scientific explanation of human behavior. Until well into the sixties there were two rival forms of psychological explanation. On the one hand, we had behaviorism with its claim to be able to do without inner states while, on the other hand, we had the folk psychological and intentional explanations of behavior that were used in legal terminology, psychotherapy, in historical and social sciences, and in everyday human relations. This tension between two forms of psychological explanation could, in spite of the claims by philosophical behaviorists that folk psychology was merely the internalization of originally outer speech, no longer be maintained. It made the cognitive revolution inevitable and, perhaps necessary.

Indeed, one direction of cognitive psychology made folk (or commensense or belief/desire or naive) psychology its main starting point. This direction, which one can best call 'symbol processing' or 'representational

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[1] Translation by Pamela Jones.

theory of mind' is embodied most clearly in the theoretical analysis of Fodor (1987) and Pylyshyn (1984). Indeed, Fodor (1987) introduces a set of reasons to show that an approach based on folk psychology is indispensable. These reasons are the following:

1) The frequency with which folk psychology formulates true propositions is extremely high. This has to be the case, because folk psychology co-ordinates the actions of individuals (see also Pylyshyn, 1984).

2) The predictions which are derived from folk psychology are of a deductive form, that is, they comply with the ideals of an explanatory scientific method. Nor are these predictions trivial, rather, they function as the background of non-observable structures or entities.

3) Folk psychology is indispensable for the classification of mental states. Thus, for example, without an understanding of the meaning of a sentence no interpretation of its behavioral affects can be made.

For Fodor (1987) the development of computer technology is the most significant breakthrough in psychology in the last two centuries. It generated a model which is capable of bringing about a connection between folk psychology explanations of behavior and causal theories of the mind. The central problem of psychology is how semantic representations can have causal effects. This question can be answered by pointing out that each semantic content can be interpreted as a set of symbols with a definite internal structure. This internal structure of a set of symbols plays a causal role in the processing of these symbols because it is this structure which permits particular forms of processing effects. One can, therefore, develop a causal theory of the mind comparable to a theory of computation. Fodor writes:

> The operations of the machine consist entirely of transformations of symbols; in the course of performing these operations, the machine is sensitive solely to syntactic properties of the symbol; and the operations that the machine performs on the symbol are entirely confined to altering their shapes. (Fodor, 1987, p. 19)

Fodor and Pylyshyn's theory linking cognitive and folk psychology has been questioned during the past decade. The most distinguished criticism comes from Paul Churchland (1981), Stephen Stich (1983) and Patricia Churchland (1986), all three of whom doubt whether folk psychology is a productive starting point for cognitive psychology. I will briefly consider their arguments.

1) Folk psychology explanations apply only to a limited group of people. Folk psychological explanations do not hold across widely different cultures. The same can be said for

children and the mentally ill who either do not as yet possess the adult social structure norms or else who deviate completely from these norms.

2) Folk psychology is a theory without dynamic; since the ancient Greeks nothing has changed in its content.

3) Folk psychology is so ambivalent that it is impossible to formulate a contradictory-free system of propositions to represent it.

As may be expected of Stich's account of the derivation of the cognitive revolution in psychology, his formulation of folk psychology functions as a reference point, in the same way as it does for the Churchlands and Fodor/Pylyshyn, which is either adopted or else rejected for cognitive psychology. Apart from its function as a reference point, there are other characteristics to note in the interpretation of folk psychology by its supporters and opponents.

First, its contents are always defined in the form of propositions in the third person, although one might have expected a first person perspective. Typical statements which are understood to be of a folk psychological nature are 'Person P believes that ...'. This shows, secondly, that folk psychology has been viewed as a quasi-scientific theory, in the sense of being a theory about rational and coherent mental processes. Thirdly, it follows that folk psychology has a close relationship to Proof Theory, a theory which concerns itself with how propositions can be construed without mistakes creeping into the course of its deductions and inductions.

Contrary to earlier psychological attempts, neither immediate observable behavior in its natural or social environment (as in behaviorism) nor the immediate given facts of consciousness (as in experiential, structuralist, or consciousness psychology) are objects of cognitive psychology. The objects are more decisively, if only temporarily, established through a number of folk psychological explanations, which are reconstructed, refined, or replaced by cognitive psychology. Fodor in particular is explicit about this conception. The classifications of mental states and relations drawn from folk psychology represent a group of phenomena (propositions) which are taken as the explanandum and as the empirical standard for cognitive psychology. Fodor writes:

> What I've said so far amounts largely to this: An explicit psychology that vindicates commonsense belief/desire explanations must permit the assignment of content to causally efficacious mental states and must recognize behavioral explanations in which covering generalizations refer to (or quantify over) the contents of the mental states that they subsume. I now add that the generalizations that are recognized by the vindicating theory mustn't be crazy from the point of view of common sense; the causal powers of the attitudes must be, more or less, what common sense supposes that they are.

After all, commonsense psychology won't be vindicated unless it turns out
to be at least approximately true. (Fodor, 1987, p. 14f)

But Stich (1988) also sees in the explanations of folk psychology and in
the derived theory of symbolic processing the group of propositions which are
to be replaced by a real scientific theory. Both strategies offer an attempt to
overcome the gap in explanation; one is reconstructive, the other eliminative.

What are we to think about this definition of the objective of cognitive
psychology? In the following I want to put forward some arguments which
demonstrate that the claims of Fodor/Pylyshyn on the one hand and
Stich/Churchlands on the other are asymmetrically true or false. A short
historical analysis will show that Fodor/Pylyshyn are correct in their idea that
folk psychology is empirically a relevant starting point for the development of
psychological theories, however its interpretation as a constitutive moment
for scientific psychology is extremely problematical. Second, it will be shown
that Stich/Churchlands are correct in maintaining that folk psychology is not
a serious candidate for the development of a theoretical psychology, but, at
the same time, that folk psychology cannot be excluded as a serious empirical
candidate for the explanation of the development of concrete psychological
theories. To put the matter another way, it will be shown that the notion of
commonsense psychology when normatively used for the development of a
theoretical psychology is misleading, but that its descriptive use may be
fundamental for the history of psychology. The way in which this will be
shown is by appeal to the history of science. To prove these two claims we will
briefly examine the situation of German Psychology between 1922-1933, and
will see that, during this time, there was a fundamentally different conception
of folk psychology. Because of this, one cannot make the necessary
generalizations on the basis of folk psychology essential for the development
of a theoretical psychology.

The history of psychology during the period 1922 to 1933 in Germany is
linked very closely to the slogan "crisis of psychology" and to the appearance
of a book by the same name by Bühler (1927). In that book, Karl Bühler, an
experimentally oriented psychologist and better known for his research on
thinking and his axiomatics of language theory, tried to prove the necessity of
a hermeneutical viewpoint for a complete system of psychology. As the
background to this attempt, he offered his axiomatic theory of language. A
second background consideration was his analysis of the meaning of 'meaning'
and 'intentionality' in all areas of psychology. Bühler supported these
analyses by two arguments.

1) The hermeneutical viewpoint emphasized that without the
   moment of "intentionality" a statement about what and how
   human beings perceive is not possible. The homogenous
   area of perception can be factually divided into two parts:
   into the "immediate control" of an organism by its environ-

ment (e.g., by an orienting reflex), where a stimulus which has deterministic effect on the organism, and the "mediated control", when perception of the environment can only be explained by the adherence to definite internal intentions (see the difference between "interrupt" and "test" by Pylyshyn, 1984).

2) The hermeneutical viewpoint emphasized that in social interaction, the reciprocal understanding and control over intentions is a fundamental occurrence. However, this phenomenon was only explicable if one analyzed adequate reaction types, independent of a concrete individual, and afterwards recorded the particular realization of these reaction types of the participating persons. Neither of these procedures were possible with the purely causal methods of experimental psychology.

Bühler's (1927) focus on the meaning of the hermeneutical viewpoint for experimental psychology had a major impact on at least one part of scientific psychology. Therefore, it is obvious that two conditions must have been fulfilled, namely,

1) some must be skeptical about this claim, and

2) there must have been a hermeneutical psychology which was worthy of all this effort.

In 1927 both these conditions were fulfilled. The skeptics of the meaning of a hermeneutical viewpoint as a complete system of psychology were Bühler's pure 'structural' and 'content' psychologists or, more concretely, the Gestalt psychologists and the psychoanalysts. Both these important approaches to psychology tried to do without the category of intentionality in their scientific theories. The Gestalt psychologists relied on the regulatory processes found in natural systems to explain the purposefulness of human behavior and thinking, while the psychoanalysts by means of the libido theory which through the stability principle and the acceptance of the death instinct lead to an analogous biophysical premise.

On the other hand, there were numerous hermeneutically oriented psychologists who, in the widest sense, claimed that they could deliver a theory of the human mind (see Spranger, 1926) without having to fall back on neurophysiological explanations. These hermeneutically oriented psychologists were primarily concerned with the applications of psychology, for example, in psychiatry, as "understanding psychiatry" (Jaspers, 1923, 1963; Kronfeld, 1920; Roffenstein, 1926), and in education, as "understanding pedagogigs" (Spranger, 1932). The central claim of these theories was that mental processes could be seen as relations between intentional mental

events, which could be described and defined without any causal explanations whatsoever.

The discussion surrounding the necessity of a hermeneutical viewpoint for a complete system of psychology during the years of the Weimar Republic is analogous to contemporary cognitive psychology's concern with 'folk psychology'. The hermeneutical version of psychology tried, first, to give a reconstruction of mental relations from a third person-perspective, and secondly, it tried to identify the typical structures of semantic relations by means of this reconstruction. Third, it saw itself explicitly in continuity with everyday practical problems of interpersonal understanding and influence. Moreover, these early hermeneutic psychologists faced the same criticisms as their modern counterparts, namely, that their methods were not sufficient for an analysis of mental illness, but only for disturbances of normal psychological processes (Bumke, 1924). It was said, that their analyses possessed no genuine conceptual and theoretical clarity (Störring, 1927) and hindered the foundation of psychology as a natural science (Koffka, 1925).

But the deciding factor for the present interpretation, is not the extent to which folk psychology is identical with the earlier expressions of a hermeneutical psychology, but that there exists a similarity between folk psychology and hermeneutical psychology only insofar as some of the typical contents of both theory constructions are excluded. While the proponents of modern cognitive psychology claim that folk psychology strives after a more or less rational analysis of reasons for human behavior, and, therefore, draw a parallel to the epistemological and theoretical problems of truth theory, the proponents of hermeneutical psychology saw the most meaningful aspects of mental life in the expressions of individuality or personality, in human emotions and irrationality, and in the difference between mental structures reflecting social circumstances.

Without being able to go further into the similarities and differences between the contents of today's folk psychology and the former hermeneutical psychology, I will draw some conclusions from Bühler's *Crisis of Psychology* for the theses of Fodor/Pylyshyn and Stich/Churchlands. A first conclusion from a comparison of the different contents of the former hermeneutical psychology and today's folk psychology is that one cannot, contra Fodor and Pylyshyn intend to, substantiate a generally applicable cognitive science and at the same time refer to folk psychology. Concrete folk psychology concepts, that is, all those notions which are developed on a more or less abstract level about the mental antecedents of actions of other persons, rely very much on global historical situations and traditions. It is not the lack of dynamics (see Stich, 1983) which makes folk psychology a nonacceptable candidate for the project of a general scientific psychology, rather the contrary, namely, folk psychology has a too powerful dynamic in the notions about human motivation and mental structures.

If one refers to Bühler's axiomatics it becomes clear, as a second conclusion, that the claim to reconstruct or eliminate folk psychology by means of cognitive psychology is at the same time too narrow and too broad a goal for a scientific psychology.

It is too narrow because psychology examines the whole question of how human beings comprehend and understand the world. The nonsemantic and nonintentional psychophysics, which exists in the explanations of folk psychology only as a 'special case' (with special regulations for the blind, deaf, etc.), is very much a part of psychology.

It is too broad, because behavior in social circumstances is not explainable by psychology alone, but by an analysis of the social situation itself. An analogous example is that just as one cannot explain social processes psychologically it is impossible to explain the flight of birds solely by the internal activation pattern of the wing muscles, but only by considering the beating of the wings in relation to the objective conditions of air pressure, air movement, and so forth.

A third conclusion is that folk psychology plays a much more important role in the development of psychology than Stich or Paul Churchland lead us to believe. The concepts of folk psychology, which exist side by side with scientific psychology and which are generalized in definite practical connections and formed into quasi-scientific systems, influence the theoretical course of scientific psychology tremendously. This influence is not only ascertainable in the light of the 'Two Cultures' which Stich thinks he can diagnose in the history of behaviorism, it can also be found in the period of the Weimar Republic in the relationship between hermeneutical psychology, medical psychology and experimental psychology. The rise of folk psychology concepts, so to speak, are responsible for the crisis which took place in the core area of psychology between 1922 and 1933, and for the development in psychology in Germany until well into the 1960s. In particular its practical effect — in that folk psychology gives varying but clearly explanatory information, in a given historical situation, about the backgrounds of human behavior — makes it a serious and institutionalized competitor for scientific psychology. Apart from this one can trace 'folk psychology' far further back than scientific psychology in the history of sciences. The beliefs of folk psychology greatly influenced the development of scientific psychology (see Jaeger & Staeuble, 1978). In a conception such as this 'folk psychology' assumes an important function in the conceptual realms of social history and history of science and plays the role of the mediator between externalist and internalist explanatory strategies in the history of psychology. But in this case, the intratheoretical reference which Fodor/Phylyshyn's commonsense psychology contains must be abandoned.

References

Bühler, K. (1927). *Die Krise der Psychologie.* Stuttgart: Fischer.

Bumke, O. (1924). Über die gegenwärtigen Strömungen in der Klinischen Psychiatrie. *Münchener medizinische Wochenschrift, 71,* 1595-1599.

Churchland, P. M. (1981). Eliminative Materialism and Propositional Attitudes. *Journal of Philosophy, 78,* 67-90.

Churchland, P. S. (1986). *Neurophilosophy.* Cambridge: Bradford Book.

Fodor, J. A. (1987). *Psychosemantics: the problem of meaning in the philosophy of mind.* Cambridge: Bradford Book.

Jaeger, S., & Staeuble, I. (1978). *Die gesellschaftliche Genese der Psychologie.* Frankfurt: Campus.

Jaspers, K. (1923). *Allgemeine Psychopathologie* (3rd ed.). Berlin: Springer.

Jaspers, K. (1963). *Gesammelte Schriften zur Psychopathologie.* Berlin: Springer.

Koffka, K. (1925). Psychologie. In M. Dessoir (Ed.), *Die Philosophie in ihren Einzelgebieten* (pp. 497-603). Berlin: Ullstein.

Kronfeld, A. (1920). *Das Wesen der psychiatrischen Erkenntnis.* Berlin: Springer.

Pylyshyn, Z. W. (1984). *Computation and Cognition.* Cambridge: Bradford Book.

Roffenstein, G. (1926). *Das Problem des psychologischen Verstehens.* Stuttgart: Püttman.

Spranger, E. (1926/1971). *Die Frage nach der Einheit der Psychologie.* In: Ges. Schriften IV (pp. 1-37). Tübingen: Niemeyer.

Spranger, E. (1932/1971). *Männliche Jugend.* In: Ges. Schriften IV (pp. 206-262). Tübingen: Niemeyer.

Stich, St. (1983). *From Folk Psychology to Cognitive Science.* Cambridge: Bradford Book.

Stich, St. (1988). From connectionism to eliminativism. Commentary to: P. Smolensky (1988), On the proper treatment of connectionism. *Behavioral and Brain Sciences, 11,* 1-74.

Störring, G. (1927). Die Frage der geisteswissenschaftlichen und verstehenden Psychologie. *Archiv für die gesamte Psychologie, 58,* 389-448.

A FUNCTIONAL THEORY OF ILLNESS

Michael E. Hyland

SUMMARY: Recent research suggests that psychological states have an impact on the incidence and outcome of physical illness. A fundamental question arises out of this research. Is the psychological state-morbidity relationship an accident of nature or it is biologically adaptive? I propose that the archeological and psychological data are consistent with the hypothesis that illness was functional during the Paleolithic, that although illness ceased being functional from Neolithic times onwards, the modern genotype reflects that earlier function. This hypothesis is supported by (a) considering two ways in which death is functional, (b) examining the different selective forces operating during Paleolithic and Neolithic/postneolithic times, (c) showing that the psychological data is consistent with morbidity evolving a function during Paleolithic but not Neolithic/postneolithic times, and (d) examining a possible mechanism for the psychological state-morbidity relationship.

Research in health psychology and psychoneuroimmunology provides evidence that psychological states and physical illness are related (see reviews in Friedman & Booth-Kewley, 1987; Jemmott & Locke, 1984). Although the research data in this field is by no means consistent, there is evidence that psychological states can have a causal effect on physical illness. The mind-body relationship has been a topic of theoretical concern in psychology for many years. In order to achieve consistency in use of the word *cause* when used between mind states and body states, Kirsch and Hyland (Hyland & Kirsch, 1988; Kirsch & Hyland, 1987) proposed a method of theory construction called *methodological complementarity* for dealing with psychosomatic causes, a method which is consistent with all mind-body philosophies other than dualism. The basic assumption of methodological complementarity is that mind states and body states should be treated as complementary descriptions of the same event. Causal relations do not occur directly between minds and bodies. The theoretical mechanism which allows mind states to affect body states is through the use of identity relations, these being simultaneous and complementary descriptions of mind and body.

That mind states might cause morbidity raises a question not normally considered in health psychology. Why should psychological states affect illness? How did the psychological state-morbidity relationship evolve? Did it happen by chance or did it, as will be argued here, evolve because it had biological utility? In this paper, I will focus on one aspect of morbidity, the fact that morbidity can lead to mortality, and I shall present the view that it is because morbidity can lead to presenescent death that it can be biologically adaptive.

The death of an organism is biologically adaptive if it enhances the survival of organisms with similar genes (Hamilton, 1964). In nature there are many examples where presenescent death is biologically adaptive. For example, worker bees will sacrifice themselves to protect the hive; the parent sockeye salmon die within a few days of spawning and their rotting carcases provide a rich food source for the developing young.

The rate of evolutionary development of a species is variable and the type of development depends on the selective forces operating in that particular evolutionary environment. Broadly speaking, morbidity-induced death can be biologically adaptive in two quite different ways, depending on the selective forces operating on the species. Under conditions of high intraspecific competion for scarce resources (e.g., K-selection), if weaker or less successful individuals get ill and die, then this leaves more resources for the strong (Christian, 1980). Thus, illness could be a mechanism of redistributing resources to the more biologically able individuals of a species. An example of this first type of biological advantage is provided by the trout *Salmo gairdneri* where members of the species low down in the social hierarchy are immunosuppressed compared with those higher up in the hierarchy (Peters, Faisal, Lang, & Ahmed, 1988).

A second form of presenescent functional death occurs where intraspecific co-operation is necessary for survival, that is, where individual survival depends on co-operative strategies (e.g., a particular type of r-selecion). Under such circumstances, if individuals who do not fulfil their co-operative role get ill and die, the survival chances of the remaining group members is enhanced.

Whether illness is functional in a competitive or co-operative environment, the only possible distal signal for morbidity inducing processes would be behavior or its antecedent, psychological state. Thus, in either type of evolutionary environment psychological states could be associated with morbidity and mortality because they signal biological utility.

Human evolution has passed through a variety of selective environments, some involving intraspecific competition for scarce resources and some involving high levels of co-operation. Examples of intraspecific competition are provided by Neolithic and postneolithic cultures and they may also have occurred at much earlier periods of evolution. However, high levels of intraspecific co-operation are primarily associated with the Paleolithic.

The environmental stress of the Paleolithic (which included glaciation, drought, and high winds) would have resulted in conditions which are biologically harsh for humans and which could only have been survived through co-operative behavior. Modern anthropological equivalents show that in harsh environments co-operation is needed for both intragroup (local) activities such as hunting, moving camp, and storing food, as well as intergroup (regional) activities such as providing food supplies for neighbors

affected by a catastrophic food shortage (Binford, 1983; Gamble, 1986; Geist, 1978). By implication, individuals who detracted from the harmonious operation of group activities in Paleolithic times would have threatened the survival of the group and hence of their own genes.

The anatomically modern humans of the Upper Paleolithic, the Upper Paleolithic people, differed from the Neanderthals of that time (i.e., living between 50,000 and 40,000 years ago) in terms of longevity. Whereas there is no evidence of Neanderthals living longer than about 40 years (the same as the maximum life expectancy of zoo-maintained chimpanzees), there is evidence of contemporaneous Upper Paleolithic people living well into their sixth decade (Trinkaus, 1986; Trinkaus & Thompson, 1987), and, of course, the modern maximum life expectancy (which is genetically determined) is just over 100 years.

As the existence of a significant postreproductive period in animals is extremely unusual (Pianka, 1983), it is likely that longevity in Upper Paleolithic people evolved because it was biologically adaptive. Elderly Upper Paleolithic people must have had an important social role, possibly as a resource of knowledge, or as educators of the young, or as mediators in intragroup and intergroup behavior. Whatever their role, two conclusions can be drawn about this particular evolutionary development. The first is that elderly people could have had a socially beneficial role only in a group which was characterized by harmonious relations. The second is that relatively few elderly people would have been needed to fulfil this role, and, given that elderly people are physically less able, there would have been a selective advantage if the number of elderly people was kept low.

In sum, the arrival of elderly people coupled with the stressed Paleolithic environment has a general implication for the selective forces governing human evolution at that period: that there is a selective advantage if people who do not fit into an appropriate role within a harmoniously co-operative group get ill and die. In particular, selection of redundant postreproductive people would be necessary to ensure that the group does not carry the burden of feeding those who do not contribute to the survival of the group.

By contrast, at the end of the last glacial, the selective pressures for human survival would have changed dramatically. Not only was the environment less harsh but also, with beginning of agriculture, there was a greater density of people and hence the possibility of intraspecific competition for scarce resources (Binford, 1983). More recent historical times provides good evidence of one group being aggressive and competitive and, therefore, surviving at the expense of some other group. Thus, whereas during the Paleolithic co-operativeness had biological utility, in Neolithic and later times competitiveness had biological utility.

Illness could have evolved a function either in a co-operative, Paleolithic environment or in a competitive Neolithic and postneolithic environment,

and in both cases biological utility and hence the presence or absence of illness would be signalled by psychological states. However, the particular function of illness would differ between these two environments as would the psychological states which would be indicative of biological utility.

In a Paleolithic environment, states indicating that a person is making a useful contribution within a group (group-oriented success) should signal biological utility and health whereas states indicative of interpersonal conflict should signal lack of biological utility and disease. By contrast, in a Neolithic or postneolithic environment, states indicative of interpersonal competitive success (individualistic success) should signal biological utility and health whereas states indicative of 'being at the bottom of the pecking order' should signal lack of biological utility and disease.

Starting from these two different sorts of predictions, it is possible to examine the data to see whether the psychological state-morbidity relationship fits better the Paleolithic or Neolithic/postneolithic pattern. As there is some controversy over which psychological states are related to morbidity as well as the specificity of these states to particular diseases, I shall simply list psychological states which have been identified, at least by some researchers, as healthy and unhealthy and without specifying the type of physical disease.

Unhealthy psychological states include depression (Jemmott & Locke, 1984), particularly states associated with bereavement (Jones, 1987), loneliness (Lynch, 1977), anxiety (Friedman & Booth-Kewley, 1987), hostility and anger (Friedman & Booth-Kewley, 1987), inability to express emotion including inability to express hostility (Shaffer, Graves, Swank, & Pearson, 1987), suspiciousness (Barefoot, et al. 1987), and time pressure (Wright, 1988). Healthy psychological states include social support (Seeman & Syme, 1987), optimism (Scheier & Carver, 1987), affiliation motivation (Jemmott, 1987) and effective coping styles including hardiness, commitment and control (Hull, van Treuren, & Virnelli, 1987).

Although some of the healthy/unhealthy psychological states, such as, depression and optimism, fit equally the Paleolithic and Neolithic/postneolithic models, other psychological states are consistent only with the Paleolithic model. For example, high levels hostility should be healthy according to a Neolithic/postneolithic model as hostility would increase individualistic success but unhealthy according to a Paleolithic model as hostility would reduce group co-operation. Similarly, only the Paleolithic model is consistent with the finding that social support and affiliation motivation are healthy.

Perhaps some of the clearest evidence in favor of the Paleolithic model comes from research into the Type A behavior pattern, where potential for hostility (Dembroski & Costa, 1987) and time pressure (Wright, 1988) are believed by some researchers to be the pathological components. The Type A behavior pattern, which can be characterized as an aggressive individualistic

preoccupation with success, is adaptive in our capitalist postneolithic society, and is often viewed positively by personnel managers. Yet the Type A behavior pattern is physically unhealthy, indicating that this particular psychological state-morbidity relationship could not have evolved under conditions similar to the present.

In sum, although there are inconsistencies in the data relating psychological states to physical illness, there is no data which exclusively shows that the psychological state-illness relationship evolved during Neolithic or postneolithic times but there is (albeit controversial) data which exclusively shows that this relationship evolved during the Paleolithic. Thus, the total pattern of evidence supports the hypothesis that the psychological state-morbidity relationship evolved during the Upper Paleolithic and either that selective forces were insufficiently strong to effect a change in Neolithic/postneolithic times or that the necessary chance genetic mutations to effect such a change simply did not take place.

If, as I suggest, the relationship between psychological states and morbidity evolved through illness having a function, what mechanism underlies this relationship? Let us start from the methodological assumption that psychological states are 'identified' with (possibly unknown) physiological substrates and the physiological substrates then have causal effects on other physiological states (Hyland & Kirsch, 1988; Kirsch & Hyland, 1987). A possible mediating mechanism between psychological states and morbidity is likely to have two characteristics. First, it should entail physiological substrates which are known to be closely related to psychological states; and, second, because evolutionary change tends to be economical, it should involve any existing nonpsychologically-mediated mechanism which controls the occurrence of illness.

In all complex organisms, senescence is controlled by the time-based expression of pathological genes, where, typically, these pathological genes are timed to be expressed when fecundity decreases (Pianka, 1983). Thus, the increase of various diseases in old age is not simply a passive process of the body 'wearing down' but represents an active process whereby the expression of pathological genes (such as oncogenes) is delayed until old age. Gene expression is known to be controlled by neurotransmitters and by hormones, both biochemicals being associated with psychological states.

Thus, if illness evolved a function through psychological signals of biological utility, then it seems likely that this function would have evolved through the already existing mechanism of gene expression. Specifically, I propose that psychological states can alter the way in which pathological genes respond to an internal clock. Psychological states can elicit the expression of pathological genes at times well in advance of normal senescence. Similarly it may be possible that psychological states can delay the expression of pathological genes.

The functional theory of illness suggests a new way of looking at illness, death and their psychological antecedents. Although at an individual level death is a negative event, from a perspective of the species as a whole, death is a necessary and, therefore, beneficial part of evolution. The functional theory of illness takes a species level view of mortality to suggest that the timing of death is not accidental and that psychological factors affect that timing. Although the psychological state-illness relationship is no longer biologically adaptive, our genotype reflects the evolutionary forces which shaped human survival in a much harsher environment when survival of our species was crucially dependent on co-operation.

References

Barefoot, J. C., Siegler, I. C., Nowlin, J. B., Peterson, B. L., Haney, T. L., & Williams, J. B., Jr., (1987). Suspiciousness, health and mortality: a follow-up study of 500 older adults. *Psychosomatic Medicine, 49,* 450-457.

Binford, L. R. (1983). *In pursuit of the past.* London: Thames & Hudson.

Christian, J. J. (1980). Endocrine factors in population regulation. In M. N. Cohen, R. S. Malpas, & H. G. Klein (Eds.), *Biosocial mechanisms of population regulation* (pp. 55-115). Yale: Yale University Press.

Dembroski, T. M., & Costa, P. T. (1987). Coronary prone behavior: Components of the Type A pattern and hostility. *Journal of Personality, 55,* 211-235.

Friedman, H. S., & Booth-Kewley, S. (1987). The "disease-prone personality": A meta-analytic view of the construct. *American Psychologist, 42,* 539-555.

Gamble, C. (1986). *The Paleolithic settlement of Europe.* Cambridge: Cambridge University Press.

Geist, V. (1978). *Life strategies, human evolution, environmental design.* New York: Springer-Verlag.

Hamilton, W. D. (1964). The genetical evolution of social behavior: I and II. *Journal of Theoretical Biology, 7,* 1-52.

Hull, J. G., Treuren, R. R. van, Virnelli, S. (1987). Hardiness and health: A critique and alternative approach. *Journal of Personality and Social Psychology, 53,* 518-530.

Hyland, M. E., & Kirsch, I. (1988). Methodological complementarity with and without reductionism. *The Journal of Mind and Behavior, 9,* 5-11.

Jemmott, J. B. (1987). Social motives and susceptibility to disease: Stalking individual differences in health risks. *Journal of Personality, 55,* 257-298.

Jemmott, J. B., & Locke, S. E. (1984). Psychosocial factors, immunologic mediation, and human susceptibility to infectious diseases: How much do we know? *Psychological Bulletin, 95,*78-108.

Jones, D. R. (1987). Heart disease mortality following widowhood: Some results from the OPCS longitudinal study. *Journal of Psychosomatic Research, 31,* 325-333.

Kirsch, I., & Hyland, M. E. (1987). How thoughts affect the body: A metatheoretical framework. *The Journal of Mind and Behavior, 8,* 417-434.

Lynch, J. J. (1977). *The broken heart: Medical consequences of loneliness.* New York: Basic Books, New York.

Peters, G., Faisal, M., Lang, T., & Ahmed, I. (1988). Stress caused by social interaction and its effect on susceptibility to *Aeromonas hydophila* infection in rainbow trout *Salmo gairdneri. Diseases of Aquatic Organisms, 4,* 83-89.

Pianka, E. R. (1983). *Evolutionary ecology.* New York: Harper & Row.

Scheier, M. F., & Carver, C. S. (1987). Dispositional optimism and physical well-being: The influence of generalised outcome expectancies on health. *Journal of Personality, 55,* 169-210.

Seeman, T. E., & Syme, S. L. (1987). Social networks and coronary artery disease: A comparison of the structure and function of social relations as predictors of disease. *Psychosomatic Medicine, 49,* 341-354.

Shaffer, J. W., Graves, P.L., Swank, R. T., & Pearson, T. A. (1987). Clustering of personality traits in youth and the subsequent development of cancer among physicians. *Journal of Behavioral Medicine, 10,* 441-447.

Trinkaus, E. (1986). The Neandertals and modern human origins. *Annual Review of Anthropology, 15,* 193-218.

Trinkaus, E., & Thompson, D. D. (1987). Femoral daphyseal histomorphometric age determinations for the Shanidar 3, 4, 5, and 6 Neandertals and Neandertal longevity. *American Journal of Physical Anthropology, 72,* 123-129.

Wright L. (1988). The Type A behavior pattern and coronary artery disease. *American Psychologist, 43,* 2-14.

TESTS VERSUS CONTESTS:
A THEORY OF ADJUDICATION

Warren Thorngate and Barbara Carroll

SUMMARY: Adjudicated contests are often held to determine who merits a limited resource. Attempts to employ consistent and fair criteria of merit are vitiated by increases in the contestant population; as fair contests grow, they eventually devolve into unfair ones. Contestants can use any of three strategies to adapt to this devolutionary circumstance. Psychological aspects of these strategies are outlined, and their limitations discussed.

To increase our understanding of human behavior and experience it is necessary, though not sufficient, to increase our understanding of the circumstances in which they occur. It is rarely easy to understand fully these circumstances because their definition is ultimately subjective and thus prone to disagreement. Even so, certain types of circumstances do enjoy high definitional consensus; they are labelled with the same words, and appear to evoke or direct their own distinctive set of actions and reactions. Included here are many behavioral settings (e.g., classroom, kitchen), social roles (student, parent), projects (child rearing, graduate work), and life events (marriage, divorce).

Social scientists have studied a few circumstances extensively. For example, there are extensive literatures describing educational and organizational settings, meetings and marriages, and the behavior and experience generated therein. Many more circumstances have not been as thoroughly investigated. Included here are large numbers of situations traditionally considered the domain of moral philosophers or economists, and derived from the necessity of dividing resources among people who desire them. Resources have often been allocated by aggression, intimidation, wealth, privilege, or power. As we have come to embrace liberal political philosophies, however, we have come to endorse the principle that resources should instead be divided according to rights, accomplishments, or need. In acting on the principle, we have invented a new class of situations for assessing how much people merit or deserve. These situations are collectively known as *adjudications*.

The basic structure of an adjudication is relatively simple: people who desire a limited resource present their cases for receiving it to presumably neutral judges who weigh the merits of each case and distribute the resource accordingly. As evidenced by the proliferation of application forms, adjudications have become very popular. Indeed, they now affect the lives of almost everyone. For example, adjudications are now conducted to allocate grades, jobs, promotions, parole, loans, housing, welfare, licenses, trophies, leading

roles, research grants, journal space, credit cards, medical operations, daycare spaces, and charity dollars.

Adjudications are supposed to be fair, and to reach this goal of procedural justice, adjudicators are expected to apply the same reliable and valid criteria in judging everyone. If adjudicators use different, unreliable, invalid or arbitrary criteria to judge people, then their adjudication is likely to be unfair; some people will be given what they do not merit or deserve, while others will be denied their due. Most of us recognize that completely fair adjudications may never occur. Judges are human and their criteria, measures and judgments may never be infallible. Of course, some of the resulting mistakes may be worse than others, and classic political debates have sprung from contrary beliefs about the morality of inclusive and exclusive errors. In response, many people have tried to improve measures of merit or desert. Playing fields have been leveled. Jurisprudence has been refined. Psychologists have striven to increase the reliability and validity of measurement instruments.

Yet with the proliferation of adjudications has come the proliferation of complaints about them. Many of the complaints may be dismissed as expressions of envy, tactics of maintaining self-concept, sour grapes. Many more, however, are based on legitimate concerns about the results of adjudication. We all have stories of injustice: jerks with jobs and good workers without them; dross accepted and gems rejected for publication; adjudications held only to justify decisions previously and unfairly made. In response to concerns about including the undeserving, attempts are often made to increase the stringency of selection — usually by adding hurdles in the resource race. In response to concerns about excluding the meritorious, attempts are often made to increase the supply of resources — often by borrowing from future generations. The historical waxing and waning of these concerns and responses to them define seasons of conservatism and liberalism. Their opposition suggests an important dialectical relation between the limits of resources and the limits of the adjudicative process.

Tests Versus Contests

Adjudications come in two basic forms: tests and contests. *Adjudicated tests* provide a desired resource to every person who reaches some minimal test standards. Ideally, for example, a driver's license is given to all people who pass a driving test, welfare is given to all those who show sufficient need, education is offered to all who merit it, and justice is given to all who show injustice done. In such situations, the test standards remain fixed and the resource expands (or contracts) to accommodate those who meet them. Fallible measures may cause adjudicators to make mistakes, sometimes giving the resource to those who do not merit it, and sometimes withholding the resource from those who do. However, an improvement in the measures will

reduce both errors, and with perfectly reliable and valid measures fairness will prevail.

In contrast, *adjudicated contests* provide a desired resource only to those who are judged to do better on the relevant test than anyone else. Most competitions are adjudicated contests: judges give medals to athletes who go faster, farther, higher; recording contracts are given to the most marketable musicians; jobs are first offered to those with the best looking resumes. In most adjudicated contests the number of people who receive a resource remains fixed, and test standards are raised (or lowered) to accommodate the number of people desiring it. Here too, fallible measures may cause adjudicators to make mistakes. But unlike adjudicated tests, improvements in the measures of adjudicated contests provide no guarantee of increasing fairness.

In the past few years we have undertaken several computer simulations to explore some of the variables that affect the outcomes of adjudicated contests (Thorngate & Carroll, 1987). Three variables have received special attention: the number of contestants, the validity of the criterion measure (test/true score correlation), and the contest structure (e.g., round robin, hierarchical elimination by rounds, seeding). Our simulations show that contest outcomes are dramatically affected by the relation between test validity and contestant population. For example, if the correlation between test score and true score on some measure of merit is as high as $r = +0.90$, and if 10 people with normally distributed test scores submit them for adjudication, then the chances that the person with the highest true score will win the contest is about 80%. If 100 people enter the competition, then the chances that the test score winner will have the highest true score drops to about 40%. If 1,000 people enter the competition, these chances plummet to less than 20%. The winner is almost always *among* the best contestants. But in crowded contests, small measure errors have magnified effects, and the best person rarely wins (see also Einhorn, 1978).

These findings demonstrate how sensitive even fair contests are to what some call the problem of the diminished range. Correlations calculated over a wide range of test and true scores become smaller when the range is diminished. Even fallible indicators can correctly distinguish the genuinely awful from the truly outstanding. Yet even slightly imperfect indicators can lose their discriminatory power when employed to distinguish the excellent from the outstanding. Assuming that outstanding contestants are rare, we can expect that few will appear in small contests, and thus that they will be easily distinguished from the rest. In large contests, however, we can expect several outstanding contestants, and thus expect that choosing the best will be a far more arbitrary and error-prone task.

Our simulations also show that, although structure has no consistent effect on contest outcome, it does have a dramatic effect on the tasks of judges

and contestants. Judges can be easily overwhelmed by the information generated in even moderately large contests; examining the applications or watching the performances of even 50 contestants in a 'one-shot' competition, for example, can exceed many judges' attentional capacities or discriminatory powers. As a result, such contests are usually conducted in rounds of smaller competitions. Local, first rounds are held to cull those of undetectable merit. Survivors continue to subsequent rounds until they too are eliminated. By keeping each round relatively small, the attentional burden of judges is reduced and, it is hoped, the quality of their judgments is correspondingly increased.

Yet what is good for the judges is not generally good for the judged. One-shot contests by definition require all contestants to compete only once. Hierarchical eliminations require the winners of all but the final round to compete again, and thus to sacrifice more of their lives to the adjudicative process. Because only winners of the previous round compete, each round is more difficult to win than the last. Losers drop out, and by doing so reduce the range of merit. Thus, winners become more alike, more difficult to distinguish, and more subject to measurement or judgment errors. In large, multi-round contests, first round winners can usually feel confident they do indeed have more merit than the losers. In the final found, few contestants can count on more than luck.

The Devolution of Contests

Many adjudicated contests are held repeatedly — sports have yearly playoffs; granting agencies hold annual competitions; graduate schools recruit every spring. As a result, some contests may change over time according to the nature of contestants and to the amount of a contested resource they desire. If resources multiply but contestants do not, then we might expect adjudicators to relax their criteria of merit and give more people what they want. If contestants multiply but resources do not, then we might expect adjudicators to become more stringent in their judgments and more exclusive in their allocations. The latter possibility holds some danger for the survival of fairness, and thus we believe it deserves further study.

An adjudicator's first task is to make distinctions. We have noted that when more people enter a contest, the number who merit a prize is bound to increase, and so is the difficulty of distinguishing winners from losers. In repetitive contests this difficulty is likely to become more acute over time. Losers often note how the winners won, and strive to mimic the winning performances. If two or more contestants do so successfully, then their performances will no longer distinguish the winner, and judges will be forced to use additional criteria that will. Thus, the criteria used to select winners will change over time. Eventually judges will exhaust the supply of fair

criteria, and only unfair criteria will remain to choose winners from all those who deserve to win (Thorngate, 1988).

In sum, our studies lead us to conclude that if resources do not multiply as fast as contestants, then fair contests eventually devolve into unfair ones. Many contests (e.g., for journal space; see Thorngate, in press) have already devolved, and others are sure to follow. How do people adapt to these circumstances? Many strategies are possible. Some ignore unfairness, some attribute unfairness to the adjudicators, and some acknowledge the devolution of adjudication criteria.

Adaptation Strategies

Strategies based on ignorance of unfairness. Judging from our own experiences with graduate school applicants, many contestants are remarkably ignorant of the fallibility of judges and the devolution of adjudication criteria. They assume that judges know what they are doing, and that criteria never change. They believe that success will be guaranteed by reaching minimal standards, and often spend considerable effort attempting to reach them. In essence, these contestants confuse contests with tests and though their strategy is a good one for passing tests, it is likely to be maladaptive for winning contests, especially repeated or hierarchical ones.

As we have noted, talented contestants have a good chance of winning opening rounds of hierarchical contestants, and a decreasing chance of winning subsequent rounds. If they lose, contestants who deny that judges or adjudication criteria change over time are liable to attribute their loss to themselves. Rewarded in the preliminary rounds, they are likely to respond by investing even more time and effort into increasing their merit in future contest appearances. As they do so, they raise their expectations and fuel their perfectionistic tendencies. Other pursuits are likely to be sacrificed; contestants may narrow the focus of their lives, link their identity to their contest performance, and equate their self-worth with that performance.

These rising expectations and self-worth equations are likely to have serious personal consequences when adjudication criteria shift or devolve. Winners of local contests or first rounds who remain unaware of changing criteria will not adapt to the changes, and their chances of winning will decline. Sooner or later it may become maladaptive to continue, and more adaptive to search for other sources of reward. But if the decline is slow and inconsistent, contestants with their egos or identities on the line may succumb to the pathologies of partial and diminishing reinforcement, and persist in contesting long after it is adaptive to do so.

There are two possible reactions to this destructive cycle. One is to cheat. As research with children has shown, when one's performance cannot keep pace with extremely high expectations and when failures disappoint

oneself or others, the motivation to cheat is high and cheating often occurs (see, e.g., Pearlin, 1971). For those who do not cheat but continue to judge their self-worth by contest performance, clinicians suggest that their contest losses are likely to lead to procrastination (in an attempt to avoid further evaluation), to a deep sense of personal failure, and eventually to depression (see, e.g., Burka & Yuen, 1983).

Strategies based on the perception of unfair judges. Not all contestants deny the fallibility of adjudicated contests. Some do perceive that such contests are not always fair but seem unaware that adjudication criteria devolve. As a result, they fail to appreciate that a degree of unfairness is inherent in the adjudication process, and maintain the belief that all contests can and should be fair. If they lose, they are likely to blame the judges or the criteria that judges employ.

Contestants who hold these views may become angry with the perceived unfairness. They are likely to feel persecuted, and with repeated, unfair adjudications may exhibit a form of learned helplessness. These contestants are likely to deny any personal responsibility for their loss. They will, therefore, not be motivated to improve or adjust their performance, even when such changes may be beneficial. They will likely become contest drop-outs, and their talent may go to waste.

An alternative strategy is to press for fairer adjudication criteria. Many women who have lost job competitions have then tried to eliminate selection criteria that discriminate on the basis of sex. Such a proactive approach can be effective in small contests where adjudication criteria have long been clearly unfair. It will be less effective in contests so crowded that judges use unfair criteria because they can no longer distinguish contestants using fair ones.

Strategies based on acceptance of the devolution of the adjudication process. While some contestants focus on eliminating unfair criteria, others assume that some unfairness is inevitable, especially as contests devolve. They do not expect judges to employ invariant criteria, nor do they assume that the best person will always win. Instead, they expect an increasing degree of judgment fallibility, at least in the final rounds of large contests, and attempt to adapt themselves accordingly.

Some contestants who accept the devolution of adjudicative criteria in large contests do not attempt to change or exploit it. Instead they are most likely to respond with resignation and to adjust their expectations of themselves. Often they may seek smaller and less competitive contests to reward their merit. In effect, they choose to be big fish in small ponds, rather than dead fish in large ponds. Small contests typically attract fewer good contestants, and have more stable adjudicative criteria. As our simulations suggest (Thorngate & Carroll, 1987), this will increase the best person's chances of

winning some reward, even though it will likely be smaller than the reward of winning a larger contest.

Other contestants who accept the devolution of criteria used to judge merit in large contests may attempt to make the devolutionary process work in their favor. Many may try to anticipate the criteria that judges will use in the next adjudicated contest, then groom themselves accordingly. Judges of some contests (e.g., research grant competitions) may willingly provide information to contestants about changes of adjudication criteria. Judges of other contests (e.g., torts) many be offended by requests for 'inside information', or may give incorrect information. Because the devolution of the adjudication process is often haphazard, the forecasted criteria may deviate from those used.

Instead of asking judges, contestants may try to anticipate criteria for some future contest by extrapolating from shop talk or from gossip about previous winners and losers. This practice, while widespread, is not always useful. Hot tips are often based on a small and perhaps biased sample of acquaintances, or on a flourish of rumors. Neither source is generally reliable, and the personal costs of acting on information gleaned from either will be great when contestants devote much time and energy to improving themselves in irrelevant ways.

Contestants may also attempt to manipulate adjudicative criteria to serve their own interests. Hard-working 'C' students may request professors to consider effort in future grading schemes. Rich contestants may offer poor judges bribes. If many contestants attempt such manipulations, a new 'meta-contest' will evolve, and its winner will become the winner of the original. Contests become political in this way.

Practical Suggestions

Can judges in recursive fashion adapt to the contestants' adaptations in ways that would promote fairer contests? The fairness of a contest is ultimately limited by the amount of contested resources. Yet within this limit, the suffering of at least some contestants might be attenuated if judges and contestants acknowledged the arbitrariness of the adjudication process. This could be achieved, for example, by selecting winners for the final round of large contests at random so no loser need claim inferiority, and no winner could claim superiority. It could also be achieved by distributing resources more equally among finalists so no single best contestant, but many good ones, would receive rewards.

Adoption of the above measures could have advantages for society as well. The measures may decrease emphasis on judging individual achievement by social comparison, and promote more realistic self-expectations. With less emotional investment in a contest, and with more time available to

pursue other things, talented individuals may use their talent in a greater variety of ways. Society could then gain more of the benefits of talented losers who are now distinguished from winners by little more than luck.

References

Burka, J., & Yuen, L. (1983). *Procrastination: Why you do it and what you can do about it*. Reading, MA: Addison-Wesley.

Einhorn, H. (1978). Decision errors and fallible judgement: Implications for social policy. In K. Hammond (Ed.), *Judgment and decision in public policy formation* (pp. 142-169). Boulder, CO: Westview Press.

Pearlin, L. (1971). *Class context and family relations: A cross national study*. Boston: Little Brown.

Thorngate, W. (in press). The economy of attention and the development of psychology. *Canadian Psychology*.

Thorngate, W. (1988). On the evolution of adjudicated contests and the principle of invidious selection. *Journal of Behavioral Decision Making, 1*, 5-15.

Thorngate, W., & Carroll, B. (1987). Why the best person rarely wins: Some embarrassing facts about contests. *Simulation and Games, 18*, 299-320.

A GENERAL MODEL FOR INTERINDIVIDUAL COMPARISON

Gunnar Borg

SUMMARY: Problems in interindividual comparisons of perceptual intensities are discussed both from a philosophical and a psychophysical point of view. A theoretical model (the range model) is presented. In this model maximal perceptual intensities are set equal for different individuals in spite of the fact that the corresponding physical intensities may vary greatly. The intensity of a perception is evaluated depending upon its position in the total range from zero (or a minimal intensity) to a maximal subjective intensity (also considering the type of growth function in question). An empirical test of the model has been performed in the area of effort and exertion. High correlations have then been found between estimations of perceived exertion and corresponding heart rates. The range model may also have a more general application in psychology and physiology for most kinds of 'interprocess' comparisons.

One of the oldest problems in theoretical psychology concerns the subjectivity of human perception. The general problem deals with the universal metaphysical mind-body question and the relations between physical events and mental events. The interpersonal problem deals with questions on the possibility of knowledge about other minds and similarities in mental events.

The Philosophical Problem of Interindividual Comparisons

The epistemological problem on 'intersubjectivity' and 'sameness' in perception has a long history, but psychology does not provide a working theory or model for interindividual comparisons. The main questions to be answered are: How can I think that another person perceives the world in about the same way as I do? How can our belief in others' minds be justified?

A person's perception of an event depends upon the psychical stimuli, but also upon the sensory system, previous learning and experiences, language acquisition and motivation. We can, therefore, never know for certain what another person perceives. We can never creep under the skin of another person and see with her eyes or hear with her ears.

Philosophers have always discussed this epistemological problem and tried to solve it or to dissolve it. The skeptic has argued that it is impossible to state anything about other minds. The challenge, then, is to present reasons for assumption of 'sameness' in mental events for different individuals. According to common sense it is quite natural to go from 'privacy' to 'publicity', from first person account to third person account.

The classical philosophical attempt to defend the common sense point of view is the argument from analogy. Several philosophers, among others Bertrand Russell (1948), have argued along these lines. The reasoning goes as follows: if I in a certain situation react in a certain way with a certain perception, then another person in the same situation showing the same behavior should also have the same perception. This attempt of justification does not convince the skeptic and there does not seem to be a consensus among philosophers. A review of these and related problems is given by Sagal and Borg (1989).

Similarities of Sensory Intensities

A naive philosophical assumption of 'sameness' in sensory perception gives us a plausible explanatory framework for other people's mental events and a foundation on which to build a model for evaluations of perceptual intensities of different individuals. From this simple and first order assumption it is then also possible to tackle problems of individual differences. Sight and hearing changes with age and a ten kilo weight cannot be equally heavy to a child as to a weightlifter. In this context 'sameness' does not mean perfect equality, nor does it mean 'sameness' in physical events or distal stimuli. 'Sameness' refers to subjective states and relations between events. A tree may look bigger to a child than to an adult, but the subjective size-relation to a bush may still be the same.

Psychophysical Intensity Evaluations

The psychophysical scaling methods are constructed to measure perceptual intensities on ratio scales, thus enabling quantitative descriptions of the relations between subjective and physical intensities. The methods worked out, especially by S. S. Stevens and collaborators at Harvard (see Stevens, 1971), have been found to function quite well for rough intermodal comparisons. These methods are, however, not perfect ratio scales, since the results obtained by these scales are influenced by several response biases. This weakness is, however, of no major concern to the content of this article.

A more difficult problem concerning the ratio scaling methods is their fundamental weakness with regard to interindividual comparisons. According to these methods, it is possible to say that one perceptual intensity is, for example, four times more intense than another sensation. However, it is not possible to say if a sensation is 'strong' or 'weak'. With these methods we cannot obtain any direct level estimates for interindividual comparisons. We do not have an interindividually valid unit of measurement. We cannot determine the measure constant of the psychophysical function in a meaningful way for each person. We cannot draw two psychophysical functions for two different individuals in the same diagram.

To demonstrate the inadequacy of ratio scaling methods for inter-individual comparisons, let us give an example using magnitude estimation (ME). One common way to use ME is to select a certain stimulus intensity as a standard and call that for example '10'. If another stimulus intensity is perceived to be three times as intense as the standard, the subject is supposed to say '30'. Individual functions may thus be obtained which all pass through the same fixed standard. However, there is no way to justify that the standard is perceived to be equally strong for each subject. The same difficulty is obtained with 'free ME'. Stevens (1971) compared his perceptual scales with physical scales and claimed that in psychophysics the scale unit is arbitrary in the same way as in physics. This is, however, not the case, since the physical units are 'public', while the perceptual are 'private'. The latter may vary over individuals. Often we do not know how big they are, or even how to determine them. Our fundamental problem is to find reliable and valid units of measurement permitting interindividual comparisons.

The Range Model for Interindividual Comparisons

A possible solution to this fundamental problem was first proposed by Borg (1961, 1982). It was suggested that a maximal subjective intensity should be used as an interindividually valid point of reference. Previously some medium intensity level or the absolute threshold, as in dB-measurements, have been used as a fundamental unit. The drawback in this way of scaling is that we do not know the upper limit. We cannot say whether 5 times or 20 times the threshold means a strong intensity or not. To do so we need the total range from the threshold to the maximal intensity.

One perceptual modality for which it is possible to determine a maximal perceptual intensity is the perception of effort and exertion in heavy physical work. For healthy people it is possible, without pain, to strain themselves to a 'maximal' exertion. This subjective intensity is very clear and well-defined and seems to be fairly equal. A certain submaximal intensity is then evaluated according to its position in each individual's range from the minimum to the maximum, also considering the typical growth curve in question, for example, the exponent.

Figure 1 shows how the range principle works. In the figure, the two functions represent different individuals (1 and 2). The curves are drawn from the starting point of the function (a/b) to a maximal (terminal, t) value (R_t/S_t) according to the general expression of the psychophysical and physiological response function (Borg, 1961, 1978; Mountcastle, Poggio, & Werner, 1963):

$$R = a + c(S - b)^n$$

where c is the measure constant relating perceptual or physiological magnitude (R) to stimulus intensity (S). The constants a and b may also stand for the absolute threshold R_O and S_O.
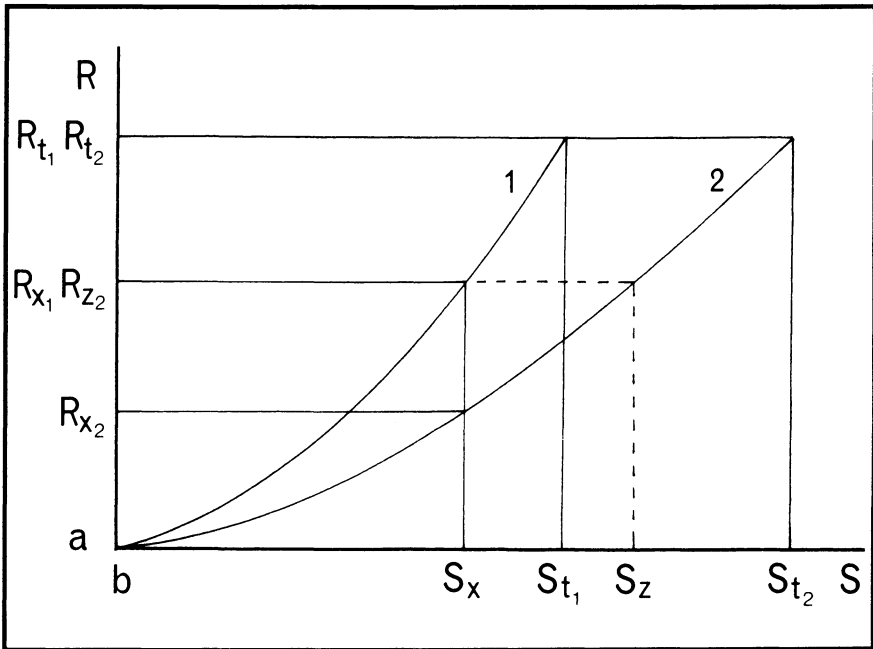
Figure 1. The figure shows psychophysical functions for two subjects (1 and 2). S is the physical intensity (e.g., weight in kg), and R is the perceptual intensity. S_t denotes a maximal intensity (e.g., the heaviest weight a person can lift) and R_t the corresponding subjective intensity, which is set equal for both subjects according to the range theory (see text).

The positively accelerating functions in Figure 1 represent perceived exertion in physical work. Subject 1 is the stronger of the two and can perform (e.g. lift weights) 50% more than subject 2. When both subjects are exerting themselves to their maxima, their perception of effort is assumed to be about the same. In this way it is possible to make interindividual comparisons for any intensity level, for example S_x. As a unit of measurement, the maximal intensity or a fraction of it may be used. The measure constant can then be determined for each individual:

$$c = \frac{R_t - a}{(S_t - b)^n}$$

In most cases both a and b are zero, except for some physiological variables and a few perceptual ones. For the perception of exertion in walking (with S referring to speed, km x h^{-1}) positive values are obtained for both of

these constants. The constant c can be assessed for each individual, since R_t-a is set equal for everyone and $(S_t-b)^n$ is determined empirically. By using the thus obtained values of c relative response measurements may then be calculated.

Empirical Tests of the Range Model

The range model for interindividual comparisons has been tested against some physiological criteria. In the field of physical exertion, subjects have had to estimate the degree of perceived exertion from a very low intensity to one close to maximum. Maximal performances were determined and predictions were made according to the range model. Estimations of perceptual intensity levels were then correlated with measured heart rates. High correlations were found; they could not have been obtained if corrections according to the range principle had not been made. For a detailed account of these studies, see Borg (1962) and Marks, Borg, & Ljunggren, 1983).

Conclusion

The range model for interindividual comparisons of perceptual intensities has proved to work quite well in the modality of perceived exertion in physical work. The same model may also be applied in other areas in psychology and physiology, for example in studies of mental load where 'raw' physiological measurements, that vary greatly over individuals in absolute ranges, are used without any corrections or only corrected for a basic intensity level. The range model has the potential to be a unified model for many different kinds of 'interprocess' comparisons. The model also contains interesting philosophical problems in the theory of knowledge that should be further analyzed.

References

Borg, G. (1961). Interindividual scaling and perception of muscular force. *Kungliga Fysiografiska Sällskapets Förhandlingar, 12*, 117-125.

Borg, G. (1962). Physical performance and perceived exertion. *Studia Psychologica et Paedagogica, Series altera, Investigationes XI*. Lund: Gleerup.

Borg, G. (1978). Subjective effort in relation to physical performance and working capacity. In H. L. Pick, H. W. Leibowitz, J. E. Singer, A. Steinschneider, & H. W. Stevenson (Eds.), *Psychology, From Research to Practice* (pp. 333-361). New York: Plenum Publishing Corporation.

Borg, G. (1982). A category scale with ratio properties for intermodal and interindividual comparisons. In H. G. Geissler, & P. Petzold (Eds.), *Psychophysical judgment and the process of perception* (pp. 25-34). Berlin, GDR: VEB Deutscher Verlag der Wissenschaften.

Marks, L. E., Borg, G. A. V., & Ljunggren, G. (1983). Individual differences in perceived exertion assessed by two new methods. *Perception and Psychophysics, 34(3),* 280-288.

Mountcastle, V. B., Poggio, G. F., & Werner, G. (1963). The relation of thalamic cell response to peripheral stimuli varied over an intensive continuum. *Journal of Neurophysiology, 26,* 807-834.

Russell, B. (1948). *Human Knowledge: Its Scope and Limits. Part VI*, Ch. 8.

Sagal, P. T., & Borg, G. (1989). The range principle and the problem of other minds. Manuscript submitted for publication.

Stevens, S. S. (1971). Issues in psychophysical measurement. *Psychological Review, 78,* 426-450.

TOWARD A THEORY OF HUMAN COMMUNICATION

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SUMMARY: This paper presents the outline of a theoretical perspective in terms of which it should be possible to discuss the manner in which two or more human minds can interact through the medium of language. It presents an overall schematic diagram of the 'communicative situation' within which such events take place. It takes, as its normative point of departure, the dyadic relationship between a single speaker and hearer in face-to-face dialog and treats all other situations as derivative from this, requiring allowances to be made for perceived differences. 'Meaning' is discussed as an activity engaged in by the speaker rather than as an inherent property of the medium used for communication. Similarly, 'understanding' is the fundamental activity of the hearer in which he attempts to understand the speaker's intended message behind the utterance rather than some meaning *in* the utterance. The conditions necessary for a consideration of these activities are presented. It is generally argued that language production yields extended discourse so that the usual focus on word or sentence 'meanings' is both insufficient and misguided. It is only by focusing on the mental activities of speakers and hearers that we will develop a substantive theory of both meaning and understanding.

In commenting on the papers presented at the last ISTP conference in Banff (Baker, 1988) I suggested that, while the various presentations reflected a wide variety of views, we all seemed to be searching for a common language through which we could explore the differences represented. If there is to be any integration of our scientific community, it must begin through the development of a linguistic community. This is consistent with the relativization of science and the recognition of the impossibility of separating the scientist from his own manner of discussing his theories.

Vygotsky (1986/1934, p. xviii), commenting on the "crisis in psychology" in the '30's (we always seem to be having one of those in psychology) suggested that the same fact, being discussed within the languages of the different schools of psychology, becomes a different fact. In the extreme, what is a fact from one perspective, is a fiction from another, for example, Freud's concept of the Id from the perspective of psychoanalysis or behaviorism.

Much of the concern with respect to the philosophy of science in the post-positivist era is clearly a concern with language, and this is made abundantly clear in Gergen's (1988) presentations as well as others. The putative 'cognitive revolution' has raised the issue of language use to center stage both with respect to the participants in our experiments and their language data, and with respect to how they and experimenters communicate. But despite all

of this, we have given relatively little thought to developing a clear theoretical foundation for this most ubiquitous of all forms of human behavior – the use of language. The strong concern with the problem that clearly existed in Europe from the 1880's to the 1930's, from Wundt to Karl Bühler (see Blumenthal (1980) for an excellent review of this otherwise unnoted history in the English literature of our field) and, to a somewhat lesser extent, into the modern era seems to have had little impact on modern psychology which is so dominated by the American literature.

I would like to present you with what I deem to be a fundamental set of considerations which must be taken into account if we are to develop such a theory and, hopefully, take you some distance toward such a theory. I take it as a given that the basic, irreducible unit for analysis in this domain is the dyadic relationship which comes to exist between a given speaker and a given hearer. In addition, I take the Vygotskian position that language is initially learned in a social context, that is, as a consequence of the face-to-face interaction of two separate minds trying to communicate with each other through the use of language. All other uses of language I consider to be derivative with respect to that and to require discernable 'allowances' for deviations from this normative situation. Thus we make allowances for lack of face-to-face contact when we speak over a telephone, when we are writing for an audience, but without knowing how they are reacting as they read our material, in our 'inner speech' while talking to ourselves, and so on.

I have developed a diagram for you in which I try to indicate the nature of the 'communicative situation' analogous to what Rommetveit (1974, p. 25) has referred to as "an intersubjectively established social reality" that comes into existence when speaker and hearer become aware of each other and an inference is made by the hearer that the speaker has an intention to communicate. (Intention, here, is with respect to motivation, not the philosophical version.)

These mutually aware individuals bring to this situation all of their own cognitive apparatus — their beliefs, their views of the world, their current moods and attitudes, and their assessments of the situation in which they find themselves. All these elements are inescapably present in their interaction, and they govern, in very significant ways, how the speaker will formulate an utterance and how the hearer will interpret it. Almost by definition this makes each communicative situation unique to the dynamics of each dyad, to each unique speaker and hearer at some specific place and moment in time, but this does not make generalizations impossible, nor does it bar the development of a reasonable theoretical perspective toward what they are doing. However, it does interpose considerable 'distance' between these generalizations and the unique events. We will, nevertheless, pursue the generalizations.

THE COMMUNICATIVE SITUATION

Current State of the Speaker's Mind

    Beliefs, General and about Specific Situation   (b)

    Memory for Preceding and Current Context   (c)

    Motivation and Intentions   (d)

    Available Linguistic Skills and Devices   (e)

        Intended Message (m)

        Message Structure $[(I_s (I_r (I_d)))]$

        Lex./Syn. Structure of Utterance (x)

        Motor Plans and Production

Utterance and Its Physical Environment (y)

Current State of the Hearer's Mind

    Beliefs, General and about Specific Situation   (b')

    Memory for Preceding and Current Context   (c')

    Motivation and Attention   (d')

    Available Linguistic Skills and Devices   (e')

        Basic Sensory and Perceptual Acquisition

        Perceived Lex./Syn. Structure (x')

        Inferred Message Struc. $[(I_{s'}(I_{r'}(I_{d'})))]$

        Construed Message (m')

        Evaluation of (m')

Conventional psychological and linguistic approaches to this domain often make the mistake of trying to consider the speaker or hearer in isolation. Such an individualistic conception misses the crucial point that speakers do not just formulate utterances; they formulate utterances for a specific audience, and that formulation can vary considerably, for the same intended message, for discernably different audiences. Hearers, in interpreting utterances, are strongly influenced by any and all available beliefs about the specific individual who produced that utterance. Another major mistake is to focus on single words or single sentences. It is quite rare that any significant attempt at communicating restricts itself to such impoverished units. Language normally occurs as extended discourse. One may be able to extract a single sentence from this presentation and make some reasonable guesses as to what the author intended, but a full *understanding* would come only when it is embedded in the entire presentation. Worse yet, isolated words and sentences are often treated as if they had, as an intrinsic property, 'meanings'. I will seriously argue that words or sentences do not *have* meanings. There are, of course, apparently fixed 'conventional' meanings, but even these have their force only because speakers and hearers, in the absence of explicit contextual cues to the contrary, impose these readings onto the given lexical forms. Thus, even these conventional meanings are not properties *of* the words themselves. The forms can be used by speakers in order to mean something, but that is an *act* on the part of the speaker, not a *property* of the utterance itself. How these forms can also come to be meaningful for the hearer is another aspect of the problem we are addressing here. My major concern here is to eliminate the belief in what Rommetveit (1987) calls "eternal sentences", sentences that are believed to have the same, identical 'meaning' no matter who uses them and in any and all possible situations.

Let us look more closely at the diagram I have given you. I assume that the speaker has an intended message and an intention to communicate that message to a specific audience. Thus it follows that the speaker will tailor the utterance to suit his assessment of what he believes the audience requires; he does not do it simply to satisfy an appropriate match for himself between the intended message and the utterance. If we ever hope to follow the usual scientific desire of 'predictability' with respect to his behavior, that point is an essential ingredient.

I have labelled the intended message as m, and I first want to consider what is necessary to make the move from m to y, the utterance. I begin with the assumption that *thinking* and *meaning* are two different although interrelated activities. (These views are reflected in both Vygotsky, 1986/1934, and Hörmann, 1981/1976.) Thinking can lead to the development of an intended message. Meaning is the act of realizing that intended message in an utterance. Meaning generally entails the use of language even though other forms of communication are possible. Thinking often takes a linguistic form in the educated adult, but this is not *necessarily* the case. Language, once

acquired through social interaction, becomes a very convenient, but not a necessary tool in the service of thinking (again, following Vygotsky). Thus, m is characteristically pre-linguistic while x and y take on a linguistic form intended to represent or, possibly better, to approximate the message.

Below m is a line I will refer to as the 'information structure' of m. I have no substantive reason for postulating its existence beyond the view, following Wundt (Blumenthal, 1980, pp. 9-33), that the intended message is a unit which must, of necessity, be analyzed into parts which are realizable in any language system. Such an analysis is a consequence of the properties of the medium into which the message must be placed, but it need not be a property of the message itself. These comments touch significantly on the more general problem of 'mental representation', but we cannot pursue that issue here. I would note in passing, however, that there often appears to be an unfortunate confusion between the demands of the medium for the communication of ideas (here, the form of the language system) and the manner of representation of those ideas in the mind. This leads to an unnecessary and, in my mind, incorrect equation of thinking with the use of language. Let me set that issue aside and return to my diagram.

Briefly, three general classes of information are present in an intended message: denotational (I_d), relational (I_r), and sentential (I_s). Denotational information is generally realized as noun or verb phrases, relational by syntactic devices such as word order, case markers, and so forth, and sentential by sentence patterns indicating declarative, interrogative, or other general sentence forms. However, this 'information' is still pre-linguistic; how each component will be realized is, of course, language specific and leads to the development of x, the linguistic realization of m. That realization must then be externalized or uttered in, generally, either speech or writing. It is clear that the specific formulation of x is not totally independent of the specific, anticipated medium, but we will gloss over that here. Through either motor plans for speech or for writing, the utterance is realized as an external event.

It should be obvious that the exact form that the utterance takes will be strongly constrained by the speaker's beliefs about the audience and situation, and by his stock of linguistic skills, to say nothing of the clarity of his thoughts with respect to the intended message. It seems rather evident that the specific utterance does not, in itself, convey a great deal of insight into many of these factors but, on the other hand, an astute observer can often gain more information from the situation than the speaker may have intended to convey in the utterance.

The speaker has an obvious advantage over the hearer in the communicative situation in that he has the intended message in mind; he knows, *a priori*, the goal of the act. The hearer must take in utterances and try to build up a possible interpretation as he goes along. Much of this will be tied to how closely linked the speaker and hearer are in the communicative situation, and

how familiar the hearer is with the general nature of the material being presented. From the utterances taken in, the hearer must attempt to approximate the speaker's x with his x'

Out of x' he constructs an m', his interpretation of what he believes the speaker intended to convey to him. Beyond that, he evaluates m' with respect to his own world views, beliefs, and so on in order to make decisions about credibility, consistency, or whatever. Perfect communication would result when $m = m'$, but it does not necessarily follow that the hearer, even in that ideal situation, must believe or accept what he has come to understand as the speaker's intended message. Thus, there is an evaluation of m' once the hearer believes he has understood the speaker's intended message.

When we consider this general outline, the problems of the communicative situation are reasonably obvious. I have labelled the general conditions within the interacting individuals as b to e for the speaker, and b' to e' for the hearer. To the extent that these coincide, communication is clearly facilitated, but it need not be the case that they must be identical. It is only necessary that speaker and hearer be aware of, and thus able to make allowances for, mismatches in any of these. We must fully appreciate how dynamic the communicative situation is, how it is continually redefined and modified through protracted discourse as speaker and hearer alternate roles and explore each other's minds in order to develop the possibility of understanding. None of these parameters are rigidly fixed. In a sense, one may view serious discourse as an attempt to alter these parameters, generally to change beliefs or motives. To the extent that mismatches in these parameters go undetected, unexplored in the interchanges between speaker and hearer, adequate communication can be prevented or, at least, made more difficult or less clear.

I take it as a given that thinking and meaning (the latter in the sense of attempting to convey an intended message through language) are independent though related activities. Thus, at the more specific level of the move from m to y by the speaker, m is pre-linguistic and not necessarily propositionalized. But m must be structured into something analogous to a propositional form if it is to be realized in language because that is precisely the form demanded by the medium to be used. Depending strongly on the nature of the intended message, such a reformulation may be quite simple or quite unsatisfactory. The formulation is dependent upon the clarity of m, the speaker's linguistic skills, and possible limitations imposed by his assessment of his audience. Here again we can realize the reasons for protracted discourse, the use of many utterances and apparent redundancy as the speaker searches for forms that will work within the specific situation.

The movement from the speaker's x through y to the hearer's x' is generally not problematic, given clear speaking or writing. But once we move beyond that to the hearer's analysis, we again see the burden placed on the hearer to not just perceive x' but to fathom the intended message behind it,

the *m'* which is an estimate not so much of what the utterance contained as it is an estimate of what the speaker intended when he produced that utterance.

If we focus too narrowly on individual words or individual utterances, we can easily conclude that adequate communication is virtually impossible. If we fail to appreciate the impact of role-switching between speaker and hearer and the value of each party knowing what it is to be in the position of the other, we will fail to grasp the dynamics of the acts of communication. But if we take the much broader perspective of the communicative situation, and see language events, utterances, as embedded in this and being used by speaker and hearer to dynamically redefine many of the parameters of that situation — in other words, if we view the use of language in extended discouse as a means of exploring and even modifying the minds of speaker and hearer — we see the possibility of success in communication as coming out of that very high level of interaction. It is only the very narrow view of meanings *in* words or *in* specific utterances that makes it all seem impossible.

Further, the role of context must be appreciated, but context must not be construed in the reified notion of the preceding text as such. Context, too, is psychologically defined. It plays a part only if it is retained in the mind of the hearer. He determines what will be active from the past in influencing the processing of the current utterance. Again, it is clearly an error to assert that a sentence *has* a given context. The hearer brings his understanding of the context to a given utterance, and uses it to help in his understanding of that utterance.

This has been a necessarily brief overview of a very complex problem, but I hope that I have provided you with sufficient information to cause some modification of some of your beliefs and attitudes, and that you have produced relatively sympathetic evaluations of your estimates of my intended message. I look forward to your discussion.

References

Baker, W. J. (1988). The current direction of theoretical psychology. In W. Baker, L. Mos, H. Rappard, & H. Stam (Eds.). *Recent trends in theoretical psychology* (pp. 367-372). New York: Springer-Verlag.

Blumenthal, A. L. (1980). *Language and psychology: Historical aspects of psycholinguistics.* (Rev. ed.). New York: Krieger.

Gergen, K. J. (1988). The concept of progress in psychological theory. In W. Baker, L. Mos, H. Rappard, & H. Stam (Eds.). *Recent trends in theoretical psychology* (pp. 1-14). New York: Springer-Verlag.

Hörmann, H. H. (1981/1976). *To mean — to understand: problems of psychological semantics.* New York: Springer-Verlag.

Rommetveit, R. (1974). *On message structure: a framework for the study of language and communication.* New York: Wiley.

Rommetveit, R. (1987). Meaning, context, and control. *Inquiry, 30*, 77-99.

Vygotsky, L. S. (1986/1934). *Thought and language.* Cambridge, MA: The MIT Press.

WHAT IS PSYCHOLOGIC?

Jan Smedslund

SUMMARY: Psychologic is an attempt to explicate and systematize the implicit common sense psychology embedded in everyday language and taken for granted by its users. The resulting system consists of definitions, axioms, and logically derived theorems and corollaries. It allows one to distinguish clearly between conceptual relationships which follow necessarily from the definitions of the terms involved, and empirical relationships which can only be determined through observation. The distinction between conceptual and empirical relationships allows one to avoid *pseudo-empirical* research, that is, studying logically necessary relationships by empirical methods, and to test one's procedures through comparing outcomes with what follows logically from assumed conditions. Four major objections to psychologic are discussed under the headings of *contextuality, intrinsic contestability, prototypicality*, and *lack of universality* of the basic concepts. It is concluded that, without the support of psychologic, empirical psychology remains a pseudoscience.

The point of departure for the project of psychologic is the world of everyday psychological terms and modes of explanation and prediction. By virtue of having acquired ordinary language and a given culture, every person possesses a rich set of concepts and ways of understanding herself and her fellow human beings. Whatever the shortcomings of this common sense or folk psychology, it is sufficiently rich and flexible to allow people to live together, and to explain and predict each others experiences and behavior with a reasonable amount of success. No academic system of psychology even approaches this achievement. Hence, it is not unreasonable to focus scientific attention on the conceptual structures of common sense psychology, as they are embedded in implicit form in the discourse of everyday life.

Undertaking this task, one must *explicate*, that is, formulate directly, that which has up to now remained *implicit*, that is, unformulated and merely taken for granted. This explication is necessary, because only then can we communicate with some precision about *what* we are observing. Being explicit and clearly defining our concepts also allows us to discriminate clearly between those relationships which are conceptual, that is, follow necessarily from the formulated definitions and assumptions, and those which are empirical, that is, can only be determined through observation. Failure to make this distinction between the conceptual and the empirical may lead either to unquestionably *pseudoempirical* research (e.g., studying whether or not all bachelors actually *are* unmarried and male) or, more frequently, to research where one does not know to what extent the findings are genuinely empirical and to what extent they merely reflect logical-semantic relationships (Smedslund 1987, 1988b).

Also, explicitness is necessary for the strict evaluation of every psychological *procedure*. If a procedure attempts to establish the necessary and sufficient conditions of a given phenomenon as it is explicitly defined, and the phenomenon does not occur, then one *knows* that the procedure did not work. If one attempts to establish the necessary and sufficient conditions of surprise, defined as the state of a person who has experienced something the person had expected *not* to occur, and the person denies having become surprised, one *knows* that the procedure has failed to introduce the required conditions *or* to register correctly the actual state of the person. One must, then, explore various possible explanations of the findings, such as that the person did not really expect the event not to occur, failed to recognize the event in the proper way, was dominated by other overruling concerns, really was surprised, but lied to the psychologist, and so on.

In summary, it is not possible to do psychological research, whether it consists in looking for empirical regularities, or in evaluating procedures, without having an explicitly formulated conceptual system. Since the only system which allows people to live with each other is that which is embedded in ordinary language and culture, it appears to be worthwhile to try to make it explicit. This is the project of psychologic.

Although it is not known to what extent the project can be realized, it appears reasonable to require that the outcome approximately fulfills at least the following two basic conditions: a) the explicated system should be logically well formed, and b) it should correspond to what is taken for granted as necessarily true by all competent members of a given culture or all competent speakers of a given natural language. Condition a) simply means that propositions and their proofs should be formulated as carefully and stringently as possible. Condition b) means that one should try to formulate the propositions of the system in such a way that they yield approximations to *consensus* within the given population. To the extent that the preceding two ideal requirements are attained, psychologic may be said to mirror *the set of logical implications taken for granted by all members of a given culture or speakers of the language of that culture.*

It is apparent that decisions about the extent to which the project of psychologic is feasible must rest on the outcome of attempts to carry it out. Up to the present, one first major presentation of psychologic has appeared (Smedslund, 1988a), as well as several earlier fragments (Smedslund, 1978, 1981, 1982b, 1984) and two additional papers are finished (Smedslund, unpublished a & b). This work has produced a considerable corpus of definitions, axioms, theorems, and corollaries, which, although their level of formalization is variable, can probably be developed in the direction of ever better logical form, increasingly extended derivations, and higher levels of consensus. As to the latter, there have been a few attempts to estimate directly the degree of consensus about parts of the system in the general

population (Smedslund 1982a, 1982b, unpublished b). High degrees of con-
sensus have been found in all the mentioned studies. Hence, the results are
encouraging, even though the methods used and the interpretations ventured
may be subject to criticism. Finally, the metatheoretical status of psychologic
has been compared to that of traditonal empirical psychology (Smedslund, in
press).

It remains to mention and respond briefly to four particularly fundamen-
tal criticisms which may be directed at the project. These may be briefly
referred to by the headings *contextuality, intrinsic contestability, prototypicality,*
and *lack of universality.*

The first criticism points out that the meaning of terms varies with their
context. Every term can be made to mean anything, given sufficient manipula-
tion of the context. If this is true, a general psychologic becomes impossible
since the terms in any given proposition will vary in meaning according to the
given context and no general derivations can be made. Psychologic may be
defended in two ways against this kind of attack. Firstly, and most important-
ly, although it is true that the meaning of a term varies with the context, it is
not true that it varies indefinitely. Context influences the meaning of a text,
but the text also influences the meaning of its context. Psychologic is based
on the assumption that central psychologically relevant terms in ordinary
language have a stable *core meaning*, which may be explicitly defined by means
of other terms, which also have stable core meanings, and so on. The
feasibility of this assumption cannot be decided in advance, but must be tried
out. The second defense as regards contextuality is that all propositions in
psychologic have been given the standard form 'Person P in context C at time
t'. All derivations are given relative to this form, hence holding person,
context and time constant, and acknowledging that logically necessary in-
ferences cannot be made across varying contexts. This corresponds to a
similar, generally recognized limitation in everyday folk psychology. One is
usually reluctant to rely unquestioningly on inferences from a person's ex-
perience and acting in one set of circumstances to what a person might
experience and do under entirely different circumstances.

The second criticism which may be directed at psychologic was formu-
lated by John Shotter (in press) referring to an earlier article of Gallie
(1955-1956) who wrote: "... This is what I mean by saying that there are
concepts which are essentially contested, concepts the proper use of which
involve endless disputes about their proper uses on the part of their users."
However, even if it is conceded, for the sake of the argument, that many
psychologically relevant terms belonging to psychologic may, in fact, be of this
type, that is, may be essentially contested, this does not preclude that they may
have stable and uncontested core meanings too. In fact, it is hard to under-
stand how a term can give rise to disputes if it does not have some un-

disputable core meaning. Disagreement can only occur on a background of agreement.

A third criticism directed at psychologic is that the meaning of basic psychological terms in ordinary language cannot be expressed by means of classical definitions specifying necessary and sufficient conditions, but must be seen as *prototypes*. Prototype definitions are of the kind 'X is a Y if at least n of the following N criteria are fulfilled'. This is not a strong objection since it merely moves the classical definitions one step down to the components. However, if one insists on a prototypical definition of the components too, and of their components, and so on, the system rapidly becomes hopelessly complex and unmanageable.

A final and fourth objection to psychologic is that its propositions cannot pretend to be universally and eternally applicable. The answer is that this is obvious. Psychologic is an explication of the implicit system embedded in a natural language and a culture. As the language and culture changes so must its psychologic. However, such changes are by necessity slow and small. See Valsiner (1985) and Smedslund (1986).

Concluding remarks: the project of psychologic, as briefly outlined here, appears to be unavoidable if we want to establish a scientific psychology in the minimal sense of fulfilling two basic requirements. One of them is that one can distinguish between statements expressing conceptual and statements expressing factual relations. The other is that one can test the adequacy of one's procedures, by comparing empirical outcomes with those derived logically. As seen from this point of view, contemporary empirical psychology, which has remained conceptually inexplicit, is a pseudoscience.

References

Gallie, W. B. (1955-1956). Essentially contested concepts. *Proceedings of the Aristotelian Society, 56,* 167-198.

Shotter, J. (in press). Illusion and contest in 'social scientific' understanding: The scope and limits of Jan Smedslund's 'geometry' of common sense. In K. J. Gergen & G. Semin (Eds.) *Everyday understanding: Social and scientific implications.* London: Sage.

Smedslund, J. (1978). Banduras theory of self-efficacy: a set of common sense theorems. *Scandinavian Journal of Psychology, 19,* 1-14.

Smedslund, J. (1981). The logic of psychological treatment. *Scandinavian Journal of Psychology, 22,* 65-77.

Smedslund, J. (1982a). Seven common sense rules of psychological treatment. *Journal of the Norwegian Psychological Association, 19,* 441-449.

Smedslund, J. (1982b). Revising explications of common sense through dialogue: Thirtysix psychological theorems. *Scandinavian Journal of Psychology, 23,* 299-305.

Smedslund, J. (1984). What is necessarily true in psychology? In J. R. Royce & L. P. Mos (Eds.) *Annals of Theoretical Psychology, 2* (pp. 241-272. New York: Plenum.

Smedslund, J. (1986). How stable is common sense psychology and can it be transcended? Reply to Valsiner. *Scandinavian Journal of Psychology, 27*, 91-94.

Smedslund, J. (1987). The epistemic status of interitem correlations in Eysenck's Personality Questionnaire: the a priori versus the empiri-cal in psychological data. *Scandinavian Journal of Psychology, 28*, 42-55.

Smedslund, J. (1988a). *Psycho-logic.* Heidelberg & New York: Springer-Verlag.

Smedslund, J. (1988b). What is measured by a psychological measure? *Scandinavian Journal of Psychology, 29*, 148-151.

Smedslund, J. (in press). Psychology and psychologic: characterization of the difference. In K. J. Gergen & G. R. Semin (Eds.) *Everyday under-standing: social and scientific implications.* Beverly Hills: Sage.

Smedslund, J. (unpublished a). The psychologic of forgiving.

Smedslund, J. (unpublished b). The conditions for being angry.

Valsiner, J. (1985). Common sense and psychological theories: the historical nature of logical necessity. *Scandinavian Journal of Psychology, 26*, 97-109.

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