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THE RELATIONSHIP BETWEEN BIOLOGY AND PSYCHOLOGY*

> In the middle of the Decade of the Brain we need to tackle some of the difficult, confusing issues about the logical relationship between biology and psychology.
>
> (Miller, 1996, p. 619)

> Much controversy remains about where biological phenomena fit into psychological science and vice versa.
>
> (Miller & Keller, 2000, p. 212)

> Biologists know what a brain is, but they are as confused as ever about the mind.
>
> (Lewontin, 2001, p. 105)

The relation between biology and psychology is an issue of broad significance for psychology as well as for many matters of social policy. Consider, for example, a recent article in the *Journal of Law, Medicine, and Ethics* that considered the question of whether genetic evidence for an association with the diagnosis of anti-social behavior is sufficiently predictive to be useful for prevention, intervention, and rehabilitation (Carey & Gottesman, 2006).

Over the past ten to fifteen years I have been enormously impressed with the gains made in biology. In contrast with many parts of psychology, recent developments in molecular biology and neuroscience are based on foundation concepts and accepted research methods. Although a while ago my cell

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biologist friend Ira Black told me that he thought his field was as problematic as my own, I have a hard time believing that to be the case. Before I retired, we had three faculty members teaching introductory personality with almost no overlap in books used or content presented. I can't imagine that being the case in a comparable introduction to biology or neuroscience.

Recognizing the importance of these developments, the eighth edition of my personality text (Pervin & John, 2001) for the first time included a chapter on "Biological Foundations of Personality." At the same time that I was developing this chapter, I was thinking about the broader issue of the relationship between the fields of biology and psychology, leading to a chapter entitled "Mind, Brain, and Behavior: Psychology, Biology, and the Question of Reductionism" in the third edition of my book *Current Controversies and Issues in Personality* (Pervin, 2002). In part such thought was stimulated by a proposal in the Rutgers department of psychology to split the department in two. One part would consist of the biopsychology-behavioral neuroscience and cognitive science groups, the other part to consist of social, personality, and developmental psychology. The status of clinical psychology was left undefined. There were various reasons for consideration of such a split, but one component was the feeling among some members of the biopsychology-neuroscience group that they represented the "true" science of psychology, the future of the field. Although for some such a split seemed strange, the point was made that many other universities had a department of psychology and one of neuroscience, or some similar division. At other universities there were life sciences programs, with the behavioral neuroscience psychologists being closely allied with other members of the program, in particular biologists.

For various reasons, the split in my department did not occur and harmony among us as a family of psychologists was established. However, a short time thereafter a candidate was interviewed for a position in the department. The candidate gave a lecture in their area of expertise and a possible appointment was considered at a subsequent faculty meeting. The candidate was recommended by members of the neuroscience group but the following sentiment was expressed by a number of members of the department: "We went to the lecture but could not understand what was being said. Is this psychology? Perhaps this person is a more likely candidate for an appointment in the Biology department."

Much of what I have to say is influenced by recent developments in the field and by two noteworthy books that consider many of the relevant issues—*In Search of Memory* (2006) by Eric Kandel and *An Argument for Mind* (2006)

by Jerome Kagan. Both are autobiographical accounts that also trace the history of developments in their respective areas, cell biology and developmental psychology, over the past half century. Of particular note, Kandel (2006) describes his early commitment to psychiatry and psychoanalysis and his subsequent commitment to research on the brain. He describes how during the 1950s his supervisors were heavily oriented toward psychoanalysis and away from psychopharmacology. The use of drugs was discouraged because they were viewed as interfering with the psychotherapeutic process.

Similarly, Kagan (2006) describes how he was "indoctrinated" into the behaviorist and psychoanalytic emphasis on the environment and the "dismissal" of biology. With time he was able to "unshackle" himself from the prejudices against biology held by both theoretical positions and to not only appreciate the biological point of view but to emphasize it in his studies of temperament. He describes the painful transition as follows: "Years after Francis Crick and James Watson had published their famous paper describing the structure of DNA, more than fifty years ago, Crick published a series of lectures. One paragraph summarized his belief that future research would reveal that brain neurochemistry had a major influence on human behavior and its variation. I wrote in the margin of that page, 'No!' Stubborn facts have forced me, kicking and screaming, to relinquish the pleasing premise of biology's irrelevance that attracted me to psychology so many years ago" (Kagan, 1960, p. 193). Although still committed to viewing himself as a psychologist interested in "mind," Kagan, at this point in time, is so fascinated with the biological that he notes the following in relation to individual differences in temperament: "If a genie were to appear and grant me an answer to only one question, I would ask this generous spirit 'What is the inherited neurochemistry that contributes to this quality'?" (Kagan, 2006, p. 193).

Jumping ahead to what should perhaps be part of the conclusion to this talk, Kagan makes a prediction about the future of the field—it will fission into two fields, one the study of the biological foundations of sensation, perception, etc., and the other the study of human emotions, beliefs, culture, etc. The two fields will use different concepts and measures. Of particular interest is his viewing this development as analogous to Harvard's earlier split into two departments, Psychology and Social Relations. Since currently the two are together as parts of one Department of Psychology, if Kagan's prediction comes about, in some sense the field will have come full circle.

SOME ALTERNATIVE VIEWS OF THE RELATION BETWEEN PSYCHOLOGY AND BIOLOGY

At least three views can be considered concerning the relation between the fields of psychology and biology—the two are separate and completely independent of one another, the two are competing views and one (psychology) can be reduced to the other (biology), and the two represent different levels of explanation with the scientific goal of establishing links between the two levels. In some cases a view is expressed in terms that clearly fit within one of these alternative views. In other cases the view is more nuanced and appears not to fit readily into any one alternative view.

Psychology and Biology as Separate Disciplines

According to the view that psychology and biology are separate disciplines, one can not move from analysis, understanding, and explanation in terms of one to analysis, understanding, and explanation in terms of the other. At its extreme, the view of psychology and biology as completely separate endeavors reminds one of Descartes' mind-body dualism. Few, if any, psychologists or biologists would take such a Cartesian view. However, some border on it in terms of their emphasis either that the phenomena of mind cannot be reduced to the processes of the body or, on the other hand, that the phenomena of mind are of little scientific interest in and of themselves, mere epiphenomena that are secondary to the underlying processes. As we shall see in the next section, some biologists come close to stating the latter position. And, perhaps as a response to such biological imperialism, some psychologists come close to stating the former position.

Bandura (2001) and Kagan (2006) have articulated the view that different vocabularies are necessary for psychological and biological analyses of the phenomena of interest. Bandura (2001), for example, expresses concern with the threat of reducing the psychological to the biological. He states that processes of the mind (e.g., cognitive processes) are emergent phenomena that differ qualitatively from the neural events that are part of them and can not be reduced to these neural events: "Emergent properties differ qualitatively from their constituent elements and therefore are not reducible to them…Mapping the activation of neuronal circuitry subserving Martin Luther King's "I Have a Dream speech would tell us little about its powerful socially inspirational nature" (Bandura, 2001, pp. 4, 19). He draws the anal-

ogy of the properties of water such as fluidity, viscosity, and transparency not being reducible to the components of hydrogen and oxygen. Similarly, Kagan (2006) suggests that just as chemistry is different from physics, it is not possible to analyze psychological phenomena in biological terms without losing important meanings: "The current vocabulary for neurons, transmitters, and circuits can never replace the psychological terms" (Kagan, 2006, p. 212).

It is not that Bandura and Kagan discount the importance of understanding brain and other biological processes, or the legitimacy of their investigation. Indeed, both have conducted research that relates psychological phenomena (e.g., self efficacy beliefs and temperament) to biological processes. However, it is their view that appreciation of biological correlates can not be accepted as a substitute for analysis and understanding of psychological phenomena on their own terms. As stated by Bandura: "Psychological principles cannot violate the neurophysiological capabilities of the systems that subserve them. However, the psychological principles need to be pursued in their own right" (2001, p. 19).

Another expression of this point of view is represented in Miller's (1996) presidential address to the Society for Psychophysiological Research. In that address Miller expressed concern that a "naively reductionistic" view of psychological concepts is prevalent. He suggested that "we need to tackle some difficult, confusing issues about the logical relationship between biology and psychology" (p. 619). and noted that there often seems to be an ideological war between the psychologically and biologically inclined researchers. He rejected the view that biology is more fundamental than psychology as well as such phrases as "biological underpinnings," "biological substrates," "neural substrates," and "physiological foundations": "Fundamentally psychological concepts require fundamentally psychological explanations" (Miller & Keller, 2000, p. 212). Remember that this was in a presidential address to a psychophysiological group.

Psychology and Biology as Alternative, Competing Disciplines: The Question of Reductionism

Miller's comments lead us to consider the second view, that of psychology and biology as alternative and often competing disciplines. This position often emanates from biologists who suggest that psychological terms can

and should be reduced to biological terms. For example, consider the following statement by a Princeton University biologist: "My feeling is that molecular biologists are going to move into psychology and take over the field. I think that's the way psychology is going to be rejuvenated" (Silver, quoted in Weiner, 1999, p. 243).

For many psychologists the term reductionism tends to have a negative implication, suggesting that something of value is lost or eliminated in the process of going from one group of units to another. Thus, for example, the argument of Bandura: "There is a growing unease about the progressive divestiture of different aspects of psychology to biology...It is feared that as we give away more and more psychology to disciplines lower down on the food chain, there will be no core psychological discipline left" (Bandura, 2001, p. 18). Miller similarly argues against the reductionistic explanation of psychological phenomena in biological terms and extends the issue as follows: "The worst consequences of the biology versus psychology war is the assumption that dysfunctions conceived biologically warrant interventions conceived biologically and similarly for dysfunctions and interventions conceived psychologically. This assumption is rampant in the popular press and common in prominent in scholarly works, but it is groundless" (Miller, 1996, p. 625).

In contrast with such views, in his book *Consilience* the biologist Wilson (1998) argues that the reduction of wholes and large units into smaller units makes for good science. According to him, we must first reduce the level at which we analyze phenomena and then work back toward synthesis. Although he accepts the view that there are different levels of explanation, each with its own laws and principles, he argues for biology as the most relevant discipline for unifying the life sciences. According to him, reductionism, followed by synthesis, is the primary and essential activity of science: "The love of complexity without reductionism makes art; the love of complexity with reductionism makes science" (p. 54).

I suspect that Wilson represents the kind of explanatory and disciplinary elitism that so troubles Bandura, Kagan, and Miller. It is not just that Wilson sees biology as unifying knowledge, it is that he also argues against holistic explanations and against cultural relativism. Thus, Bandura (2001) specifically links Wilson with a view of biological determinism that rejects the importance of culture. In his emphasis on the ability of humans to exercise control over the nature and quality of their lives, Bandura rejects what he views as Wilson's biological determinism and the rule of nature.

Markus (2004), in her president's column for the *Society for Personality and Social Psychology*, articulates the threat that many psychologists feel in relation to the neuroscientific model: "Did the students responsible for the Columbine shootings have abnormalities in their cingulate gyruses or did a tight knit small town create a set of conditions that made it difficult for these students to escape their excluded and stigmatized status? In treating anorexia, should we look inside the person or outside to the social norms regulating eating in a given social context" (p. 3)?

Competition between the psychological and the biological often morphs into unnecessary and unproductive explanations for phenomena of interest. The nature-nurture controversy has a long history in the field. Although advances in behavior genetics played a very useful role in bringing to the attention of psychologists the crucial role of genetic factors in personality, in partialing variance into genetic and environmental factors it also served to maintain a useless dichotomy—there never are genes without environments or environments without genes. The question of why children from the same family are so different (Plomin & Daniels, 1987) might have been asked along side the question of why identical twins from the same family are so different. As an aside, let me note here the problem of using only phenotypic data in such analyses. Such data may minimize the shared family environmental contribution to the development of personality. For example, one sibling may be greatly overweight and the other anorexic, or one may be a habitual procrastinator while the other compulsively gets things done on time, if not early. In both cases one can consider a possible shared environmental influence, anxiety about food and weight in the former and getting things done on time or perfectly in the latter, although phenotypically they look very different. The general systems concept of *equipotentiality*, that the same starting point, in this case shared family environmental influence, can lead to different outcomes, if only phenotypically different, applies here.

The issue of competing biological and psychological explanations perhaps comes up most dramatically in terms of the understanding and treatment of mental illness. Is alcoholism a biological disease or a psychological difficulty? Is weight gain a biological problem or a question of will power? Is mental illness a disease of the brain or a social and psychological problem? Luhrman (2000), in her book *Of Two Minds: The Growing Disorder in American Psychiatry,* suggests that psychiatrists have inherited a Cartesian dualism. This dualism is expressed in the division between psychodynamic psychiatrists who emphasize the treatment of the mind through psychotherapy

and the biological psychiatrists who emphasize treatment of the brain through drugs. According to her, these represent "two profoundly different notions of what it is to be a person: to feel, to choose, to do good, to have meaning" (p. 5). Kandel (2006) is critical of psychiatrists and psychoanalysts who early in his training viewed biology as irrelevant, a criticism with which I would agree. At the same time, one can be critical of the current situation where many psychiatrists receive no training in psychotherapy and where managed-care companies may more readily reimburse for pharmacological treatment than for psychotherapy—the former just costs less.

Currently I consult at a local psychiatric facility. At a recent case conference I was amazed to hear a psychiatrist ask the presenter, in this case a psychologist: "Why are you doing psychotherapy? The patient is receiving medication!" A follow-up question determined that he was not concerned that the medication might interfere with psychotherapy but rather that the psychotherapy was unnecessary—the medication was sufficient treatment for the depression!

Clearly, it is easy to slip from a view that all mental illnesses have a biological component, to the view that all mental illnesses have a biological basis, to the view that all mental illnesses should be treated pharmacologically, perhaps exclusively pharmacologically. However, such a sequence in thinking is neither logically nor scientifically warranted. Kandel (1998) suggests that every mental state is a brain state, and therefore that every mental disorder is a disorder of brain functioning, and therefore that treatments work by altering structure and function of the brain. However, he also suggests that disturbances of brain function can be caused by environmental events ("nurture" can impact upon "nature") and that psychotherapy may be effective in the treatment of mental illness, with different psychotherapies achieving their goals through different brain mechanisms by altering different parts of the brain. Psychotherapy and pharmacotherapy may each be effective and in similar or different ways produce changes in brain structure, brain functioning, and subjective well-being.

Most recently I am struck with debate concerning whether our understanding of biological factors "constrains" our work in the areas of personality and social psychology. Over a decade ago Cacioppo and Berntson (1992) wrote that "knowledge of the body and brain can usefully constrain and inspire concepts and theories of psychological function…" (p. 1025). I was greatly impressed with their view and took little note of this particular sentence. Indeed, I still see little that is problematic with it.

However, John Kihlstrom (2006), a psychologist for whom I have very high regard, is critical of what he perceives to be the suggestion that biological data constrain social psychological theory in the sense that the former level is somehow privileged: "Put bluntly, it betrays the idea that social psychology can't make theoretical progress without neuroscience" (p. 16), without what Kagan (2006) describes as data from the "High Church." Kihlstrom goes on to suggest that "Good social-psychological theories will make for good social neuroscience. After all, Psychology without neuroscience is still Psychology; but neuroscience without Psychology is just neuroscience" (2006, p. 17).

Some may see the debate as purely an issue of semantics—what one means by "constrain." However, I think that it goes beyond that to issues of competing points of view, of professional identity, and perhaps of which types of research should receive priority in funding. In terms of the latter, Steven Breckler, APA Executive Director for Science, expresses the following: "The dramatic shift in funding priorities at NIMH offers perhaps the best example of the pendulum swinging too far in the direction of reduction. NIMH, once the greatest protagonist of the biopsychosocial model of mental illness and health, is now paying little more than lip service to the social and behavioral systems in which mental health is embedded" (2006, p. 23).

Psychological and Biological Levels of Explanation

Despite the tendency toward bifurcation in terms of biological and psychological, there remain many who call for a multilevel, integrative framework. Such a framework rejects reductionism (i.e., the reduction of complex biological, psychological, and social illnesses to *strictly* biological elements) and mind-body dualism in favor of a multilevel, systems view. The person is treated as a whole, with observations at the various levels of organization (e.g., cell, tissue, organ, person, family, community) being recognized in terms of their distinctive contributions to the phenomena of interest. Thus, Kandel (1998) argues for an approach in which each level of analysis is recognized for its own contributions as well as for the potential contributions to understanding at other levels of analysis: "We now need to ask, How do the biological processes of the brain give rise to mental events, and how in turn do social factors modulate the biological structure of the brain" (p. 464)? From this perspective, the psychological and biological approaches are joined.

My sense is that the concept of levels is gaining increased usage in the personality literature (Ochsner & Lieberman, 2001). However, somewhat troublesome is the fact that generally there is not discussion of what is meant by levels of description, analysis, or explanation, or what kinds of relationships are possible among the different levels. Elsewhere I have discussed the alternative senses in which the concept of levels has been used in the personality literature (Pervin, 2002). Here I want to consider the concept of levels in the sense of units at lower levels being embedded in units at higher levels but with the units at each level having distinct properties. Thus, one can speak of interactions among, or causal connections between, the different levels. For example, we can consider the individual, group, and society levels of organization. Although a society is made up of many groups, and each group is made up of many individuals, each level has properties of its own. For example, conflict and coalitions among members within a group has no equivalent within the individual. Similarly, the concept of group cohesion has no direct equivalent at the individual level.

At the same time, one can speak of processes at one level having an impact upon processes at another level. For example, an individual can be disruptive to a group process and a group process can affect the psychological functioning of each individual in the group. The suggestion that the whole is different from, and perhaps greater than, the sum of the parts (e.g., a team is more than a collection of individuals) reflects the view that there are phenomena that may be unique to each level of analysis: "At each level of complexity entirely new properties appear, and the understanding of the new behaviors requires research which I think is as fundamental in its nature as any other...Psychology is not applied biology, nor is biology applied chemistry" (Anderson, 1972, p. 393).

As further illustrations of this model, consider illustrations from economics and biology. In economics we have macroeconomics and microeconomics, the former referring to the analysis of the economy as a whole, the latter to specific actions made by such groups as businesses, consumers, and governments. Decisions made at one level have implications for those at another level, and findings at one level have implications for analyses at the other level, but the two levels of analysis are distinct from one another and economists identify themselves as macro or micro economists.

In biology there are analyses at the molecule, cell, tissue, organ, system, and organism level. Again, events at one level can have implications for those at another level (e.g., tissue damage has implications for organ func-

tioning). Each level is accepted as an appropriate choice for description, analysis, and explanation. Biologists may make distinctions among one another (e.g., molecular biologists and cellular biologists), and in some cases separate departments have been formed, but it is accepted (hopefully) that different levels of investigation are more suitable for answering different questions. And, one would not say (hopefully) that one or another level of analysis is more fundamental, basic, or scientific than another. Each level of description, analysis, and explanation is better suited for different purposes, with the findings at one level having implications for understanding phenomena being considered at another level.

If psychological (mind) and biological (body) phenomena are considered to exist at different levels of analysis, then a variety of possible relationships among research at these levels can be considered. One possible relationship is where research at one level for the most part results in findings that duplicate those at another level. For example, an *f*MRI investigation found that emotional processing was more engaged in the solving of some judgments than others, essentially duplicating reports that people would have a harder time pushing a stranger off a bridge to save five people than hitting a switch that saves five people while killing one (Greene, Sommerville, Nystrom, Darley, & Cohen, 2001). In another study, involving investigation of the neurobiological basis of framing effects in a decision–making task, *f*MRI data indicated that presenting options in ways that engage emotional processes is associated with greater amygdala activity than presenting options in ways that do not involve such a framing quality (DeMartino, Kumaran, Seymour, & Dolan, 2006). In this case the results duplicated but did not extend earlier research based on the work of Tversky and Kahneman.

In a second type of relationship between levels, data from one level validate (i.e., confirm), invalidate (i.e., disconfirm), or clarify data from another level. For example, heritability data played a valuable role in invalidating the purely environmental model of schizophrenia as caused by a "schizophrenogenic mother" and the model of autism as caused by a "refrigerator mom." Similarly, neuroscientific findings invalidated the psychoanalytic view of the *phantom limb* experience as being due to use of the mechanism of defense of denial to deal with the psychologically painful experience of loss of a limb. In the addiction area, many discounted the view of addictions having a biological component, as in heroin addiction, because addictions such as gambling, shopping, and sex did not appear to have a comparable biological component. Discovery of the role of neurotransmitters in emotion

supported the view of a common biological component to all addictions. Note that this did not indicate that all addictions are exclusively caused by the action of neurotransmitters but rather that they could all have a common biological component, something that did not seem apparent in the earlier heroin model.

In a third type of relationship between levels, methods of research and/or data from one level extend or advance research at another level. For example, Kandel (2006) adopted the methods of classical conditioning to do his pioneering research on the cellular basis of memory. Observation in one area can help to pose questions for research at another level, as in the distinction between explicit and implicit memory as well as the difference between conscious and unconscious processes leading to research in the associated differences in brain mechanisms and structures (Kandel, 2006). Research in the area of health and illness illustrates the potential for work at each level advancing that at the other: "Social and biological explanations traditionally have been cast as incompatible, but advances in recent years have revealed a new view synthesized from these two very different levels of analysis" (Cacioppo, Berntson, Sheridan, & McClintock, 2000, p. 829). For example, work in the area of *psychoneuroimmunology* has advanced our understanding of how psychological factors (i.e., stress) and biological factors (i.e., immune system factors, genetic differences) contribute to diseases such as the common cold and heart disease (Ader, 2001; Cohen, Frank, Doyle, Skoner, Rabin, & Gwaltney, 1998; Kiecolt-Glaser, Page, Marucha, MacCallum, & Glaser, 1998). Cacioppo's model of a multilevel approach to the relation between social and biological explanations, as illustrated in his work on the relation between social support and health, provides another example of linking findings from different levels of observation (Cacioppo, Hawkley, & Berntson, 2003).

Finally, there is the work of Caspi and his colleagues on the relation between genetic factors and environmental influences that jointly contribute to the development of depression (Caspi et al., 2003). In each of these cases thinking and research at one level is tied to thinking and research at another level, to the potential enhancement of understanding of processes linking outcomes at each.

Multilevel research is tremendously complex. Not only are there complex relations among measures at each level but complex relations among measures between levels. For example, facial, self-report, and physiological

measures of emotion often do not correlate with one another (Kagan, 2006). Relationships between levels then may vary depending on which measures are used at each level. In addition, the same behavior may express different states, the General Systems Theory principle of *equifinality* (i.e., the same end-point can be reached from different starting points). As noted by Kagan (2006) in relation to different species: "A wolf urinating near a tree in a forest intends to mark his territory; a hiker does so because he or she is miles from a bathroom" (p. 121). As suggested by Zuckerman: "A psychobiological approach to personality is often accused of reductionism. This charge is usually baseless. All types of phenomena may be studied at different levels, from the most molecular to the most molar. Each level has its own methods, constructs, and limitations...The cognitive, behavioral, and biological are complementary and not conflicting modes of explanation. Great discoveries will occur at the borders of the different levels" (1998, p. 150).

CONCLUSION

Kandel (2006) started his career interested in becoming a psychoanalyst, and turned to biology in what he describes as at the time a naïve wish to find the basis for the id, ego, and superego in the brain. Although committed to reductionism, that is, an effort to understand the biological processes involved in all psychological phenomena, I do not read him as denying the value of studying phenomena at the psychological level. Rather than fighting a battle for scientific hegemony, psychologists and biologists, biopsychologists and behavioral neuroscientists, and those who go by other names might best focus their efforts on a multilevel approach to understanding the phenomena of interest to them.

For me, the intriguing question of the relation between biology and psychology is whether phenomena observed at one level can drive new questions to be asked at another level or questions to be asked in a different way. For example, can observations concerning consciousness and the unfolding of self lead to new questions about the organization of brain structures (Damasio, 1994)) or, conversely, can the discovery of mirror neurons (Gallese, Fadiga, Fogassi, & Rizzolatti, 1996) lead to our asking different questions about the development of consciousness and self? This seems to me to be truly the area in which work at the psychological and biological levels can enhance one another and go beyond battles concerning the value of reductionism and discipline imperialism.

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