Running Head: Biologization of Psychology

# Interpreting the "Biologization" of Psychology

Brent D. Slife, Brigham Young University

Colin Burchfield, United States Air Force

Dawson Hedges, Brigham Young University

Authors Note

Brent D. Slife, Department of Psychology, Brigham Young University, Provo, Utah

Colin Burchfield, United States Air Force

Dawson Hedges, Department of Psychiatry and the Neuroscience Center, Brigham Young University, Provo, Utah

---

Requests for reprints should be sent to Brent D. Slife, PhD, Department of Psychology, 1001 SWKT, Brigham Young University, Provo, Utah 84602. Email: brent_slife@byu.edu

Abstract

Behavior and cognition, once conceived as psychological or interpersonal in origin, are increasingly thought to arise from biology. After investigating the validity of this trend of thinking, the article attempts to interpret what it means to the discipline of psychology. Two main categories of interpretation are discussed. First, this trend could mean that biological factors ultimately underlie traditionally psychological explanations – i.e., biological factors are a *sufficient* condition for understanding behavior and cognition. Second, this trend could indicate that biological factors are important, and perhaps even traditionally overlooked, but are not sufficient in themselves to explain human behavior and cognition – i.e., biological factors are *necessary* conditions among other necessary conditions. The practical and methodological implications of each of these two interpretations are clarified, with a special focus on relevant research limitations. We conclude that the evidence does not bear out a sufficiency thesis and, instead, supports more convincingly a necessity understanding of these trends.

*Keywords:* neuroscience, trends in psychology, materialism, philosophy of science

# Interpreting the "Biologization" of Psychology

The attention to neuroscience in the last few decades has fueled many new findings about the human brain. As Fenton, James, and Insel (2004) stated, "research over the past two decades has yielded revolutionary leaps in understanding the human genome and how the brain functions" (p. 263). Such advancements have also appeared to affect the way many students of human behavior and cognition conceive of psychological phenomena, including mental illness. For example, Insel (2006) noted how findings from neuroscience research have led many people to reconceptualize psychological disorders as diseases of the brain. Some have even called this recent trend in psychology the "biologization" of psychology (Fisher, 1997; Hedges and Burchfield, 2005a; Slife, 2004; Williams, 2001). Behavior and cognition, once considered psychological or interpersonal in origin, increasingly seem to be conceptualized in terms of biology.

After reviewing the trend itself, we argue that most interpretations of the biologization occurring in psychology can be categorized into two main themes: biology as a *sufficient* condition for understanding behavior and cognition and biology as a *necessary* condition for understanding behavior and cognition. We explore the implications of each interpretation for the purpose of clarification and then examine the relevant methodological limitations associated with each interpretation. Although some scholars seem to interpret the evidence as supporting biology as a sufficient condition for understanding behavior and cognition, we argue that a careful examination of the methodological context of this evidence better supports the interpretation that biology is a necessary condition for understanding behavior and cognition. Indeed, these methodological limitations preclude, in principle, an interpretation of either biological or psychological sufficiency from even future findings.

## Biologization in Psychology

Although traditional personality theorists rarely used biology to understand human behavior and cognition (cf., Rychlak, 1981; Smith, 1997), contemporary theorists in psychology have appeared to move in the other direction. Indeed, Plotkin (1998) has epitomized the sentiments of many researchers who have essentially declared that the entire discipline of psychology, as well as its related disciplines, is but, "a branch of biology, even if [only] a special branch" (p. 1). The reduction of the psychological to the biological is echoed in contemporary behavioral neuroscience circles by such authors as Kandel, Schwartz, and Jessell (2000), who suggested that, "…all behavior is the result of brain function. What we commonly call mind is a range of functions carried out by the brain" (p. 5). In this interpretation, psychological concepts such as "behavior" and "mind" are conceived as the result of brain function.

Similarly, in the more applied areas of the discipline, Leckliter and Matarazzo (1994) have written that: "…mental disorders are manifested [biological] dysfunctions within the person and are not dysfunctions associated with conflicts between the individual and societal systems" (p. 8). Likewise, Klein and Wender (1993) argue that "depression has a biological rather than a psychological cause" (p. 212). Further, emotion (Davidson, 2000; Ochsner, and Lieberman, 2001), reading disabilities and attention-deficit hyperactivity disorder (Goldsmith, Gernsbacher, Crabbe, Dawson, Gottesman, Hewitt, McGue, Pedersen, Plomin, Rose, and Swanson, 2003), schizophrenia, conduct disorder, autistic-spectrum disorders, low intelligence, and shyness (Rowe, 2001), as well as panic disorder, alcoholism, novelty seeking, aggression, sexual orientation, monogamy, and antisocial behavior (Bonham, Warshauer-Baker, and Collins, 2005) are all examples of the many traditionally psychological topics frequently conceptualized as ultimately or essentially biological.

Even psychotherapy has been increasingly viewed in biological terms. As early as the late 1980s, Mohl (1986) wrote that, "psychotherapy is a biological treatment that acts through biological mechanisms on biological problems" (p. 152). Hayes and Chang (2002) have cast this trend toward accepting biological explanations of psychological phenomena, including the treatment of psychological disorder, as partially responsible for the desire among many psychologists to garner prescription privileges. The use of biological treatments, such as medications, is a logical extension of increasing accounts of psychological dysfunction in biological terms.

These new conceptualizations, from the traditionally psychological or interpersonal to the neuroscientifically biological, have also been reflected in research. Systematic meta-analyses of this research have shown an increase in biological investigations within psychology. Trend analyses, for example, have revealed, "that the neuroscience perspective is growing in psychology" (Robins, Gosling, and Craik, 1999, p. 123). Moreover, Tracy, Robins, and Gosling (2003) found that the neuroscience perspective had overtaken the behavioral perspective in terms of its representation in the mainstream psychological literature.

These trends do not, of course, mean that all nonbiological schools of thought have been expunged from psychology, but the trends are clearly away from the nonbiological and toward the biological. The only seeming alternatives to this increasing reliance on biological explanation in psychology are the compound-factor models, such as the biopsychosocial models (e.g., Engels, 1977). These models appear to treat the *bio* as only one of several factors that *interact* to produce behavior and cognition. However, a closer analysis of these multi-factor models indicates that many of the nonbiological factors, such as the *psycho* and *socio*, are increasingly viewed in biological terms. That is to say, the psychological and the sociological

are themselves increasingly understood to be driven or caused by the "bio" factors (Churchland, 2002; DeBerry, 1993; Fisher, 1997; Hedges and Burchfield, 2005a; Williams, 2001).

As an example, consider how Ray (2004) explains the psychological part of his biopsychosocial model: "I consider...the mind (psyche), as the functioning of the brain....Our thoughts, our feelings, our beliefs, and our hopes are nothing more than chemical and electrical activity in the nerve cells of our brains" (pp. 31-32). In this sense, psychological factors are clearly delineated and taken into account in such compound-factor models, but even these models appear to be participating in the same trend as the rest of the discipline: the bio is increasingly understood to control or cause the socio and psycho. Needless to say, this control or causation drastically curtails, if not eliminates, the potential *separate* influence of any truly nonbiological factors, making the multi-factor models more a part of, rather than apart from, the biologization of psychology.

In conclusion, there appears to be clear evidence, both from published (re)conceptualizations of theories and explanations as well as empirical analyses of the literature, that phenomena once considered "psychological" are increasingly thought to be "biological." Therefore, there is some justification for scholars to speak about the biologization of psychology, because they are referring to the growing tendency to assume that biology is increasingly important in explaining psychological function and mental disorder.

## Two Interpretations of the Trend

There appear to be two primary ways to interpret this trend: a sufficient-condition approach and a necessary-condition approach. In a sufficient-condition approach, the biologization of psychology is interpreted as indicating that biological systems alone are sufficient to fully understand behavior, cognition, and mental illness, at least ultimately. In other

words, potentially nonbiological factors such as culture, spirituality, and interpersonal relationships may be involved in some minor ways, but either they are not ultimately necessary to account for behavioral and cognitive variance or they are themselves produced ultimately by biological factors.

Alternatively, in a necessary-condition approach neuroscientific findings relevant to psychology would be interpreted to indicate that biological systems are necessary and vital for understanding behavior and cognition. However, they constitute only one of several necessary conditions. Accordingly, biology is neither sufficient in itself to account for all behavioral and cognitive variance, nor is it more basic, ultimate, or causal to the other nonbiological factors. From a necessary-condition perspective, nonbiological necessary conditions and biological necessary conditions *together* form the sufficient condition for understanding and accounting for behavioral and cognitive variance. Without one set of conditions or the other – whether or not they are actually investigated – the particular behavioral or cognitive findings could not have resulted.

*Differing Implications for Explanation*

The implications of behavioral neuroscientific findings differ depending upon whether necessary or sufficient interpretations and assumptions are used. Using a sufficient-condition approach, for example, findings of abnormal brain metabolism in a particular brain region associated with a psychological disorder could be interpreted to mean that the metabolic abnormality is a sufficient condition for the production of the disorder – that is, the metabolic abnormality *alone*, or another more ultimate biological abnormality, could explain the disorder. As such, nonbiological factors would not need to be taken into account.

Alternatively, in a necessary-condition approach, data from behavioral neuroscientific findings could be interpreted as meaning that a particular biological factor is one among other factors required to understand the particular disorder. For example, after discussing various environmental and genetic factors associated with depression, Ghaemi (2003) wrote, "…depression is not best understood as either psychological or biological [but as] caused by many variations of additive genes and specific environmental effects" (p. 218). In other words, social convention and culture might be important factors, in addition to biological factors, that contribute separate influences to depression. In a necessary-condition interpretation, biological conditions *alone* do not cause or explain a disorder because the disorder occurs only when other necessary conditions accompany the biological conditions. Moreover, the "other necessary conditions" cannot themselves be produced ultimately by biological systems; they must make a separate contribution.

It is important to note that a sufficient-condition approach can be disguised in necessary-condition terminology. Phrases such as "has a role in," or "contributes to" appear to qualify what otherwise would be sufficient-condition interpretations, as in "the amygdala plays a role in OCD" or "serotonin contributes to depression." Such phrases may indicate merely that the researchers are being cautious about what they think they know (an epistemological caution). Or, researchers may mean that they believe other factors than the ones under investigation are needed for a complete explanation of its etiology (an ontological caution). It is this latter case that could be understood as a necessary-condition interpretation of the data (and is, in one sense). However, if these *other* factors are themselves biological in nature, then this interpretation or explanation still asserts the sufficiency of biology (Slife and Hopkins, 2005; Williams, 2001). As discussed here, a necessary-condition approach requires at least the potential for the other

*types* of conditions, *non*biological conditions such as culture, to contribute to the psychological condition under investigation. If the other factors are interpreted as ultimately biological themselves, the interpretation remains exclusively and sufficiently biological.

*Differing Implications for Treatment*

Sufficiency and necessity interpretations not only involve different implications for explanation but they can also have dramatically different implications for the treatment of mental disorders. Probably most readily apparent is what a sufficient-condition approach means for the treatment of psychological disorders. Because depression, for example, must ultimately have a biological cause, or system of biological causes, the proper level of treatment is ultimately biological, whether directly biological, such as through pharmacology, or indirectly biological, such as through a therapeutic experience that is assumed to alter the individual's biology. In other words, a sufficient-condition approach virtually requires that some form of ultimately biological treatment would be used to correct the biological cause of the problem.

In contrast, a necessary-condition approach assumes that treatment is possible from various levels and through various factors. Because biology is but one "causal" factor (in the sense of a necessary condition) among many factors, a necessary-condition approach would not give special preference to either a biological or a nonbiological treatment. In fact, a necessary-condition approach to treating depression would accept the possibility that either a biological treatment (such as antidepressants) or a nonbiological treatment (such as a psychotherapy that is assumed to work nonbiologically) could have valuable outcomes, and specific combinations of the two might have the *best* outcome (cf., Cuijpers, Dekker, Hollon, and Andersson, 2009). From this perspective, a *complete* conceptualization and treatment of depression requires accounting for *all* the necessary conditions (whether or not we actually *know* these conditions).

The implications of a necessary-condition and a sufficient-condition interpretation of the biologization of psychology are probably obvious at this juncture. A sufficient-condition approach implies that the complete biologization of psychology is correct and will eventually occur, in other words a reductive or eliminative materialism (Churchland, 2002). In this sense, , psychological states are fundamentally the result of various biological conditions. On the other hand, a necessary-condition interpretation *of the same findings* would not find such a reductive materialism to be justified, especially if we mean by "complete biologization" that solely biological conceptualizations and treatments ultimately matter. From a necessary-condition perspective, such a biologized psychology would misrepresent biologically related findings and misdirect the discipline toward exclusively biological explanations and treatments.

## Methodological Limitations

Given the differing implications of these two approaches to understanding biological data and the biologization of psychology, it seems important to address important methodological limitations to provide some "in principle" indications of the *type* of support available for each approach. Perhaps most pertinent in this regard are methodological limitations that stem from the underlying logic of methods, or their philosophy of science. Philosophies not only guided the original formulation of these methods but also continue to guide their conduct, however implicitly or explicitly (Curd and Cover, 1998; Slife and Williams, 1995). We describe two relevant aspects of the logic of current research methods here: underdetermination and the presence of "controlled" factors.

### *Underdetermination*

Perhaps most pertinent to this context, one such methodological aspect is that all empirical findings are *underdetermined* (cf., Curd and Cover, 1998; Hedges and Burchfield,

2005a; Popper, 1959; Rychlak, 1980; Slife, 2004; Slife and Hopkins, 2005). That is to say, the data gathered through current scientific methods "underdetermine," or do not completely determine, the explanations or interpretations made of the data. In other words, the same data can be used to support other (though not all) interpretations, and no data or pattern of data can point to only one interpretation. As Slife and Williams (1995) have acknowledged, "…the data of an experiment can be interpreted in many different ways, no one of which can be shown necessarily to be true by empirical scientific test" (p. 187).

A host of noted philosophers of science, from Karl Popper (1959) to Willard Quine (1980) to Robert Klee (1999), have shown how underdetermination is inherent in the logic of traditional scientific investigation. They have variously argued that any empirical prediction that follows logically from a particular theory or hypothesis does not mean that the experimental affirmation of that prediction points *only* to that particular theory or hypothesis. Other hypotheses or theories may explain or entail this prediction. In other words, the logical if-then statement – if the theory is true, then the data will array in the predicted manner – works just fine in the usual logical direction, from the "if" phrase (the antecedent) to the "then" phrase (the consequent). However, logic does not necessarily work in the reverse direction – from the consequent to the antecedent. Just because the consequent is affirmed through experimental evidence does not necessarily mean that the antecedent, the "if" phrase, is true. The logical error is to presume that because the prediction is "affirmed" experimentally (the consequent), the theory must be true (the antecedent). The common statement, "the data show that X [the theory] is happening," is indicative of this error. Instead, an affirmed prediction can often be explained in other ways than the theory or explanation being considered at the time. This, then, is the

underdetermination of data: the data cannot wholly determine or dictate the theory used to explain them.

Because of this underdetermination, extra-experimental factors – factors other than data – become pivotal in interpreting research findings. For example, Baxter et al. (1992) and Schwartz, Baxter, Martin, and Phelps (1996) used positron-emission tomography (PET) to investigate the neurological effects of certain therapeutic processes for obsessive-compulsive disorder. They found that the conscious withholding of obsessive-compulsive behaviors was eventually associated with the same changes in neural activity as was the recommended drug for obsessive-compulsive disorder. If these results are true (and we entertain them only for the purpose of our example), then any data indicating similar changes in neural activity, such as a PET scan of someone off the street, could be interpreted in at least three ways: 1) biological factors, such as a drug, could be solely responsible for the outcome (i.e., a biological-sufficiency interpretation); 2) some form of conscious will could be solely responsible for the outcome (e.g., a "mind"- sufficiency interpretation); or 3) more than one set of factors (e.g., conscious withholding of obsessive-compulsive behavior, certain patterns of gene expression, treatment history) could contribute jointly to the changes in neural activity (i.e., a necessary-condition interpretation).

All these interpretations provide tenable explanations of the neural activity. The first two are sufficient-condition interpretations of the data, and the last is a necessary-condition interpretation of the same data. The second explanation might be considered an example of a *non*biological sufficient-condition explanation. This type of sufficient-condition interpretation is rare, at least in the mainstream neuroscience literature, but its relevance to the present discussion should be noted. The main point here is that the issue of underdetermination means that *other*

aspects of research than empirical data contribute to the decision to favor one interpretation of the findings over another. These "other aspects" could include many nonprocedural or non-data features of the research enterprise, such as particular theories in vogue or deeply held philosophical assumptions about reality (Slife, Reber, and Richardson, 2005). Obviously, for example, a thorough-going reductive materialist would never consider the possibility of nonbiological explanations, or even nonbiological contributing factors, but the issue of underdetermination implies that this lack of consideration could be a philosophical rather than a data issue.

Our point in raising the underdetermination issue is that it implies that behavioral neuroscience data, in particular, cannot *determine* either the necessary or the sufficiency interpretation sometimes made of them. Although certain interpretations of these data are probably not plausible (e.g., biological factors are *not* important), *how* such factors are important is likely to be underdetermined. Either a necessary or a sufficiency interpretation is often plausible, as our OCD hypothetical example above illustrates.

*Presence of "Controlled" Factors*

If the first methodological limitation clarifies the potential for either interpretation of the data, we believe the second methodological limitation tips the balance between necessary- and sufficient-condition perspectives *toward* the necessary-condition perspective. Indeed, it may be the better supported of the two interpretations *in principle*. This second limitation is the continual presence of more conditions than the specific factor or factors under study (Slife and Hopkins, 2005). This limitation is trivially true in the case of correlational designs. The factors under study are never measured in a way that excludes the possible influence of other factors, so correlational studies can never point to the causal sufficiency of any one factor or set of factors.

Still, it is not always understood that determining causal sufficiency is also problematic for randomized, double-blind studies – the "gold standard" of experimental design in neuroscience and medicine. Many researchers assume that experimental evidence from a positive study proves that the independent, or predictor, variables are the sufficient causes of the effects on dependent variables, because any potential confounding factors, even if unknown, are randomized, and thus found equally among study arms (e.g., control groups). As such, the predictor variables are presumably "independent" of any confounding variables and thus are self-sufficient and causal to whatever changes occur in the dependent variables.

The difficulty with this presumption is that even in the most highly controlled of experimental studies – a truly experimental design – there are factors involved in the experimental group, other than the independent variables, that play a role in the outcome of the investigation. These factors may be controlled, measured, and equated across experimental and control groups, but they are never *eliminated* from the conditions involved in the investigation. For example, we may control, measure, or equate for the effect of glucose across experimental groups in a study of antidepressant effects, but this control does not mean that glucose plays no role in the effects, changes, or variations found. In fact, without sufficient quantities of glucose, no antidepressant effects would be found because the patients would have died. In this sense, the influence of seemingly trivial factors (e.g., gravity, atmospheric pressure, culture), even *if* equated across groups and conditions, could still be present and *necessary* for the outcome. Many of these factors could even be nonbiological, such as culture and spirituality.

The point is that if these other factors, whatever they are and however trivial they may be, are necessary for the result found, then the factors under specific study – the independent variables – can only be necessary conditions themselves. The "only" here does not mean they

are unimportant. Indeed, they are crucial and "necessary," by definition, and could have been, up to the point of their discovery, an *unknown* necessary condition. The continual presence of unspecified variables in the control conditions is, in fact, the philosophy-of-science reason that results from neuroscientific studies are often qualified with words such as "contributes to," or "plays a role in." Investigators have been taught to limit their interpretations of data, because the most their data can yield, in principle, are necessary rather than sufficient conditions.

It is apparent, then, given this brief report of relevant considerations based on the philosophy of science, that the evidence can only tell us, at best, what necessary conditions are involved in an investigation, whether they are biological or nonbiological. Importantly, these limitations of evidence do not necessarily mean that biological systems are not sufficient for understanding behavioral and cognitive variance, in *reality*, but these limitations *do* mean that there is – and can be, given the current logic of methods – no *evidence* that can *prove* the sufficiency of any particular category of conditions, biological or nonbiological. Given the logic of experimental methods and the ultimately underdetermined nature of the evidence, all sufficient-condition interpretations of data are over-inferences, because investigators only have, at best, evidence of necessary conditions, *not* sufficient conditions. Consequently, a necessary-condition interpretation of the biological trends of psychology better fits what the scientific data can tell us.

## Implications and Conclusion

Three main implications seem to follow from our argument, one concerning evidence, another regarding future research, and still another relating to the biologization of psychology. The first implication is a fairly straightforward one for scientists: do not say or promise more than you can know. Several researchers have, of course, argued for a sufficiency or reductive

materialist understanding of the neuroscience data (e.g., Churchland, 2002), but even many of those who have not made such arguments appear to assume that a sufficiency understanding is in the offing. Consider this excerpt from the most recent statement of the American Psychiatry Association on the diagnosis and treatment of mental disorders (APA, 2003):

> Compelling evidence exists that disorders including schizophrenia, bipolar disorder, and autism to name a few have a strong genetic component. Still, brain science has not advanced to the point where scientists or clinicians can point to readily discernible pathologic lesions or genetic abnormalities that in and of themselves serve as reliable or predictive biomarkers of a given mental disorder or mental disorders as a group.

First, does the term "strong genetic component" in this quote mean a necessary or a sufficient biological condition? This meaning would require important clarifications, given our analysis, because no evidence for sufficient biological conditions exists or can exist, in principle. Moreover, the kind of evidence that *is* available would never rule out the significance of nonbiological factors in whatever findings are generated. Second, even if the notion of "strong" is taken to mean necessary, rather than sufficient, the next sentence of this quote appears to imply that neuroscience will "advance to the point" where sufficient biological conditions—"in and of themselves" conditions—will likely or inevitably be found. However, our analysis of the methods available for these "advances" casts extreme doubt on this vision of future findings. Indeed, unless the logic of current methods is completely revolutionized, and this includes the current "gold standards" of investigation, there is and can be no such vision. No "in and of themselves" conditions, whether biological or psychological, can or ever will be produced.

The second implication concerning future research follows directly from the first. If other necessary conditions cannot be ruled out, can they be ruled in? Perhaps most germane to

the current biologization of psychology, are there potentially nonbiological factors that truly matter in neuroscience data? Research on this issue seems to point in at least three general directions: the placebo effect (Hedges and Burchfield, 2005b; Moerman and Jonas, 2002), conscious action (Antelman, Eichler, Black, and Kocan, 1980; Stein, Ives-Deliperi, and Thomas, 2008), and the social environment (Kendler, 2005; Schwartz, Stapp, and Beuaregard (2005). However, it is important to note that each of these potentially nonbiological necessary conditions occurs in and through the biological. This, after all, is the nature of necessary conditions; they have to occur with other necessary conditions for them to have an effect. Indeed, in this same sense, it could be said that biological factors occur in and through the cultural. An important question for future research purposes then becomes whether potential nonbiological factors *originate* from nonbiological sources and thus contribute a separate form of influence, however combined they may ultimately become.

The final implication of this analysis regards the opening question about what it means for psychology to increasingly conduct more biological research and use more biological explanations and treatments. If biologization is interpreted to mean that psychologists and other scientists are discovering that biological factors are ultimately sufficient for or determinate of traditionally psychological factors, such as relationships, culture, and conscious action, then our analysis indicates that this meaning is not supported by empirical research on these factors. No research, at least that using current methodological designs, can rule *out* the necessary influence of other, often unstudied factors. The best that current methods can do is rule *in* the necessary influence of previously unstudied factors. In this sense, the biologization of psychology is best understood as the discovery, or perhaps *re*discovery, of the vital importance of biological factors in psychological phenomena.

References

Antelman, S. M., Eichler, A. J., Black, C. A., and Kocan, D. (1980). Interchangeability of stress and amphetamine in sensitization. *Science, 207*, 329-331.

Baxter, L. R., Schwartz, J. M., Bergman, K. S., Szuba, M. P., Guze, B. H., Mazziotta, J. C., Alazraki, A., Selin, C. E., Ferng, H. K., Munford, P., and Phelps, M. E. (1992). Caudateglucose metabolic rate changes with both drug and behavior therapy for obsessive-compulsive disorder. *Archives of General Psychiatry, 49*, 681-689.

Bonham, V. L., Warshauer-Baker, E., and Collins, F. S. (2005). Race and Ethnicity in the Genome Era: The Complexity of the Constructs. *American Psychologist, 60*, 9-15.

Churchland, P. S. (2002). *Brain-wise: Studies in Neurophilosophy*. Cambridge, Massachusetts: The MIT Press.

Curd, M., and Cover, J. A. (1998). *Philosophy of science: The central issues.* New York: W. W. Norton.

Cuijpers, P., Dekker, J., Hollon, S. D., and Andersson, G. (2009). Adding psychotherapy to pharmacotherapy in the treatment of depressive disorders in adults: A meta analysis. *Journal of Clinical Psychiatry, 70*, 1219-1229.

Davidson, R.J. (2000). Affective style, psychopathology, and resilience: Brain mechanisms and plasticity. *American Psychologist*, *55*, 1196-1214.

DeBerry, S. T. (1993). *Quantum psychology: Steps to a postmodern ecology of being.* Westport, Connecticut: Praeger.

Engel, G. L. (1977). The need for a new medical model: A challenge for biomedicine. *Science, 196*, 129–136.

Fisher, A. M. (1997). Modern manifestations of materialism: A legacy of the enlightenment

discourse. *Journal of Theoretical and Philosophical Psychology*, *17*, 45-55.

Fenton, W., James, R., and Insel, T. (2004). Psychiatry residency training, the physician-scientist, and the future of psychiatry. *Academic Psychiatry, 28,* 263-266.

Ghaemi, S., N. (2003). *The concepts of psychiatry: A pluralistic approach to the mind and mental illness.* Baltimore: The Johns Hopkins University Press.

Goldsmith, H. H., Gernsbacher, M. A., Crabbe, J., Dawson, G., Gottesman, I. I., Hewitt, J., McGue, M., Pederson, N., Plomin, R., Rose, R., and Swanson, J. (2003). Research psychologists' roles in the genetic revolution. *American Psychologist, 58,* 318-319.

Hayes, S. C, and Chang, G. (2002). Invasion of the body snatchers: Prescription privileges, professional schools, and the drive to create a new behavioral health profession. *Clinical Psychology: Science and Practice, 9*, 264-269.

Hedges, D., and Burchfield, C. M. (2005a). The assumptions and implications of the neurobiological approach to depression. In B. D. Slife, J. Reber, and F. Richardson (Eds.),

*Critical thinking about psychology: Hidden assumptions and plausible alternatives*. (p. 99 – 120). Washington, D. C.: American Psychological Association Press.

Hedges, D., and Burchfield, C. M. (2005b). The placebo and its implications. *The Journal of Mind and Behavior, 26*, 161-180.

Insell, T. (2006). Mental health research: Into the future. Available: http://www.mhamd.org/help/documents/ThomasInselEditorial.pdf.

Kandel, E. R., Schwartz, J. H., and Jessell, T. M. (2000). *Principles of neural science, fourth edition, New York: McGraw Hill.*

New York: McGraw-Hill.

Kendler, K. (2005). Psychiatric genetics: A methodological critique. *American Journal of Psychiatry, 162*, 3-11.

Klee, R. (1999). *Scientific inquiry: Readings in the philosophy of science*. New York: Oxford University Press.

Klein, D. F., and Wender, P. H. (1993). *Understanding depression: A complete guide to its diagnosis and treatment*. New York: Oxford University Press.

Leckliter, I. N., andMatarazzo, J. D. (1994). Diagnosis and classification. In V.B. Hasselt and M. Hersen (Eds.), *Advanced abnormal psychology* (p. 3-18). New York: Plenum Press.

Moerman, D. E., and Jonas, W. B. (2002). Deconstructing the placebo effect and finding the meaning response. *Annals of Internal Medicine, 136*, 471-476.

Mohl, P. (1986) [Statement made at the 1986 American Psychiatric Association, as quoted in data accumulating to support concept that psychotherapy is biologic treatment.] *Clinical Psychiatric News*, 1986, 14, 28.

Ochsner, K. N., and Lieberman, M. D. (2001). The emergence of social cognitive neuroscience. *American Psychologist, 56*, 717-734.

Plotkin, H. (1998). *Evolution in mind: An introduction to evolutionary psychology.* Cambridge, Massachusetts: Harvard University Press.

Popper, K. (1959). *The logic of scientific discovery*. New York: Basic Books.

Quine, W. (1980). *From a logical point of view*. Cambridge: Harvard University Press.

Ray, O. (2004). How the mind hurts and heals the body. *American Psychologist, 59,* 29-40.

Robins, R. W., Gosling, S. D., and Craik, K. H. (1999). An empirical analysis of trends in psychology. *American Psychologist, 54,* 117-128.

Rowe, D. C. (2001). The nurture assumption persists. *American Psychologist, 56,* 168-174.

Rychlak, J. F. (1980). The false promise of falsification. *The Journal of Mind and Behavior, 1*, 183-194.

Rychlak, J. F. (1981). *Introduction to personality and psychotherapy: A theory-construction Approach, second edition*Dallas: Houghton Mifflin Company.

Schwartz, J. M., Stapp, H. P., and Beauregard, M. (2005). Quantum theory in neuroscience and psychology: A neurophysical model of mind-brain interaction. *Philosophical Transactions of the Royal Society of London, Series B, 360,* 1309-27.

Schwartz, J.M., Stoessel, P. W., Baxter, L. R., Martin, K.M., and Phelps, M. E. (1996). Systematic changes in cerebral glucose metabolic rate after successful behavior modification treatment of obsessive-compulsive disorder. *Archives of General Psychiatry, 53*, 109- 113.

Slife, B. D. (2004). Theoretical challenges to therapy practice and research: The constraint of naturalism. In M. J. Lambert (Ed.), *The handbook of psychotherapy and behavior change* (pp. 44 – 83). New York: John Wiley and Sons.

Slife, B. D., and Hopkins, R. H. (2005). Alternative assumptions for neuroscience: Formulating a true monism. In B. D. Slife, J. Reber, and F. Richardson (Eds.), *Critical thinking about psychology: Hidden assumptions and plausible alternatives*. (pp. 121 – 147). Washington, D. C.: American Psychological Association Press.

Slife, B. D., Reber, J., and Richardson, F. (2005). *Critical thinking about psychology: Hidden assumptions and plausible alternatives*. Washington, D. C.: American Psychological Association Press.

Slife, B. D., and Williams, R. N. (1995). *What's behind the research? Discovering hidden*

*assumptions in the behavioral sciences*. Thousand Oaks, California: Sage Publications.

Smith, R. (1997). *The Norton history of the human sciences*. New York: W.W. Norton and Company.

Stein, D. J., Ives-Deliperi, V., and Thomas, K. G .F. (2008). Psychobiology of mindfulness. *CNS Spectrums*, *13*, 752-756.

Tracy, J. L., Robins, R. W., and Gosling, S. (2003). Tracking trends in psychological science: An empirical analysis of the history of psychology (pp. 105-130). In T. C. Dalton and R. B. Evans (Eds.), *The lifecycle of psychological ideas: Understanding prominence and the dynamics of intellectual change.* New York: Kluwer Publishers.

Williams, R. N. (2001). The biologization of psychotherapy: Understanding the nature of influence. In B. Slife, R. Williams, and S. Barlow (Eds.), *Critical issues in psychotherapy: Translating new ideas into practice* (pp. 51-67). Thousand Oaks, California: Sage Publications.