Hook, line, and sinker:

Psychology's uncritical acceptance of biological explanation

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Can there be any doubt about psychology's headlong plunge into the tidal wave of biological materialism? Lest you think my metaphor of a "tidal wave" is an exaggeration, consider that Robert Plomin (1989) uses the same metaphor in a recent American Psychologist review. In this article, he describes how some types of biological explanation are enjoying a "wave of acceptance" in psychology that is now "growing into a tidal wave" (p. 105). You might also quibble with my notion that psychology is taking a "headlong plunge" into this tidal wave, or that psychology has accepted biological explanations "hook, line, and sinker," as the title of my presentation indicates.

As I will attempt to show, however, these metaphors are completely apt. Both the experimental and the applied aspects of our discipline are moving headlong toward exclusively materialist explanations, with almost no critical examination. Oh, there are the occasional debates, such as the debates about prescription privileges. Still, these debates rarely, if ever, broach the fundamental issues, such as "should we explain people in solely biological terms?" Our paper asks why there is so little critical examination. We answer this question with the three main arguments in favor of biologizing psychology – the empirical, the economic, and the philosophical. However, we do not stop where psychology has typically stopped – with the arguments in favor. We also critically examine these arguments, with a conclusion that I think you will find most intriguing.

The Tidal Wave of Acceptance

Where has the discipline felt these so-called tidal wave influences? Perhaps a better question is where has it not felt these biological influences. As Williams (2001) and Fisher (1999) have noted in separate articles, psychology is rapidly becoming "biologized," meaning that materialistic explanations are replacing psychological explanations. They point particularly to the more applied aspects of our discipline. Psychotherapy, for instance, has so increased its biological emphasis that there is rarely a formal or informal diagnosis that is not considered fundamentally biological, including marital dysfunction (e.g., Wright, 1994).

Even in my relatively short professional career, many psychologists seem to have changed the diagnosis of depression from a mostly psychological disorder, with psychological causes and treatments, to a biological disorder. Klein & Wender (1993) represent these psychologists when they conclude in their book on depression: "depression has a biological rather than a psychological cause" (p. 212). Also, when the causes are ultimately biological, the medical treatments are soon to follow. Some patient advocate groups, for example, claim that drugs help 90 percent of depressed patients (Valenstein, 1998, p. 215). Indeed, as Winokur (1981) put it, "only a troglodyte would not recognize that pharmacotherapy is the preferred treatment of depression" (p. 115).

Winokur's attitude is indicative of the attitude of many psychologists. It is little wonder that they are now championing prescription privileges. As their logic goes, our disciplinary province has always been that of "mental disorders." Consequently, when we learn that these disorders are biologically based, we should be able to draw upon whatever is effective to treat these disorders, including traditionally medical treatments. The only problem is that most of us are a long way from being board certified psychosurgeons, using the “knife” to alter the various neurological mechanisms that may be involved. However, many psychologists seem to think that being a psychopharmacologist is still within their grasp. In either case, the assumptions are that disorders themselves are biogenic and biogenic treatments are effective.

This assumption is not limited to the applied aspects of psychology. The neuroscience contingent of psychology departments has grown significantly in recent years. Indeed, many of the more experimental sub-disciplines of psychology, such as cognitive psychology, have increasingly bolstered their biological explanations and medical research methods. A recent candidate for a cognitive position in our department told me that many cognitive psychology positions <u>require</u> candidates to have neurological explanations for their cognitive phenomena, along with an armamentarium of neuroscientific investigatory methods. Kandel (1991) characterizes what many cognitivists now believe, “What we commonly call mind is a range of functions carried out by the brain” (p. xx).

The increasing dominance of biological explanation has led many leading thinkers in psychology to make some amazing claims. Consider the claim by Leckliter and Matarazzo (1994) in an edited book on <u>Advanced Abnormal Psychology</u>: “. . .mental disorders are manifested dysfunctions within the person and are not dysfunctions associated with conflicts between the individual and societal systems” (p. 8). Consider also that psychotherapy itself has been biologized. As Mohl (1986) put it in the <u>Clinical</u>

Psychiatric News, "psychotherapy is a biological treatment that acts through biological mechanisms on biological problems" (p. 152). Nancy Andreasen (2001) concurs with this claim in her newest book. However, she contends that psychotherapy is far too inefficient to be really useful. Why not go straight to the biology itself, with traditional medical interventions? Why mess around with traditional psychology at all?

This type of reasoning has led many extra-disciplinary observers, such as Paul and Patricia Churchland (1998), to consider psychology a quaint, but anachronistic, "folk" psychology, much like the folk physics of the Middle Ages. Psychology, in this sense, is a "prescientific, commonsense framework" that allows for some prediction and understanding, to be sure, but is soon to be completely replaced by eliminative materialism – the materialism that eliminates all quaint, anachronistic, "folk" conceptions, such as belief, desire, love, hate, joy, memory, sympathy, and intention (Churchland & Churchland, 1998, p. 3). As Butz (1994) phrased it so colorfully, biological explanations will effectively end psychology's "Jurassic Age." Traditional psychologists are dinosaurs who will become extinct unless they move into the new era of biology.

The Lack of Critical Examination

Is this really the future of psychology? Have psychologists, in George Albee's (2000) terms, "sold their souls to the devil" – the disease model of mental disorder – and is the devil about to collect (p. 248)? The most surprising thing about these questions is that they appear to have no answers, not because they cannot be answered, but because no one in psychology seems to be considering them. In other words, we see almost no systematic evaluation of these biological trends anywhere in the discipline (see Slife, in press and Williams, 2001 for exceptions). If Butz (1994), Andreasen (2001), Mohl (1986), and the Churchlands (1998) are even partly correct, our entire psychological enterprise is in the most imminent of dangers. Yet, psychologists appear to be proceeding blithely on, as if nothing is happening.

Why? Why have we not critically examined such obvious trends? Surely, we would not claim that psychologists are conflict avoidant; our discipline has been rife with conflict since its inception (Yanchar & Slife, 1997). Why, then, are psychologists avoiding a critical analysis of the biologization of their own discipline? As I mentioned, we believe there are three main factors involved – perceptions of the research, economic forces, and unacknowledged philosophies. As we will attempt to show, each leg of this three-legged support for biologization is not only completely challengeable but also sufficiently doubtable to question psychology's trends toward biologization. We would never doubt the importance of biological factors. Rather, we wish to weed out the problematic interpretations of the existing data. I will later present a way of explaining these data that give appropriate credit to our biology without jeopardizing psychological phenomena.

What is the first leg of the system supporting biologization? It is, of course, the well-known claim that the relevant research literature demonstrates the need for this biologization. Although there might have been the need, at one time, to separate the mind from the brain, recent technological advances are supposed to have shown us the error of our ways. The mind <u>is</u> the brain, and mental disorder is thus "brain disease" – case closed. This disease could be genetic, biochemical, neurological, or some combination of all three. It does not matter. What matters is that many psychologists are now convinced that the empirical research is unequivocal in its support for materialistic explanations.

The second leg of the argument in favor of biological explanations is another form of materialism – economic materialism (Slife, in press). Surely, there is no doubt, at this juncture, that the economic incentives are almost entirely in the direction of biological explanations (Polkinghorne, 2001). It is certainly no secret that third-party payers are questioning traditional forms of psychotherapy, with traditional psychological explanations. Psychotherapy is considered too long and too inefficient, whereas medication supposedly gets to the heart of the matter in just a few weeks. Moreover, there are fewer research funds for traditional psychological approaches. Granting agencies have also accepted biological materialism, as my title says, “hook, line, and sinker,” so their considerable power is being felt in biologizing psychology.

The final leg of this argument for biologization is, we believe, the most important and the most overlooked – the prevailing philosophy of psychiatry and psychology. Indeed, we would argue that neither of the other legs would exist without this third leg hovering in the background – the philosophy of naturalism. Without this philosophy of psychology, there would be no reason to conduct the biological research and no reason to fund it. In fact, the reason the natural sciences are called “natural sciences” is that they have implicitly, if not explicitly, endorsed this philosophy. The adjective “natural” is, in part, due to the subject of their inquiry – nature. However, naturalism is responsible for their interest in nature, not to mention their way of investigating nature, and any wish we might have to be more scientific brings us closer to naturalism. What is this philosophy?

In brief, the philosophy of naturalism is the familiar notion that physical laws and principles govern the world, including the human world (Slife, in press). We may not yet know all these physical laws, but we assume philosophically that they must and do occur, so we try to discover them in our science. To say, for example, that humans "choose" to become mentally disordered or that they can become cured through the force of their "free will" is patently ridiculous from this perspective. Human nature is not exempt from nature and its natural laws; humans are biological organisms and thus are determined by those laws. The Churchlands, in this sense, are right: biological materialism <u>is</u> a more fundamental explanation, in principle. Psychological explanations were merely stopgap explanations until our technology developed sufficiently. There was never an alternative to the biologization of the discipline, because there was never a separate discipline of psychology.

So there you have it – the formidability of biological explanation in three easy steps – one empirical, another economic, and a third philosophical. These three arguments coalesce to form such a convincing triad that psychologists assume it needs no critical examination. Eventual biologization is a foregone conclusion, because biological explanation is a truism; it is the way all things are. As Winokur (1981) made completely clear, only a "troglodyte" would resist the obvious validity of biological explanation.

Well, the authors of this presentation must be troglodytes, including my psychiatrist colleague, Dawson Hedges. We are not sure what this troglodytism means for our personalities and our constitutions, but we <u>are</u> sure that Winokur would consider its cause to be biological. Our own critical evaluation of these three arguments is that each is surprisingly deficient. I will attempt to outline these deficiencies here, with special emphasis on the one considered the most influential – the empirical. Given my time constraints, I will focus on the diagnosis and treatment of depression as my exemplar of current biological explanations in the field. Depression is not only the “bread and butter” of modern clinical psychology and psychiatry; it is also a relatively recent addition to the growing pantheon of biological and medical disorder. Then, I will present another way to understand biological explanation that does not make it incompatible, and thus competitive, with psychological explanation.

Empirical Problems

The use of antidepressants for the treatment of depression has escalated incredibly since the introduction of SSRI drugs in the 1990s (Moncrieff, 2001). The statistics of many countries show that antidepressants are now, as Moncrieff (2001) reports about the United Kingdom, “the most commonly prescribed class of psychotropic drug and by far the most costly” (p. 288). Presumably, this escalation and this cost are worth it. Presumably, this escalation and cost are supported by a large and rigorous research program on the effectiveness of antidepressants for depression. Unfortunately, this is not the case. As the few critical reviews of this literature show (Moncrieff, 2001; Valenstein, 1998; Williams, 2001), there are major, unsolved methodological and conceptual problems. Obviously, my time limitations prohibit a thorough review here, but allow me to highlight some of the main problems, because you rarely hear about these in mainstream psychology.

The “gold standard” in this line of research is undoubtedly the double-blind, randomized control trial (Moncrieff, 2001). However, it turns out that precious few studies reach this standard, even though they are often advertised as reaching it. As we will see, these studies are deficient in a number of substantive ways, including the problems of unblinding, placebo wash, withdrawals, discontinuation, and nonspecificity. Moreover, these problems are not merely technical considerations. When they are controlled for, the empirical results present a dramatically different picture of the efficacy of antidepressant medications.

One of the most difficult problems is called the "unblinding" of participants. That is, the physiological experience of ingesting an active drug, including its side effects, is often qualitatively different from taking a placebo that is an inert substance. This differential experience defeats double-blinding, because it reveals to participants (and often experimenters) who received which medication. As Moncrieff (1998; 2001) has shown in several investigations, this unblinding invariably favors the active drug – in this case, the antidepressant – because participants expect important things from drugs that have side effects and "do things" to them. In fact, meta-analytic studies indicate that the effect size of these differences is highly correlated with the frequency of reported side effects (Greenberg et al., 1994).

This unblinding problem would be a minor issue if studies with placebos were more favorable – our second method problem. The gist of this evidence is that the more the placebo matches the side effects, the more likely it is that differences between antidepressants and placebos "wash out," as Antonuccio et al. (1999) terms it. In the few studies using "active" placebos, such as atropine, comparisons with antidepressants typically show nonsignificant differences (Moncrieff et al., 1998; Moncrieff, 2001). Kirsch and Saperstein (1998) present an eye-opening statistical meta-analysis based on 19 studies and 2500 patients. They showed that placebos consistently produce 75% of the effects obtained with antidepressants. Indeed, they suggest that the percentage would be even higher if the placebos were presented as convincing, biological remedies.

Withdrawal problems have also plagued these studies. Many studies exclude people who withdraw early from treatment, making treatment and control groups different on important characteristics and defeating randomization. Shultz et al. (1996) and Coldtitz et al (1989), for example, have shown that withdrawals result in higher effect sizes for antidepressants. Analyses restricted to treatment completers produced response rates up to 23% higher for antidepressants (Bollini et al., 1999). Moreover, O'Sullivan et al. (1991) have shown that compliance rates contribute to better outcome. Yet, it is common for investigators to exclude significantly more noncompliant participants from the antidepressant group, due to greater side effects. An influential study by Hollyman et al. (1988), for instance, excluded 21% of the original sample from analysis – mostly from the antidepressant group – giving it the greater potential for compliance and a favorable outcome.

As a fourth major methodological problem, long-term studies of antidepressants have relied on "discontinuation designs" (Moncrieff, 2001). Participants are randomly selected to either continue with antidepressants or be withdrawn to a placebo. The problem is that discontinuing the antidepressant results in a set of symptoms that is often mistaken for relapse, thus depressing the outcome of the placebo group. Suppes et al. (1991) has demonstrated withdrawal-induced relapses in persons who stop lithium treatment, and Baldessarini (1995) has suggested that the general phenomenon has a biological basis related to long-term medication.

We should consider one last methodological problem before we review the general findings on antidepressants – the nonspecificity of antidepressants. Researchers have known for a long time that many non-antidepressant medications treat depression effectively, including reserpine, many neuroleptics, barbiturates, benzodiazepines, and buspirone (see Moncrieff, 2001 for review). This incredible variety of medications suggests to Moncrieff (2001), "that depression is susceptible to a variety of non-disease-specific pharmacological actions, such as sedation or psychostimulation, as well as the effects of suggestion" (p. 292). Thase and Kupfer (1996) concur with Moncrieff (2001), estimating that 80% to 90% of antidepressant effectiveness for mild to moderate depression can actually be attributed to nonspecific factors such as clinical support. Nor does other evidence support the notion of a specific antidepressant pharmacological mechanism. As Antonuccio et al. (1999) has shown, the biochemical basis of depression is as elusive as ever, even after "decades of intensive research" (Moncrieff, 2001, p. 293).

Empirical Findings

What do the unsolved problems of unblinding, placebo wash, withdrawal, discontinuation, and nonspecificity mean for understanding the literature on antidepressant effectiveness? First, it seems obvious to us that this literature is not unlike most social science research literatures – it is shot-through with important problems (problems which, not so coincidentally, work in the favor of antidepressant efficacy). Second, it also appears obvious to us that many of our colleagues, particularly those in the more applied realms, would be surprised at just how "shot-through" this research literature is with unsolved problems.

That is, we believe that this research is given a privileged status – because it involves biological variables – that leads to it being evaluated with lower standards. We will take up the reason for this privileged status when we discuss the philosophy of this research. At this juncture, it is important to note that we should not be surprised that there are substantive problems with this research. The subjectivity that pervades most other social science research programs also pervades this one. Indeed, the core of the phenomenon of depression is the subjective feeling of sadness.

What can we conclude about the effectiveness of antidepressants, if we consider these methodological problems? Here, we would invite you to read recent reviews of this literature by Moncrieff (2001) and Valenstein (1998), both noted scientists in the field, Moncrieff in psychopharmacology and Valenstein in neuroscience. These are not the only reviews, to be sure, but they are two of the few reviews that explicitly attempt to evaluate the literature in the light of such problems. What do they say? Moncrieff (2001) begins by noting that the precipitous increase in antidepressant use has not met with any decrease in societal depression. Recent epidemiological studies suggest that the prevalence of depression has remained unchanged since the 1950's. Khan et al. (2000) also have evidence from clinical trials that people on antidepressants are not any less likely to commit or attempt suicide than those on placebo.

Moncrieff (2001) also describes how two of the largest, most influential and independently funded trials found little difference between antidepressants and placebo. In a Medical Research Council trial conducted in the United Kingdom (Medical Research Council, 1965), differences between antidepressants and placebo were not statistically different in the principle categories of outcome, and found "negligible" differences for individual symptoms. Similarly, the second NIMH Collaborative Depression Study (Raskin et al., 1970), conducted in the United States, found “small and inconsistent effects” (p. 292). Moncrieff (2001) also points to the nonspecificity problem. A number of studies indicate that depression responds to a variety of pharmacological actions and psychotherapies, implying “that recovery is not achieved through a particular biochemical manipulation” (p. 293).

Neuroscientist Elliot Valenstein (1998) paints a similar picture of the results of these and other studies. However, he also challenges the biochemical theories of depression, contending that they all “involve a selective perception of the evidence, ignoring some findings and exaggerating and even distorting others” (p. 99). For example, traditional biogenic theories of depression ignore the fact that it takes a relatively long time for antidepressant drugs to produce any elevation in a depressed person’s mood. These theories ignore this fact, as Valenstein (1998) explains, because the drugs elevate serotonin and norepinephrine activity in only a day or two. If serotonin and norepinephrine elevate mood, as many believe (Musselman et al., 1998), then why is mood not elevated when these chemicals are elevated. Why does it take three to four weeks?

Some have attempted to explain this delay with the “cascading” effects of norepinephrine and serotonin (Musselman et al., 1998). However, according to Valenstein (1998), “the number of different brain changes that can occur over a three-week period of drug treatment is huge, as every change produces a cascade of other changes until the complexities are unfathomable” (p. 99). Moreover, none of these “theories” has even attempted to answer the primary question: How is the “huge gap” (p.

96) between neurochemistry, such as serotonin level, and the subjective experience of depression explained? In light of these problems, Valenstein concludes with the confident assertion that the evidence is "clear" – "none of the proposed biogenic amine theories of depression can possibly be correct" (p. 102). In other words, psychology has accepted such explanations <u>without</u> valid evidence. In fact, psychology has accepted such explanations despite considerable evidence to the <u>contrary</u>.

<u>Economic Forces</u>

The conclusions of Moncrieff and Valenstein may be surprising. Indeed, you may be tempted to assume that Troglodytes, such as we, would only attend to the reviews of other Troglodytes, but this is simply not true. We selected Moncrieff (2001) and Valenstein (1998) for their <u>lack</u> of biases rather than their anti-biological biases. We could impress you with their biological and scientific credentials. (Valenstein's many neuroscience texts are celebrated across the field.) However, the evidence is that the biases of this antidepressant research are, if anything, in the <u>opposite</u> direction. That is, there is considerable empirical evidence that the pharmaceutical industry has done its level and powerful best to influence this program of research, which is my segue to examining the second leg of the arguments for biologizing psychology – economic forces.

Recall in my introduction that one of the main forces behind the popularity of biological explanation in psychology is the economic force of third-party payers and grant funding agencies. This influence is, in part, because these payers and agencies are often closely affiliated with and controlled by medicine. However, it probably goes without saying that such economic factors are not in themselves cogent scientific reasons for biologizing psychology. No one would surely say, at least seriously, that we should prescribe drugs and favor biological explanations just because people will pay for it. However, we all know the insidious workings of money. What HMO's and granting agencies will pay for is a potent, behind-the-scenes motivator for diagnosis, treatment, and research. Money works its nefarious way, regardless of whether its way is justified. In fact, money typically "finds" a justification, and this is precisely what seems to have happened with antidepressants and the pharmaceutical industry.

Healy (1999), for instance, suggests that our current conception of the effectiveness of antidepressants is molded more by the marketing imperatives of the pharmaceutical industry than by the scientific findings. There is certainly no dispute that the pharmaceutical industry is the largest funder of medical research in North America, and this, as Valenstein (1998) notes, is "overwhelmingly true" for research on psychiatric drugs (p. 187). Indeed, Valenstein (1998) claims that these companies are unlikely to fund researchers who have been negative about drug effectiveness. However, it is one thing to point to this industry's massive funding efforts and profit motives, and quite another to claim this industry truly attempts to bias investigators, just to enhance its "bottom line." Is there objective evidence for this latter claim? You bet there is.

In fact, editorials in five different prestigious medical journals have all pointed to evidence that pharmaceutical funding has tainted the objectivity of these studies (Greenberg, 2001). Freemantle (2000), for example, has recently shown in a meta-analysis of comparative studies is that a sponsor's funding is the best predictor of whether studies will show the sponsor's drug to be effective. Similarly, Friedberg et al. (1999) have shown empirically that company-supported studies are more likely to report efficacy for the company's product than are independent studies of the same product. Stern and Simes (1997) also found considerable evidence that studies that do not reflect positively on antidepressants are less likely to be published. Moncrieff (2001) reports that the problem of publication bias is even more pronounced with recent SSRI antidepressants, because the majority of trials have been conducted by the pharmaceutical industry, which has no obligation to publish negative results and may see little advantage in doing so.

What do such powerful economic forces mean? One could assume that these economic forces are telling us what is supremely valid and true, through Adam Smith's "invisible [economic] hand," or some such. However, none of the editorialists or reviewers who have observed these forces considers them positive. Indeed, they view these economic forces as a primary reason so many of us are so ignorant about the large number of methodological problems and nonsignificant results. They also believe these forces have badly skewed the information on drugs, and thus seriously misled third-party payers and entire disciplines like psychology. Perhaps most importantly, we do not have to rely on anyone's "belief" about this issue. Fortunately, there is considerable empirical evidence, and all of it supports the conclusion that these economic forces have badly tainted the experimental findings.

Unacknowledged Philosophies

At this point in our journey through the arguments for biologization, we have seen major deficiencies – from major methodological flaws in the empirical research to major problematic influences in the economic forces. Why have psychologists not incorporated these deficiencies into their own sense of biological treatment and explanation, and at least been more cautious about their headlong plunge into this "tidal wave?" To reiterate, we see no evidence of caution anywhere, even among normally circumspect psychological investigators. If anything, biological explanation and treatment are viewed as inherently rigorous and inherently true. Why? It cannot be the actual rigor and truth of the claims themselves. There is too much evidence that neither is valid, at least at this point in the research. What is going on?

Our answer to this question is the unacknowledged philosophy of our culture. As I described earlier, the philosophy of our professional culture, as inherited from the natural sciences, is the philosophy of naturalism. One of psychology's historic parents is, of course, the natural sciences. The very first psychology course offered was "Psychology as a Natural Science," Professor Wundt's course at Leipzig. Psychology has occasionally flirted with non-naturalism and super-naturalism through its various personality theories, but there is little doubt that the background unity of the discipline has stemmed from its natural science methods and its natural science understanding of what really mattered – the matter itself.

As I have demonstrated in other publications (e.g., Slife, in press), the core of the philosophy of naturalism is the notion that matter is sufficient for understanding the person. No other factors than the biological are needed for explanation. This philosophy of sufficiency is the core of the problem, as we see it. Because we assume philosophically the sufficiency of the biological before investigation, we do not need to critically examine biological research after investigation. The sufficiency of biological explanation is a truism, already accepted "hook, line, and sinker" in our background assumptions about the world, before any data has been gathered. Data gathering, in this sense, is basically window dressing, validating what we already know to be true.

The problem is that we do not <u>know</u> it to be true. As scientists, we cannot accept it as true until it has been thoroughly tested. However, there is an even more compelling reason to make certain that it is thoroughly tested – this form of materialism can rob us of our humanity. If biological explanation is synonymous with the sufficiency of the material, then it is truly "sufficient," without need of and effectively eliminating any other factors. This form of materialism is appropriately labeled "eliminative materialism" because its sufficiency doctrine leads to the elimination of all the "quaint," "folk" conceptions of humans, such as love, meaning, and joy (Churchland & Churchland, 1998, p. 3).

Consider, for example, how materialism eliminates the conventional notion of love. Because natural laws govern the material, our biology can have no agency and love can have no meaning. When a large rolling boulder suddenly moves away from a hiker, we cannot say the boulder "loved" (or even liked) the hiker. The laws of nature determine the events of the boulder, as all events of nature (including human events) are supposedly determined. Without agency, humans cannot do otherwise; they have no possibilities. Saying you love someone when you have no possibilities is no more meaningful than a computer saying, "I love you." Because the computer cannot say otherwise, the meaning of love is entirely lost. As the Churchlands (1998) correctly note, the meaning is lost in all the other "quaint," "folk" conceptions of humans, including some of our most cherished virtues and most insidious vices. We are not different, in kind, from a rolling boulder.

My point here is that our uncritical adoption of the materialism of the natural sciences has come at an incredible price. Some of the most important aspects of our lives are not biological, at least not biological exclusively. Many psychologists have just assumed that this is the way things are – that we are stuck with the sufficiency doctrine. Well, I have good news and bad news. The good news is that the sufficiency doctrine is not the only way to understand ourselves and our biology. The bad news is that many psychologists automatically assume that any alternative is unscientific. Just as they have assumed that the biological and the material are automatically scientific, any deviation from this doctrine is, by the same specious logic, automatically unscientific.

Fortunately, nothing could be further from the truth regarding the alternative we have in mind. Indeed, we contend that this alternative fits the data better than the sufficiency doctrine. Moreover, it is a deceptively simple alternative. Instead of the material and the biological being the sufficient condition for explanation, understanding, and treatment, this alternative postulates that the material and the biological are merely a necessary condition. In other words, our alternative model assumes that an understanding of our biology is *necessary* to explain behavior but not sufficient *by itself*. This means that no biological account of mental, social, or even neurological functioning is complete without important additional factors like human agency.

Misconceptions of the Alternative

Unfortunately, three main misconceptions about this model prevent its wider acceptance. The first is that we already have necessary-conditions models in the many multifactor models available, such as the diathesis stress model (Meehl, 1962; Rosenthal, 1970; Zubin & Spring, 1977). The problem is, however, these multifactor models contain only naturalistic and materialistic factors. That is, the multiple factors they consider important are all material factors that interact through natural and deterministic laws, disallowing human agency, spirituality, and a host of other factors that do not fit the prevailing philosophy of naturalism. Models such as diathesis stress merely recapitulate the sufficiency doctrine to which we are trying to formulate an alternative.

Another misconception about the necessary-conditions alternative is that nonnaturalistic factors, such as agency, would themselves become sufficient conditions for the explanations of psychologists. An example would be the notion that the force of our will alone is sufficient to cure mental illnesses, such as depression – that agentic factors by themselves can remediate such illnesses. Unfortunately, this is often the way a nonnaturalistic alternative is depicted in the naturalistic literature – as a “strawman” to be knocked down. The reason it is so clearly a strawman is that a necessary conditions approach disallows any condition, including agentic factors, from being a sufficient condition, by definition. Necessary condition approaches are necessarily holistic, because the parts of the whole are necessary, but never sufficient, for understanding the whole and the parts. Treatment of a mental disorder, then, could never be merely biological or merely agentic; it requires both.

Does such a holistic conception create a philosophical dualism – the third of our misconceptions? Does it, for example, create a set of necessary conditions that are distinctly immaterial and thus completely nonbiological? The answer is clearly “no,” if one understands the holism I just described. All the relevant necessary conditions – all the parts of the whole – are intimately and inseparably related. They cannot exist without the others, because their very qualities originate directly from their relationship to one another. This means that agentic factors can never exist apart from the biological. Indeed, the agentic takes place through the biological, as several Nobel Laureate physiologists, such as Sperry (1995) and Eccles (1984), have observed. I mention these Nobel Laureate biologists to reiterate that it is not the biological itself that seems to grate against human agency; it is our philosophical <u>conception</u> of the biological, specifically its sufficiency, that makes these parts of the whole seem incompatible.

<u>Conclusion</u>

There is, of course, much more that we could say about this holistic alternative (e.g., Slife, in press). However, at this point, I should conclude with the five main points of this address. First, many observers consider psychology to be at the end of its "Jurassic Age." There is no question that biological explanation, especially in its sufficiency form, is increasing its already considerable influence. Second, the rigorous program of research that supports this sufficiency doctrine simply does not exist. Instead, there is considerable evidence that properly constructed placebos wash out the efficacy of antidepressants, <u>and</u> that none of the conventional biogenic theories of depression are correct (with none of the <u>un</u>conventional theories presently validated).

Third, this evidence has emerged <u>in spite of</u> the considerable economic pressures of the pharmaceutical juggernaut. The empirical evidence is substantial that these pressures have not only tainted significantly this program of research but also facilitated much of our ignorance about these methodological problems. Fourth, we would ask that you not underestimate the power of the unacknowledged philosophies at play here. Psychologists have been uncritical of these biological trends, in large part, because we have <u>presumed</u> them to be correct and inevitable. However, this is our background philosophy of naturalism telling us this, not our present data.

Finally, the only way to remain truly scientific, under these industry-induced and philosophically biased circumstances, is to erect a competent philosophical competitor, a competing explanation that creates doubt and questions in the minds of investigators. This competitor, we believe, is the necessary conditions approach. Indeed, we believe that with a fair and level “playing field” of research, the necessary conditions approach will triumph scientifically. Its triumph will not only give psychology a rightful place as a vital discipline but also help us to reclaim supposedly “folk” concepts, such as love and joy, that just might have been important to depression in the first place.

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