Harmful dysfunction and the search for value neutrality in the definition of mental disorder: response to Wakefield, part 2

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Abstract

Wakefield’s claims to have identified and objective scientific component of mental disorders in the concept of dysfunction are examined in light of previous attempts to state a value free concept of mental disorders. The harmful dysfunction concept of dysfunction is not value free because it confounds cause and purpose in a specious use of evolutionary theory and because evolutionary theory cannot reliably supply standards for when a function is broken. Harmful dysfunction analysis collapses into a value-laden concept of mental disorders and serves the untoward goal of promoting the status quo in the modern DSMs. If the concept of dysfunction were taken seriously and rigorously defined, then it might be possible to separate what is medical from what is not in the domain of mental disorders. © 2001 Published by Elsevier Science Ltd.

In this second response to Wakefield’s reactions to previous critiques of his views, I have focused primarily on his article “The concept of disorder as a foundation for the DSM’s theory-neutral nosology: Response to Follette and Houts, Part 2” (Wakefield, 1999b). However, the larger corpus of his writings on issues about the basic concept of mental disorders is always important to keep in view. Hereafter, I refer to Wakefield’s conceptualization according to his most recent convention, HD analysis for harmful dysfunction analysis (Wakefield, 1999c). This response is comprised of seven parts. To set the stage for exposing the fundamental flaws of HD analysis with respect to values, the first section reviews some previous attempts to identify a value free concept of mental disorder within the field of psychopathology. Such historical consideration of concepts of disorder is important because this background places HD analysis in context and
shows the persistence of the value-laden aspect of concepts of mental disorder. The second section outlines some perspectives on the role of values in science and scientific discourse. Again, this is propadeutic to analyzing the value-laden aspect of the presumably scientific component in the HD concept of mental disorders. The core of this critique of HD analysis occurs in the third section where I dismantle the HD concept of dysfunction and show that it is not value free but value-laden. This analysis shows the collapse of HD analysis into a poorly explicated value-laden definition of mental disorders. Section four addresses Wakefield’s claims that the modern DSMs are trouble shooting manuals and shows the falsehood of the analogy with automobile trouble shooting guides as well as the contradictions that are implied for other of Wakefield’s claims. Section five assesses the plausibility of Wakefield’s claim that HD analysis is actually accurate in describing the manner in which people in fact do make attributions of mental disorder. Section six re-examines the issue of whether or not basic definitions of mental disorder, such as that of Wakefield and the modern DSMs, have jurisdictional implications for different mental health professions. Finally, I conclude by stating what is useful about HD analysis and what is harmful about HD analysis within the broader context of how we mental health professionals have wrongly promoted a culture that demands a medical conceptualization of every imaginable human problem.

1. Background for the problem of values in defining mental disorders

The problem of identifying a conceptualization of mental disorders that is free from culturally relative values and morals has a long history. It is important to review that history in order to appreciate both the advantages and shortcomings of Wakefield’s harmful dysfunction analysis. In his first presentations of HD analysis as a hybrid conceptualization of mental disorders that combined value-laden and objective scientific components, Wakefield (1992a,b) reviewed some of the critiques of diagnosis as a purely value-laden concept. He also included some of the history of those who preceded him who claimed that there could be a defensible objective and scientific conceptualization of mental disorders. The purpose of reviewing such contributions in this context is to show just how difficult it is to specify a definition of mental disorders that remains more or less objective and still does the job of demarcating mental disorders from normal variations in behavior. Throughout such considerations, we should always remember that the null hypothesis in these matters is that deviant behavior is the outcome of normal human learning and adaptation rather than the outcome of some broken thing inside the person. The burden of proof is on those who seek to explain deviant behavior by other means.

1.1. Previous attempts at objective definition

Wakefield (1992b) provided a concise summary of previous attempts to rescue the concept of mental disorder from various charges that it was value-laden and without scientific foundation. These have included concepts of disorder defined as: (1) statistical deviation from either physiological or behavioral norms; (2) all conditions treated by health professionals; (3) conditions and behaviors that lead to biological “disadvantage.” Such efforts to arrive at an objective definition of mental disorders and thereby rescue the concept from charges of being value laden and culturally relative have occurred against the backdrop of numerous critiques from various camps of
the anti-psychiatry movement. A thorough review of those critiques is beyond the scope of this article, but it is worth noting that many of them continue to remain unanswered by either Wakefield or his several predecessors. In the material that follows, I reference some of those critiques as they parallel points I make about how HD analysis fails to provide a more objective and scientific basis for defining mental disorders. As it has become fashionable to dismiss the anti-psychiatrists of the 1960s and 1970s as part of a lunatic fringe responsible for current travails of the chronic mentally ill (e.g., Issac & Armat, 1990), not enough attention has been paid to their actual arguments and criticisms. To be sure, one can find a great deal of excess and outright falsehoods in the writings of many, but there are also some telling insights about the problems of stating a value free basis for defining mental disorders.

1.2. Disorder as statistical deviation

The idea that disorder can be defined by statistical deviation from some population average or deviation from an individual’s own average has the appeal of being simple and straightforward. Advocated by Cohen (1943), this idea has been incorporated into numerous definitions of disorder and disease (e.g., Kendell, 1975; Scadding, 1967). However, such an approach contains numerous problems. Does it make sense to count as disorders those deviations that may be highly valued such as intelligence, athletic ability, and physical attractiveness? Deviation from an average alone is not enough. The very concept of disorder requires that the deviation must be in some direction judged to be negative, and this again returns to the problem of values. What is to be the standard for defining the negative side of a deviation from some average? Is it better to have too much energy and personal charm or too little? It is important to note that the problem of the standard applies also to positive conceptualizations of mental health that appeal to “optimum functioning” and “positive striving” (see Kendler, 1999 for a recent critique). Whether the standard is some psychometric test or some biological performance variable, all such standards derive from some prior decision about what is valued. Some statistically deviant behaviors are very desirable. The statistical deviation approach to defining disorder implies that “normal” is a matter of being mal-adjusted to the proper degree. Such a statistical approach to defining disorder comes very close to equating mental health with conformity, and the idea of statistical normality has been used historically to justify any number of social ideologies about one group’s superiority to another’s (Hacking, 1990). As Wakefield (1992b) has noted, statistical approaches to anchoring the concept of mental disorder lead to numerous contradictions and absurdities that call for a different approach to stating a more objective and scientific definition of mental disorders.

1.3. Disorder as what professionals treat

Another approach, associated with Taylor (1979, 1980), has relied on a concept of disease or disorder that is based on what defines the class of people called patients. In other words, rather than propose some ideal of what a disorder is, this approach seeks to find what may be common and universally defining about the class of people called patients. According to this formulation, a disorder is a cluster of attributes considered abnormal by the individual and/or by the population, and these attributes also arouse concern for the individual, society, or medical professionals. Taylor was not entirely satisfied with a traditional medical approach to disease that relied on the
presence of physiological lesions, and he rejected the notion that disease could be defined according to biological norms of the species (Taylor, 1981). In effect, what Taylor proposed was a sociologically more or less objective definition of disorder by stating the conditions that lead to patient-hood. Taylor (1976) also clearly recognized the conventional nature of the norms that were applied to determine what was normal and what aroused concern. Convention, which is contingent, rather than an objective biological fact, which is less contingent, was the basis for distinguishing between order and disorder. Goodwin and Guze (1989) took a similar approach.

Another objection to the disease or medical “model” arises from a misconception about disease. Disease often is equated with physical abnormality. In fact, a disease is a category used by physicians, as “apples” is a category used by grocers. It is a useful category if precise and if the encompassed phenomena are stable over time. Diseases are conventions and may not “fit” anything in nature at all. Through the centuries, diseases have come and gone, some more useful than others, and there is no guarantee that our present “diseases” medical or psychiatric will represent the same clusters of symptoms and signs a hundred years from now that they do today (Goodwin & Guze, 1989, p. xiii).

By this definition, disease is whatever the physician deals with. This approach has the advantage of being all inclusive of de facto practice, but it does not deal with the problem of objectivity versus value relativity in defining disorders. If anything, this approach merely blesses whatever values inform current practice and takes those as definitional of disorder. Such a definition fails to provide the physician with a non-arbitrary criterion for deciding that something is not a disorder. One can also see the fault in this line of thinking by asking what would happen to health care costs if in fact this type of model of physical and mental disorders was widely adopted. From such a social perspective, if disorder is whatever the medical profession treats, then there is no limit on what is a disease and there is no way to distinguish between fraudulent and legitimate medical practice. In fact, as I will discuss at the end of this article, the expansion of the modern DSMs has brought us perilously close to just this state of affairs.

In many respects, Taylor saw clearly the problems with various attempts to reify disease concepts and to anchor them in biology as opposed to anchoring them in social practice even though Taylor himself hoped for eventual biological grounding of disorders. A major problem with such an approach occurs for those who, like Wakefield, want to find some objective definition of disorder that does not depend upon social convention. To define disorder as whatever professionals treat seems to be arbitrary and subject to considerable fluctuation based on such matters as fad, fashion, and the mere availability of professional help (Kendell, 1975). Certainly, no one thinks of pneumonia as a disorder merely because medical professionals treat it. Something more than mere professional attention and social concern would have to be part of any objective definition of mental disorders.

Such issues also point to another important feature of various definitions of mental disorder. There is more at stake than the legitimation of mental health professions. If we cannot say clearly what is and what is not a mental disorder and if we cannot ground the concepts in an objective manner, then there are ripple effects throughout the social fabric ranging from basic research on behavioral functioning to uses of definitions in judicial proceedings and insurance plans. One gets a sense of the trepidation felt by those who have fought to rescue mental disorder from mere
social convention when Wakefield (1999d) stated: “Spitzer (1999) notes that, if ‘something is wrong’ is delinked from design failure, it becomes a matter of values” (p. 469). Without some objective anchor, the specter of irrationalism arises and the bogeyman of the mad anti-psychiatrist threatens to return.

1.4. Disorder as biological disadvantage

Most recent attempts to ground the definition of mental disorders in a scientific and objective knowledge base have appealed in one way or another to various concepts of biological disadvantage. For example, Scadding (1967) proposed that a disorder was some excess or some defect that was statistically deviant within a species and that led directly to a biological disadvantage on average for the affected group as compared to the non-affected group. His paradigm case was asthma.

In medical discourse, the name of a disease refers to the sum of the abnormal phenomena displayed by a group of living organisms in association with a specified common characteristic or set of characteristics by which they differ from the norm of their species in such a way as to place them at a biological disadvantage (Scadding, 1988, p. 121).

Scadding noted that there were evaluative components in this approach, and he hoped that statistical studies would provide the basis for making the concept of disorder more objective. The evaluative components entered in the choice of defining characteristics and in the specification of the meaning of biological disadvantage. When a defining characteristic could be specified as a deviant anatomical structure, a broken physiological function, or a proven causal factor for either of the former, then the problem of values entering into the defining characteristics of the disordered group was minimized. Criteria for brokenness were more or less objective. In contrast, when the only level at which a disorder could be specified was in terms of signs and symptoms, i.e., syndromally (as in the modern DSMs), then the problem of values entering into the defining characteristics of the disorder was much more problematic. Scadding specifically noted that affective disorders in psychiatry were exceptions to the general hope of providing objective defining characteristics for disorders. As regards the problem of providing an objective definition for biological disadvantage, Scadding was rather vague and offered only the concepts of disadvantage as a threat to life and disadvantage as an inability to undertake normal activities. He simply stated that he believed it would be possible to define disease as an objective deviation from some norm, but he also noted that he did not believe this would be possible for some psychiatric disorders that were defined only in terms of symptoms.

Did Scadding succeed in providing a value free definition of disease and disorder? The answer is clearly, no. Biological disadvantage was never clearly defined. To define biological disadvantage as not being able to undertake normal activity is inadequate because this merely pushes the question back to: How shall we define normal activity? What Scadding did accomplish was to clarify why we do worry and why we should worry about the value-laden nature of psychiatric disorders and why in contrast, we generally do not have as much worry regarding genuine medical disorders. He accomplished this through his analysis of the hierarchy of knowledge in the devel-

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1 I have introduced the term “genuine” medical disorder to draw a distinction between medical disorders, most of which have a
velopmental pattern of how diseases have been named in traditional medicine. I refer to this developmental scheme as Scadding’s ladder.

Scadding (1988, 1992, 1996) recognized that there was a hierarchy in the development of disease concepts in medicine and that changes in the naming of diseases often reflected this hierarchy. Disorders were first specified in terms of signs and symptoms. Scadding regarded this elementary clinical syndrome level of analysis to be a mere starting point. Interestingly, he regarded most psychiatric disorders as remaining at this primitive stage of development. A second level of development in the accumulation of knowledge about disorders and the naming of them was evident when mere syndromal description was replaced by associated structural anomalies, as when the knowledge of tubercles were associated with the syndromally defined early description of tuberculosis (or consumption). A third level in the hierarchy of knowledge about diseases was discovery of disorder of function or of pathophysiology. Finally, according to Scadding, what medical nosology aimed for was etiology, to identify what caused the deformation or morbid structure and to demonstrate what led to failed functions as when the tubercle bacillus was isolated. In the case of tuberculosis, Scadding noted that the identification of the cause of the syndromally defined disorder and of the morbid structure defined disorder led to a reduction in disease labels: “This led to the unification of a number of diseases previously defined syndromally or morbid-anatomically into a single aetiological defined category” (Scadding, 1988, p. 124). As I noted in the first part of my response to Wakefield, such a development stands in sharp contrast to the expansion of labels for mental disorders in successive editions of the modern DSMs. Furthermore, if the Scadding type of developmental analysis is correct regarding medical disorders, and I think that it generally squares with the historical evidence, then Wakefield’s appeals to medical analogies to defend the label proliferation of the modern DSMs is just plain wrong, which I previously noted (Houts, in press a). According to Scadding’s developmental analysis of progress in medicine, progress would occur, not when syndromally defined disorders proliferate as in the modern DSMs, but when syndromally defined disorders are reduced by etiological explanation.

Scadding’s (1988) developmental analysis of knowledge and the naming of disorders within medicine clarify Wakefield’s attempts to rescue current psychiatric disorders from the charge that they are not objective. Viewed within Scadding’s framework, what Wakefield has attempted to do in HD analysis is to move the concept of mental disorder from the elementary and beginning conceptualization of a syndrome to the next level of analysis wherein disorders are defined by the presence of a dysfunction. In the actual history of physical medicine, this transition from a focus on syndrome to a focus on broken function took place not by verbal decree but by amassing evidence for broken functions. In the course of defending and improving the modern DSMs, what Wakefield has proposed is a mere edict that disorders in the modern DSMs are there because they have associated dysfunctions. Such a rhetorical move is certainly understandable in light of Scadding’s analysis because making such a move does in fact take us one step closer to a more objective basis as contrasted with many current psychiatric disorders, which lack a similar objective basis. The distinction is played out in this paper, and I leave it to the reader to decide if the distinction between “genuine” and its antonyms makes sense. A reviewer of an earlier paper of mine took offense at the expression “genuine medical disorder” as if it cast aspersions on the current state of psychiatric diagnosis, but that is exactly the point. Whereas I have a deep appreciation for the difficulties of conducting good scientific research into human problems, I have concluded that much the material in the modern DSMs is pseudoscience and pseudo-medicine and that Wakefield’s HD analysis actually exposes this even if inadvertently.
objective definition for mental disorders. The difference between what has happened with HD analysis and the modern DSMs and what has happened in physical medicine is striking. Physical medicine made such a transition when the knowledge of pathological function was amassed for a given syndromally defined disorder. In contrast, HD analysis has made the transition, but HD analysis has done so without the requisite knowledge. This amounts to science by decree rather than science by normal evidentiary toil and investigation. This is reminiscent of Kierkegaard’s quip that Danish Christendom was like a mighty army that went forth to battle, met the enemy, immediately retreated, and marched home to declare victory.

Another and slightly different version of mental disorder defined as biological disadvantage was offered by Kendell (1975). Kendell was eager to discredit the concept of disease as a physical lesion because in doing so he could discredit various critiques by anti-psychiatrists who had argued before and after the mid 1970s that mental illnesses were not really illnesses due to the fact that no lesions had been demonstrated (e.g., Ingleby, 1980; Szasz 1961, 1970; Sedgwick, 1982). What Kendell offered in place of illness as lesion was a modification and elaboration of Scadding’s (1967) biological disadvantage criterion. According to Kendell, a condition qualifies as a medical illness or disorder if the condition can be characterized by statistically abnormal features and the condition leads to increased mortality or reduced fertility. What Kendell added to Scadding’s model was to make explicit that the biological disadvantage had to be demonstrated quantitatively by measurement of death rates and production of offspring. He recognized that there were problems and inconsistencies with such a model in that medical conditions such as psoriasis would have to be removed from the category of diseases. Nevertheless, he dismissed such difficulties because he was so eager to arrive at some definition of disease that would clear up the problem of defining mental illness in an objective manner and thereby place mental illnesses on a footing of equal security to that of physical illnesses. Kendell (1975) concluded:

... we have adequate evidence that schizophrenia and manic-depressive illness, and also some sexual disorders and some forms of drug dependence, carry with them an intrinsic biological disadvantage, and on these grounds are justifiably regarded as illnesses; but it is not yet clear whether the same is true of neurotic illness and the ill-defined territory of personality disorder (Kendell, 1975, p. 314).

Especially important in this formulation was the concept of intrinsic biological disadvantage, a concept that Kendell did not elaborate except to say that any finding that a condition was associated with increased mortality and reduced fertility must also be not attributable to cultural responses to the condition. In other words, an intrinsic biological disadvantage must be one that occurred across cultures and across epochs within a culture. So slippery was the slope of putative objectivity that Kendell attempted to navigate, he ended up merely redefining social disadvantage as biological disadvantage. “If therefore, an individual is discriminated against and shunned by his fellows, it could well be argued that that in itself places him at a substantial biological disadvantage, and not merely a social one” (Kendell, 1975, p. 313). Whereas Kendell along with Scadding helped to move the debate about the value-laden nature of mental disorders away from simplistic disease models of lesions, they did not succeed in gaining an objective foothold in biological disadvantage. Biological disadvantage could not be separated from social disadvantage in many cases. Consider the highly intelligent dual professional couples that show lower birth
rates. They are clearly definable by characteristics that make them statistically abnormal, and they clearly show reduced fertility. Does it make any sense to call them mentally ill? We have invented the acronym DINS (Dual Income No Sex). Will this be a new diagnosis in DSM-V?

The theme of biological disadvantage has been carried over in Wakefield’s HD analysis but in a more complicated fashion where the concepts of dysfunction and natural selection play an important role in this latest effort to rescue the concept of mental disorder from mere value judgments and to anchor the concept in some objective scientific domain. Before addressing Wakefield’s HD treatment of objectivity and values in the definition of mental disorders, it is important to review briefly some general issues about the role of values in scientific discourse. Fundamental relations between science and values have undergone considerable change since the previous debates between proponents of objective definitions of mental disorder and their anti-psychiatry counterparts. In addition, Wakefield has not addressed some of these basic issues in his previous work.

2. Values in scientific discourse

There is a long tradition of work in the general domain of the role of values in science that has ranged from the early logical positivist’s and logical empiricist’s strict demarcation of facts from values through post positivist history, philosophy, sociology, and psychology of science. I have reviewed much of that development elsewhere (Houts, 1989a; Houts & Haddock, 1992; Houts & Krasner, 1980, 1998; Krasner & Houts, 1984), and numerous other reviews have been published (e.g., Herrnstein-Smith, 1988; Longino, 1990; Proctor, 1991). In order to focus such work on the present issue, I emphasize a recent tradition in the philosophy and sociology of science. This recent perspective and mode of analysis has been referred to variously as science and technology studies, social studies of science, and unfortunately, as social constructivism and social constructionism. I agree with Hacking (1999) that the latter two terms are used much too loosely, and I deploy some of his clarifications in what follows. This general line of thinking provides some important conceptual tools for dissecting Wakefield’s proposals to locate an objective definition of mental disorders by appealing to concepts of dysfunction and evolution. The conceptual tools also help to illuminate the salient differences between mental disorders and physical disorders. This analysis also carries forward and supplements points made in the first part of my response to Wakefield, which showed some important empirical differences between the modern DSMs and the classification of physical disorders in the ICDs.

A first thing to note is that in drawing on concepts of social constructivism, I am not denying the “reality” of schizophrenia or bi-polar disorder, for example. To focus on the excesses of social constructivist views and thereby dismiss the entire set of concepts as absurd would be easy enough and would also betray yet another simpleton approach to the difficult subject matter of explicating the role of values in defining mental disorders. Tables and chairs, quarks and black holes, reports of auditory hallucinations and grandiose delusions, are all real, and they all exist. This is trivial and obvious, so much so that one has to marvel at the circumstances under which their reality and existence would even be called into question (Latour, 1999). What is important instead are the different ways that physical disorders and mental disorders can be said to be real and can be said to exist. A social constructivist analysis helps to clarify some of these subtleties and illustrates some important differences between physical and mental disorders.
Hacking (1999) has provided a cogent analysis of the circumstances under which people appeal to social constructivist analysis. The circumstances are ones in which it is questionable whether or not a current state of affairs is inevitable or taken for granted. By questioning the present state affairs, social constructivist analysis helps to show how the present state of affairs has come to be taken for granted and how the present state of affairs has come to be experienced as inevitable. This inevitability, this taken for grantedness is generally what we mean by the idea of objective versus non-objective status of some claim. One piece of the picture has to do with two fundamental differences between the natural and the social sciences. The first difference is that the objects of inquiry in the natural sciences have an independence from human rules and practices. Neurophysiologists investigate receptor sites and second messenger systems that have a strong and different sense of entititity and objectivity that is quite different from the expressed emotion investigated by mental health researchers. Receptor sites and second messenger systems do not depend on human conventions for conducting family interactions. We do not expect receptor sites and second messenger systems to vary between one culture and another. A second difference between the objects of natural science and the objects of social science has to do with the non-interaction between the object and the idea in the natural sciences as contrasted with the social sciences. What happens at the receptor site or in the second messenger system does not change as a function of how we talk about those systems as we examine them in vitro. In contrast, naming expressed emotion and calling attention to it can change family interactions because people, unlike receptor sites, are conscious aware organisms whose behavior can be influenced by ideas. Non-interaction between object and idea and independence from human rules and practices are hallmarks of the “hard” sciences. Part of what makes them firm or stable is that what they study is independent and non-interactive.

In addition to independence and non-interaction, another contribution to the inevitability of knowledge claims in science comes from the idea that the current state of knowledge in the successful sciences has evolved in one way and that it had to do so. This is the non-contingency component of taking something for granted and experiencing it as inevitable. In retrospect, we simply cannot imagine the history of the discovery of the syphilitic spirochete as having turned out differently than it has. We simply cannot imagine that physics and chemistry could have depended on arbitrary and culturally relative practices and beliefs. Chemistry and physics are the way they are at present because nature dictated that they would turn out this way, and human conventions and cultural differences had little to do with the positive fruits gained through a process of discovery of the natural world. Another component of what convinces us that some current state of knowledge should be taken for granted and is inevitable is the concept of natural kinds. This is the idea that the object of inquiry has a structure and an essence that can be discovered. For example, Wakefield (1999d) has claimed that his notion of function and dysfunction are natural kind concepts that are based solely on a scientific rather than a value-laden foundation. Dysfunction is the concept that Wakefield uses to gain an objective foothold for defining mental disorders beyond the value-laden claim that they are “harmful” behaviors. As I show in what follows, Wakefield’s HD analysis of the concept of mental disorder collapses into a value-laden judgment about clusters of behaviors without some objective concept of dysfunction. A final component of the sense of objectivity as inevitability concerns the stability of knowledge. What convinces us that the present state of affairs should be taken for granted and what produces the experience of inevitability is the idea that current knowledge is stable because there is compel-
ling evidence. The current state of knowledge in a field is stable, not just because of social arrangements having to do with professions and learned societies and educational systems, but because there is compelling evidence. The stability comes from nature, not from arbitrary social arrangements that could change and thereby change the state of knowledge. Another way of putting this is that stability comes from inside nature and not from outside social forces.

It is instructive to compare and contrast the current state of affairs in physical disorders and mental disorders with respect to the aforementioned dimensions of relative objectivity. Why is it that we do not see a strong tradition of anti-medicine within the physical health field that is analogous to the strong tradition of anti-psychiatry within the mental health field? Why is it that we do not debate the value-laden nature of physical disorders in the same way that we debate the value-laden nature of mental disorders? Why is it that we do not see mainstream editorials and articles that lambaste and ridicule the ICDs, whereas we can find numerous examples of humorous jabs at the modern DSMs (Blashfield & Fuller, 1996; Davis, 1997; Kutchins & Kirk, 1997; Wylie, 1995)? Something peculiar is going on in the mental health field that is quite different from mainstream medicine.

Much of the difference between physical medicine and mental health has to do with what Hacking (1999) refers to as sticking points. Sticking points are those features of a domain of knowledge that lead to a greater belief and actual experience of the domain as: inevitable, not contingent; firm, not squishy; objective, not relative; scientific, not mystical; stable, not fickle; trustworthy, not bogus; hard, not soft; and any number of other polar opposites that imply objectivity and truth that are independent from human convention and values. Hacking proposed a rating system for assessing various philosophers of science as to their relative endorsement of constructivist views of science, and I suggest a minor modification here by including ratings of five dimensions rather than only his three. Consider the dimensions: contingency, nominalism, external explanations of stability, non-independence from human rules and practices, and interaction of the objects of inquiry. For each dimension, we can assign a value from 1 to 5, where 1 is at the extreme end of objectivity, and 5 is at the extreme end of social construction. Table 1 illustrates how this way of thinking can be applied to the differences between physical disorders and mental disorders.

Consider the dimension of contingency versus inevitability. This refers to the extent to which

| Table 1 |
| Sample ratings of dimensional differences between the science of physical disorders and the science of mental disorders<sup>a</sup> |
| --- | --- |
| **Physical disorders** | **Mental disorders** |
| Contingency vs. inevitability | 2 | 4 |
| Nominalism vs. natural kinds | 2 | 4 |
| Social vs. natural structure basis of stability | 2 | 4 |
| Dependence on vs. independence from human social conventions | 2 | 5 |
| Interactive vs. non-interactive objects | 2 | 4 |

<sup>a</sup> Ratings are on a 1–5 Likert scale where 1 favors the right hand side of the polar dimension and 5 favors the left hand side of the polar dimension. Higher ratings indicate that a knowledge domain is more “socially constructed” in accord with Hacking (1999).
we believe and experience the current state of knowledge as inevitable. When we think of physical diseases where the etiology of the morbid structure or the etiology of the broken function is known, we cannot imagine that the current state of knowledge would have turned out differently than it has. There is a sense that we have that nature has been revealed in the science. Pneumonia, tuberculosis, Huntington’s disease, etc. are true scientific discoveries. Real medical disorders have this type of knowledge where it seems that nature has spoken, and we have gotten it right. Our knowledge is unlikely to undergo drastic changes. Contrast this state of affairs with something like Post Traumatic Stress Disorder (PTSD). Although the modern concept of PTSD had predecessors in World War I shell shock and World War II combat exhaustion and gross stress reaction, the current concept of PTSD came about largely because of contingent social factors surrounding the problems of Vietnam veterans and the coincident revision of DSM-II (Scott, 1990; Young, 1995). PTSD remains a syndromically defined cluster of behaviors where the pathophysiology is unknown, and we certainly do not have the confidence that nature has spoken in our current state of knowledge for this condition (O’Donohue & Elliot, 1992). It is easy to see how current knowledge of PTSD could have been otherwise. It is also quite likely that PTSD could disappear in another half century, once the niche that gave rise to it is no longer present to support its continued use (Hacking, 1998), and the courts finally reject the improper extension of the diagnosis to excuse all manner of misbehavior (Saletan & Watzman, 1989). As contrasted with genuine medical disorders, PTSD has much more of the feel of invention than the feel of discovery. In this regard, PTSD is no different from several other conditions that were formerly considered mental disorders but are no longer considered such in the modern DSMs, e.g. homosexuality, neuroses, alcohol problems as personality disorders, etc. In mental disorders as contrasted with physical disorders, there is much more of a sense of contingency of knowledge and much less of a sense that current knowledge has reached some inevitable endpoint. In fact, one of the signs that a formerly regarded mental disorder such as general paresis has made the transition from contingency to inevitability, from mental disorder to genuine medical disorder, is just this sense that the etiology has been identified and current knowledge seems to have reached bedrock. Nature has spoken. For these reasons, Table 1 shows a score of 2 for physical disorders, and a score of 4 for mental disorders. 

Similarly, for the dimension of nominalism versus natural kinds, we have a great deal more confidence that tuberculosis refers to some underlying pathological process and structure than we do for PTSD. Borrowing from Skinner’s (1957) notion of the tact in verbal behavior, within the scientific community, the name tuberculosis makes contact with observable physical conditions that are always present when the outward syndromal pattern associated with tuberculosis is present if indeed there is a tubercular infection. Even a less clear cut case like asthma has a more or less solid physical location in terms of the broken functioning of the airways of the lungs. Contrast those physical disorders, genuine medical disorders, with PTSD. PTSD is much more a name for outward behavior and its history than it is a name for some physical structure. PTSD makes contact with a cluster of behaviors, but only under fairly restrictive conditions such as administration of a structured clinical interview. PTSD does not make reliable contact with any known broken function that we can locate in space and time. PTSD is much more nominalistic than physical disorders. Once again, PTSD is but an example from many possible examples of mental disorders. Outside of certain organic brain anomalies, some extreme forms of substance dependence, and occasional sleep and sexual problems, which might qualify for a score of 1 or 2 on the nominalism dimension, it would be difficult to argue that very many of the mental disorders listed in DSM-
IV belong below the midpoint of nominalism versus natural kinds. This is the type of reasoning that leads Table 1 to show contrasting scores for physical and mental disorders on the nominalism versus natural kinds dimension.

Consider the third dimension of social versus structural basis of stability. Again, Table 1 expresses the difference between physical and mental disorders. We experience physical disorders as stable because we are able to point to the structures that are broken, either morphologically broken or functionally broken. We can locate the source of the stability in the natural conditions, and these disorders do not vary in incidence merely because of social arrangements. If they do vary because of social arrangements, we consider that something has gone wrong in the diagnostic process because we know that these are natural kinds rather than social conventions and inventions. In contrast, a disorder like PTSD can be said to have been invented precisely because the stability of the condition has depended on social arrangements rather than upon mere natural conditions. What makes PTSD stick are the social conventions and practices surrounding our responses to extremely stressful life events. As evidenced by the history of its coming and going, PTSD is not stable for the same reasons that physical diseases are stable (see Scott, 1990; Young, 1995). One of the much heralded achievements of the modern DSMs, the narrowing of the diagnoses for schizophrenia and bi-polar disorder in the U.S., illustrates this point as well. The Cross National Pilot Project showed that prior to the reforms that came with the Research Diagnostic Criteria and the modern DSMs, American psychiatrists were using much more liberal criteria to diagnose schizophrenia than were their European counterparts (Cooper et al., 1972). DSM-III corrected this practice, but the correction was a social intervention, namely changing the social conditions of reinforcement for using the term schizophrenia. Stability was achieved by social means, not by the discovery of some natural structure that explained schizophrenia and distinguished this condition from manic depressive psychosis.

Overall, mental disorders are much more determined by human social conventions than are physical disorders. This is evident from the relative constancy of physical disorders as those appear in the ICDs as contrasted with the much more changeable nature of mental disorders as they appear in the transition from DSM-I to DSM-III. I provided quantitative and qualitative demonstrations of this in the first part of my response to Wakefield (Houts, in press a). Whether or not someone has pneumonia, does not depend on social conventions for diagnosing pneumonia. If two diagnosticians disagree on the basis of syndromal presentation and conventions for quantifying the syndromal presentation information, their dispute can be resolved in the laboratory with appropriate blood tests. In contrast, whether or not someone has expressed enough aggressiveness to qualify for a diagnosis of Intermittent Explosive Disorder is based entirely on social convention. No laboratory measure is available to resolve the matter if clinicians from two different cultures cannot agree on what constitutes “a degree of aggressiveness that is out of proportion to any psychosocial stressors” (APA, 1994, p. 612). On this dimension of dependence on human social conventions, the current state of knowledge about mental disorders is based much more based on social convention than is that of physical disorders. Table 1 shows the sharpest contrast between physical and mental disorders for the dimension of dependence on human social conventions. This is the dimension that Wakefield has focused on in his HD analysis in hopes of using the concept of a biological dysfunction to extirpate mental disorders from mere social convention and place them on a more objective footing.

Finally, the interactive versus non-interactive dimensional differences between mental and
physical disorders is obvious. What makes physical disorders firm, what makes them stick in terms of stability of knowledge is that they are not influenced by how we classify them. If we change the rules for the diagnosis of pneumonia, that rule change will not increase the incidence of actual infection as determined by laboratory studies. The viruses and bacteria that cause pneumonia do not adjust their behavior to accommodate or to counteract our rules for classifying them. Viruses and bacteria are not conscious aware beings. In contrast, in the case of mental disorders, we have numerous examples of people adjusting their behavior according to changing criteria for meeting a diagnosis. In the early days of using the PTSD diagnosis within Veterans Administration facilities, several publications that catered to veterans carried advertisements for instructional video tapes that taught veterans how to behave in order to get benefits then associated with the new diagnosis of PTSD. The object of psychiatric diagnosis, behavioral patterns exhibited by conscious aware humans, is an interactive object in the way that the object of physical disease, a pathogen or broken physical mechanism of a non-conscious entity, is not interactive. This is also expressed in the respective scores in Table 1.

Hacking’s (1999) dimensional analysis of what makes domains more or less like traditional concepts of the “hard” sciences provides some clarity on the difference between mental disorders and physical disorders. Such an analysis also calls attention to the notion that a distinction between facts and values is better understood when one conceptualizes their difference as along a continuum rather than as an absolute antinomy. In terms of logical reasoning, it may well be the case that one cannot logically deduce value claims from factual claims, but in terms of describing the relative facticity of domains of knowledge, the idea of a continuum between facts and values is useful and accurate. Concepts such as mental disorder and physical disorder are not either purely factual or purely value-laden. Instead, such concepts are more or less factual and more or less value-laden. Even in the case of hybrid, fact-value, concepts of disorder such as Wakefield’s, it pays to examine the so called factual or scientific side of the concept for its relative facticity. That is actually what much of the popular dissatisfaction with the modern DSMs is about. Ordinary people are understandably aghast to learn that such conditions as performing poorly at arithmetic and having trouble adapting to shift work are now considered mental disorders.

What recent developments in science and technology studies have clarified is that facts are socially negotiated claims made by scientific communities. This idea that facticity is a matter of degree provides a useful addendum to older distinctions between facts and values. Those older distinctions between facts and values, the so called fact-value dichotomy, are also important as tools for assessing the claims of HD analysis to have uncovered a “factual” or objective and scientific base for defining mental disorders.

2.1. Fact versus value distinctions

With roots in Kant’s two critiques, the traditional sharp distinction between facts and values can be traced to Moore’s (1903) Principia Ethica, which deployed the notion of logical types later associated with Russell (Whitehead & Russell, 1913) to claim that ethical concepts were of a different logical type than factual or empirical concepts. Moore used the distinction to show the futility of the naturalistic fallacy, the logical fallacy of drawing conclusions about what is morally good from consideration solely of facts obtained from the empirical sciences. This has also been referred to as the is versus ought distinction or problem (Frankena, 1973; Hudson, 1969;
MacIntyre, 1981). Leaving aside the somewhat different developments in continental European philosophy, this sharp distinction between facts and values was extended within Anglo American philosophy through the influence of logical positivism and logical empiricism on philosophy of science (Houts, in press b; Houts, 1989a; Krasner & Houts, 1984). Vestiges of the sharp distinction and recapitulation of the political concerns that gave rise to the sharp distinction in the 1930s are still visible in current treatments of the role of values in the social sciences (see Kendler, 1999). As noted previously, just as there has been a tendency within philosophy of science to consider facts as not absolutely disjunctive with values, so too, in moral philosophy and ethical theory, there has existed a tradition of finding some continuity between facts and values and a new understanding of the relationships between them (Edel 1955, 1961; Putnam, 1987; White, 1981). Just as the question of norms and values within science (epistemology) has taken a turn toward naturalistic epistemology where the traditional questions of normative philosophical discourse have been addressed by the empirical sciences of psychology and neuroscience (Fuller 1988, 1992; Kornblith, 1994; Magnani, Nersessian, & Thagard, 1999), so too, there is the possibility for a parallel naturalistic approach to ethics (Quinn, Houts, & Graesser, 1994a,b,c). The traditional positivist chasm between facts and values has been partially bridged, but the distinction remains a useful one, and the difficulties of establishing relationships between facts and values should not be minimized.

Drawing a sharp distinction between the domain of science as the domain of facts and the domain of ethics as the domain of values has served many purposes. One major use of this distinction has been to claim that “real” science is value free and that the ethical and moral problems surrounding scientific development have to do with the uses of the science by humans not with the science itself. According to such a view, science is said to produce knowledge that is objective in just the sense that it is not prescriptive with regard to human laws but only descriptive with regard to the laws of nature.

This kind of thinking where there is a sharp line between facts and values has informed much of the previous debate about whether mental disorders are fundamentally value concepts or fundamentally scientific concepts. I do not think that the distinction can be so sharply drawn as it once was, precisely because the notion of what is factual is no longer so clear for reasons outlined in the aforementioned analysis of the relative stability of physical as compared to mental disorders. Error, deception, and peril wait at either end of the continuum between facts and values. History is replete with erroneous science, which missed nature completely because of the intrusion of social values into the science (e.g., Joravsky, 1970). History is also replete pseudoscience that has been used to justify atrocious acts in the name of natural law and “science” (e.g., Kevles, 1985). The distinction between facts and values is important because so much rests on the distinction. The history of the problem of defining mental disorders in a scientific manner shows that this problem lies squarely in the middle of the dilemma of distinguishing facts from values. Wakefield’s HD analysis attempts to resolve the issues by admitting that mental disorder is a hybrid concept comprised at once of both a value component and a value-free scientific component. With the aforementioned tools and background in hand, it is possible to assess Wakefield’s claims.
3. Harmful dysfunction analysis and values

An important thing to note about HD analysis is that the concept of mental disorder is a value-laden concept first and foremost. On this point, Wakefield and I are in complete agreement. For Wakefield, the value judgment enters at the point of declaring that some behavior is harmful. In order for the behavior to be judged harmful, some standard is required for what is harmful as opposed to what is harmless. If there is no harm resulting from the behavior, there can be no mental disorder even if a dysfunction is present: “To be considered a disorder, the dysfunction must also cause significant harm to the person under present environmental circumstances and according to present cultural standards” (Wakefield, 1992b, pp. 383–384). Wakefield (1992b) has admitted that he did not delve deeply into the value-laden nature of mental disorders because he conceded that such a value component was necessary and obvious. In this respect, Wakefield differed from his intellectual predecessor, Klein (1978), who first introduced the use of evolution to define dysfunction and who apparently holds that dysfunction alone is both necessary and sufficient to define disorder (Klein, 1999). The HD agenda was to bolster the scientific standing of the concept of mental disorder rather than focus on the admitted value component. From its inception, HD analysis has focused on specifying a value free criterion by invoking the concept of dysfunction. Before examining the concept of dysfunction in HD analysis, it is important to note that without an objective definition of dysfunction, HD analysis amounts to stating that a mental disorder is some pattern of behavior that produces harmful consequences for the individual. What is harmful and who should set the standards are questions unanswered in HD analysis, and this lack of specificity regarding the value-laden issues has the effect of obscuring rather than highlighting the value component in HD analysis. Put another way, HD analysis rhetorically shifts the focus from a value component to a putative scientific component. In HD analysis, the crux of the matter is the claim that dysfunction is an objective anchor for defining mental disorder over and above rather arbitrary and culturally unspecified norms for what may constitute bad behavior.

This shift of emphasis from the value-laden nature of mental disorders to a so called scientific component has had the effect of justifying the status quo of the modern DSMs (Sadler, 1999) and of providing a scientific patina to the concept of mental disorder. It should come as no surprise that I regard the edifice of dysfunction erected by Wakefield, dysfunction as painted with the legitimacy of evolutionary theory, to be mere warmed over psychiatric ideology. The concept of dysfunction within HD analysis fails to provide an objective footing for mental disorders, and what is worse, it obscures the value-laden nature of HD analysis in its entirety. That conclusion is based on the following analysis, which shows that dysfunction is not a value free or purely scientific concept.

3.1. Under-specified functions

Wakefield defined dysfunction as “a scientific term referring to the failure of a mental mechanism to perform the natural function for which it was designed by evolution” (Wakefield, 1992b, p. 373). This quote, as do several others that follow, contains some peculiar assumptions and hidden claims. I have already commented on the issue of mental mechanisms and the lack of specificity in mentalistic hypostases (Houts & Follette, 1998), but it is worth noting again just how vague and how broadly Wakefield has defined such things that supposedly get broken in
mental disorders. This is the problem of under-specification of functions, the things that are supposed to go wrong, the things that do the objective explaining of the difference between bad behavior and genuine mental disorders.

Perception, language, learning, action, belief, emotion, thought, drive, and all other furniture of the mind have their distinctive functions that explain why they exist in the first place and why they have the structures that they do. So, although this account of “natural function” is in a broad sense biological, it is not in any sense physicalistic (Wakefield, 1992a, p. 236).

This appeal to evolutionary psychology has all the pitfalls of that approach. What is the distinctive function of thought that explains why it exists? Of course, humans are biological organisms, and of course, humans evolved by a process of blind variation and natural selection. But, it is highly likely that what we call thought is a side effect of the big human brain that evolved under selective pressures, and those selective pressures in turn probably had little or nothing to do with what we call thought. Mayr (1997) has noted that our current brain is the same brain that evolved 100,000 years ago.

The brain of 100,000 years ago is the same brain that is now able to design computers. The highly specialized mental activities we see in humans today seem not to require an ad hoc selected brain structure. All the achievements of the human intellect were reached with brains not specifically selected for these tasks by the Darwinian process (Mayr, 1997, pp. 74–75).

If Mayr is right, and he is at least an evolutionary biologist, then what sense does it make to argue that evolution can supply the normative standards of functioning for mental mechanisms? From the standpoint of evolutionary biology, the claim that thought or that any other psychological construct has a distinctive function, which explains why it exists, is simply vacuous speculation. We have absolutely no idea what brought thought into existence, and even if we did, we have no warrant for saying that those original conditions can provide a standard for judging whether or not thought has dysfunctioned in the present.

There is a twofold problem with Wakefield’s doctrine of dysfunction. First, the things (functions, mechanisms, parts) that go wrong, the things that fail to function “properly,” are grossly under-specified. Just consider the so-called “loss-response mechanism.” Wakefield (1998) has used this ephemeral notion to account for what has gone wrong in depression. This type of error is characteristic of the appeal to evolutionary psychology.

Evolutionary psychology uses modularity to atomize behavior into a priori, subjectively defined, and poorly separated items (not known modules empirically demonstrated by neurological study), so that selective value and adaptive significance can be postulated for individual items, as the ultra-Darwinian approach requires (Gould, 1997b, p. 50).

The appeal to a “loss-response mechanism” is a good example of how mechanisms can be freely invented without any reasonable constraint from empirical facts. The strategy seems to be, look at the behavior, declare it harmful; then, invent a mechanism that may have broken and
insist on knowing that it is broken by inventing a purpose that the mechanism must have been
designed to serve. Nuts. This is pseudoscience.

The under-specification of functions in HD analysis can be contrasted with the more specific
proposals of Klein (1999) who also subscribes to the notion that mental disorders can be objec-
tively specified by stating falsifiable hypotheses regarding broken functions. Klein has proposed
that the broken function in persons who display the behaviors indicative of panic disorder is a
physiologically based suffocation alarm that falsely indicates suffocation when the threat is in
fact not eminent (Klein 1993, 1994). In other words, what has gone wrong in these people is a
failure to regulate normal breathing under conditions of distress. Various aspects of this proposal
have been tested in empirical work (Horwath, Adams, Wickramaratne, Pine, & Weissman, 1997;
Klein, 1996). Although the false suffocation alarm hypothesis may eventually be rejected or may
end up accounting for only a specific subset of those individuals who present with panic symptoms
focused around breathing difficulties, the hypothesis is at least falsifiable in principle. The physio-
logical location of this putative mechanism is at least specifiable as likely in the limbic system.
Klein (1999) has also proposed testable hypotheses for cyclical conditions such as bi-polar dis-
order where the broken function is some homeostatic regulation system for affect. Others have
proposed that some forms of depression may be attributable to dysregulation of the hypothalamic
pituitary adrenal axis feedback mechanisms (Gold, Goodwin, & Chrousos, 1988a,b). None of this
requires evolution to state what has gone wrong, only further studies of normal physiological
functioning. Such hypotheses, if one day established, will elevate the conditions they explain to
the level of genuine medical disorders. In contrast, Wakefield has so under-specified the putative
broken mechanisms involved in mental disorders that scientific progress is impossible.

A second major problem with the HD doctrine of dysfunction is that the criteria, standards,
and norms needed to assess proper functioning of these under-specified functions cannot be
reliably extracted from evolutionary speculation. As I show in what follows, attempts to extract
norms of functioning from evolution are little more than projections onto nature of those norms
for functions that we humans value.

3.2. Dysfunction is not value free

Returning to Wakefield’s basic definition of dysfunction, one hidden claim is that there are
natural as opposed to some other kinds of functions. A second claim is that these natural functions,
as opposed to the other type, have been designed by evolution. The appeal to natural functions
is an appeal to the language of objectivity to signal that we are talking about nature as it is found
rather than human artifice and intervention. The appeal to nature is an appeal to the sticking point
of independence, independence from human wishes, desires, and values.

A disorder is different from a failure to function in a socially preferred manner precisely
because a dysfunction exists only when an organ [or function] cannot perform as it is naturally
(i.e., independently of human intentions) supposed to perform (Wakefield, 1992b, p. 381, ital-
ics added).

The HD appeal to nature in describing dysfunctions serves to obscure the influence of social
and cultural factors on the very functions that Wakefield mentions. Kirmayer and Young have made this observation.

Despite this attempt to separate fact and value, Wakefield's definition of dysfunction involves a set of implicit values. As used in HD analysis, dysfunction refers only to failures of internal mechanisms to perform naturally selected functions. This focus on internal mechanisms shifts attention away from social learning and interactional problems [what I regard as the null hypothesis]. The insistence that natural functions are defined by evolutionary design diverts attention from the role of culture and social context in creating new functions and ways of using the brain's faculties and consequently creating new sorts of psychiatric problems. Indeed, by implying that scientists' characterizations of functions are somehow "natural," evolutionary analysis serves to obscure the social construction of concepts of dysfunction and disorder (Kirmayer & Young, 1999, p. 450).

The immediately foregoing quote from Wakefield (1992b) contains another curious locution. In what sense can we know how an organ or a function is supposed to perform? The phrase "supposed to perform" implies some norm of performance. Where does this norm of performance come from? In ordinary medicine, this norm of performance generally comes from empirical studies and statistical norms of bodily functions and activities. The value we attribute to statistical deviations is a value we introduce because we value health or longevity or some other end. Nature does not care about statistical deviation, we do. In contrast, Wakefield replaces human intention with the "intention" of evolution, and this is where the value aspect of dysfunction is smuggled into the HD analysis.

What can it mean to say that some function has been designed by evolution if in fact most biologists assume that natural selection is "directionless, non-teleological, and materialistic" (Gould, 1997a, p. 35)? Wakefield is trading in metaphors. Throughout his writings, Wakefield repeatedly draws an analogy between the human designer of artifacts and design by evolution. This analogy, or rather the muddled use of this analogy, is the key to understanding why Wakefield's notion of dysfunction is not value free. The analogy between a human designer and evolution as designer has all the problems of Paley's (1831/1802) famous watchmaker analogy, which he used to "prove" the existence of God by an argument from design. Over twenty years before Paley had ever written a word of his now famous watchmaker example, Hume (1965/1779) had pointed that such arguments from design were fatally flawed. Collins (1967) provided a summary of the Humean objection to such arguments from design.

In the design argument, it is not simply a case of arguing from like effects to like causes in some degree. The real task is to show that the natural world must be construed as a crafted effect at all, in the sense of being the outcome of a making process somewhat similar to the process of making on the part of the intelligent human maker. The very aboriginality and uniqueness of the cosmogonic making process prevent it from belonging in a wider class of
makings which men can experience, and hence prevent the design analogy from having that experiential basis required for a sound analogical argument in everyday life and scientific inquiry (Collins, 1967, p. 56, italics added).

Wakefield’s analogy between the design of human artifacts by people and the “design” of mental mechanisms by evolution falls apart just as Hume indicated. The burden of proof is on Wakefield to demonstrate the logical necessity of concluding that so called mental mechanisms have been “made” by evolution just as automobiles and toasters are made by people. He has not done so, and he cannot do so because the “making process of evolution” is farther out of reach than even the big bang. It is amusing that we should be revisiting these issues of over two hundred years ago at the passing of the 20th century.

Fulford (1999) has provided a careful linguistic and logical analysis of the HD formulation of mental disorder, and in his response to Fulford, Wakefield (1999d) has missed the point. The value-laden nature of Wakefield’s treatment of dysfunction can be seen in the breakdown of his analogy between the design of human artifacts and the “design” of nature. In the case of machines that we have built such as automobiles, we have designed those machines to achieve certain goals like moving us about from place to place. In a molar sense, the function of a car is to provide us with transportation. We value transportation, and we brought cars into existence for this purpose. This means and ends type of analysis shows the teleological side of car functioning and exposes the value-laden basis for why cars are here. Because we designed automobiles to perform certain functions for us, we also know when they have failed to function. A dysfunctional automobile is one that does not provide transportation. If we move from the molar level down to a more molecular level of parts of the automobile, we can define the proper functioning of the ignition switch and easily specify when it has dysfunctioned. In Wakefield’s sense, we can say that what explains the ignition switch is the effect it has of starting the car. The ignition switch is there because it produces this outcome of starting the engine. We can do this analysis of function of the ignition switch because, in the first place, we designed the car to achieve the ends that we value, and we designed its components to perform various functions toward that end. We put the purpose in the car to serve ends that were important to us. So much for artificial functions that we designed.

Wakefield has attempted to apply the same type of reasoning to biological functions or natural functions. In his analogy, natural selection takes the role of the designer. This is how he gets to the locution that natural functions are those “designed by evolution.” What can it mean to talk as if evolution were a designer of functions analogous to human engineers as designers of automobiles? Wakefield (1999d) has referred to his most recent rendition of what this might mean by claiming that function is a black box essentialist concept. This is a retreat to obscurity. The steps in his reasoning are important to follow in some detail. Wakefield begins with an almost mystical admiration for the current state of biological organisms.

Organisms are unique in the way their functions are complexly related, hierarchically organized, remarkably beneficial, and produce a viable overall pattern of life and reproduction. There is no other natural context where recursive causal processes are so puzzlingly miraculous. It is not difficult to imagine how a recursively stable storm or whirlpool might accidentally occur; it is much more difficult to imagine how the nonaccidentally beneficial effects of organ-
isms’ mechanisms might have come about. It is assumed that there must be some process that explains how such benefits came to exist. The notion of function is created to label the results of this unique biological process, whatever it is (Wakefield, 1999d, p. 471, italics added).

What is crucial in this passage is the expression “nonaccidentally beneficial effects of organism’s mechanisms.” In agreement with Fulford (1999), Wakefield is saying that recursive processes of complex systems in nature can explain how random events may bring about ordered states of affairs as in weather systems, but Wakefield is going further to explain why the order observed in organisms is different. What makes the order observed in organisms different from the order observed in other natural phenomena is the nonaccidental beneficial effects. What is a nonaccidental beneficial effect of natural selection? Wakefield gives examples: “eyes seeing, hands grasping, feet walking, teeth chewing, fearing danger, and thirsting for water” (Wakefield, 1999d, p. 471). It is a total mystery what is nonaccidental about these functions. Why are these functions not every bit as accidental as any other outcome of evolution? Furthermore, the benefits are benefits to us. I am just as impressed as the next person with how well adapted to our environment we are in some respects. However, I assume this is due to the same accidental process of nature that also gives us some effects we do not now regard as beneficial such as a preference for fatty and sweet foods and the ability to produce excessive cholesterol. Later in the same paper, Wakefield goes on to admit that values enter into the definition of dysfunction, but he then takes this away in the same sentence.

This account explains why values play a role in defining function (the definition refers to prototypical nonaccidental benefits) but also why function is not a value term (to judge an effect a function is to make not a positive value judgment but a judgment that the effect is explained by the same process that explains prototypical positively valued effects) (Wakefield, 1999d, p. 472).

If I understand this, and I am not at all sure that I do, what Wakefield is saying on the one hand is that we cannot formulate the concept of function without believing that there is some plan in nature, i.e. there are nonaccidental beneficial effects of evolution. On the other hand, once we have so formulated the concept of function, the idea that various outcomes, good or bad, are the result of natural functions is not evaluative but merely descriptive. We are free to attribute various behavioral outcomes to be the effects of natural functions as much as we want once we have formulated the idea of function. But the problem here is not with regard to the concept of function. The problem is with regard to the concept of dysfunction. How do we know that a function is broken? Wakefield never answers this question except to say that a dysfunction is what happens when a function no longer does what nature intended. But how do we know what nature intended? Wakefield seems to be satisfied with the answer that we know what nature intended by listening to our own intuitions: “For now detection of design failures relies mainly on intuitive judgments based on circumstantial evidence” (Wakefield, 1999d, p. 465). This just does not make sense, and shows that the doctrine of function and dysfunction is not value free within HD analysis.

Throughout his discussions of function and dysfunction, Wakefield confounds the concept of function with the concept of purpose as Fulford (1999) has pointed out in his critique of HD
analysis. The fundamental muddle in Wakefield’s doctrine of function is evident in his holding to the analogy between evolution and the design of artifacts by human inventors. Grinnell has commented on this general problem with functional language as follows.

Consider the following three statements about mammalian hearts:

1. The purpose of the heart is to pump blood.
2. The function of the heart is to pump blood.
3. The effect of the heart is to pump blood.

The first and second statements are sometimes dismissed because of their teleological implications. In the first statement, for instance, the language can be mistaken for an analogy with conscious, purposive human behavior. While the purpose of artificial hearts might be to pump blood (the purpose belonging to the artificial heart’s inventor or the surgeon who implants it), hearts as they have developed evolutionarily have no conscious purpose insofar as one is aware. The third statement is a causal form in which nothing more is expressed than an observation of what hearts have been observed to do. From this statement, however, one cannot understand the significance of that particular activity of hearts with respect to an organism as a whole. This is the point of the second statement. That is, hearts not only pump blood; they also have a special relationship to mammalian organisms. This relationship is precisely what is implied when one states the heart’s function (Grinnell, 1987, pp. 40–41).

At times, Wakefield defines function in terms of number 3, the cause effect relationship: “When we say, in the strong sense, that the function of X is Y, we are offering a sketchy, partial, factual explanation of the existence, maintenance or structure of X in terms of one of X’s effects” (Wakefield, 1999b, p. 1015). At other times, Wakefield defines function in terms of number 1, the idea of purpose and intent.

It is obvious, for example, that the eyes do not accidentally enable us to see, that the hands do not accidentally grasp, and that the legs do not just happen to enable us to walk. Such complex beneficial effects are clearly what these mechanisms are “designed” to do; the mechanisms must somehow have been shaped by these effects. Until Darwin’s theory of natural selection, it was difficult to understand how this kind of purposive causal relationship could occur without a divine entity designing us with these effects in mind. Darwin explained how, without a designer, the effects of mechanisms could still play a role in explaining the mechanisms (Wakefield, 1999b, p. 1015).

The move from function in the sense of number 3 to the sense of function in the sense of number 1 is patently clear. By confounding function as cause and effect explanation with function as a teleological, purposive, and value-laden explanation, HD analysis accomplishes the logically impossible feat of turning values into facts.

There is an additional problem with the HD analysis of dysfunction and the question of values or normative standards. Wakefield (1999b) has attempted to skirt this issue by merely restating
his claim that the concept of dysfunction can be defined objectively and without reference to normative issues.

It is a misreading of my position to suggest that I attempt to extract values from evolution. That would indeed be a conceptually muddled and even dangerous approach (Wakefield, 1999b, p. 1020).

Protestations notwithstanding, HD analysis presumes that evolutionary theory can supply the standard for judging when a function is broken, i.e., when a dysfunction has occurred. As noted previously, the judgment of dysfunction requires some standard or norm for proper function. This entails a value judgment: What is normal, what is functional? How are we going to get a standard from what may be known about evolution? What are we to do if our knowledge of evolution suggests there may be multiple explanations for any given function? Wakefield blurs the lines between the distinctions of original naturally selected function, co-opted function, current utility, and nonadaptive side effects or functionless by-products of evolution. Though he denies it in several places, Wakefield is, along with most evolutionary psychologists (Buss, Haselton, Shackelford, Bleske, & Wakefield, 1998), prone to what Gould has referred to as adaptionist fundamentalism. Wakefield (1999c) has claimed that his appeal to natural selection to explain the purpose and proper function of some mechanism is not adaptationist, and he has informed us that Stephen J. Gould is confused for having accused evolutionary psychologists of being fundamentalist Darwinians by seeking to explain all manner of current functioning in terms of adaptation. In the present context of HD use of the concept of dysfunction, the issue surrounds the problem of being able to say what is the proper functioning of some mechanism and to be able to ground the definition of proper in some objective biological process. Otherwise, proper is merely a synonym for value-laden preferences. HD analysis must confront the problem that some mechanism may have evolved under one set of conditions so that its proper function was X (the original adaptive origin), but now that same mechanism has been co-opted for some other use so that its current purpose is Y (the co-opted function or exaptation). The issue here is: Which is the function against which we should evaluate dysfunction? Is it failure to perform X, or is it failure to perform Y? Wakefield (1999b) glosses over this difficulty by insisting that failure to perform either X or Y is equally a functional explanation and therefore just fine within HD analysis. This begs the question. The question is, how does Wakefield or anyone else decide between X or Y as the standard for proper functioning of some putative mechanism? To complicate matters even further, Gould introduced the notion of spandrels, which are, by definition, non-adaptive by products of selected mechanisms or structures.

In fact, Lewontin and I coined the term “spandrel” precisely to make this crucial distinction between nonadaptive origin and possible later utility. We did this in order to expose one of the great fallacies so commonly made in evolutionary argument: the misuse of a current utility to infer an adaptive origin (Gould, 1997c, p. 57).

The point here is that Wakefield repeatedly infers biological and objective purpose from current utility. In fact, by insisting that there is no distinction between what has been biologically selected
and what our intuitions tell us is the current utility of some function, Wakefield loses the possibility of grounding dysfunction in an objective biological base.

Within HD analysis, the concept of dysfunction is not value free. Without this objective anchor in biology, HD analysis collapses into a value-laden concept of mental disorders. What is worse, the net effect of HD analysis is to shift focus away from the value component of what constitutes harmful behavior and present the appearance of objectivity. In this sense, HD analysis is a mere handmaiden to the modern DSMs functioning to justify the status quo of thinking about mental disorders and occasionally improving on the reification of current diagnostic categories by suggesting that the mere intuition of a dysfunction is good enough.

4. Is the DSM a trouble-shooting guide?

Wakefield has claimed that the DSM is a trouble-shooting guide for how things can go wrong with the mind. He has drawn an analogy between the DSM and automotive trouble shooting manuals. A close examination of this analogy shows that it is fallacious for reasons similar to the aforementioned problems with arguments from design. The analogy with cars breaks down because we designed and manufactured cars, and therefore we know right answers about cars. We made cars to perform certain functions, and we designed the parts of cars to perform certain functions. We can say definitively whether or not there is a dysfunction in a transmission or a carburetor, and we can trace back the symptoms of aberrant car behavior to certain parts of the car. We can actually run independent tests on the transmission and on the carburetor.

In contrast to the situation with respect to automobile function and dysfunction, we have no comparable knowledge about humans and their behavior. If a car misbehaves, we know where to look and how to rule out hypotheses. If a human misbehaves, we cannot rule out many of the hypotheses that Wakefield has proposed. How do you, for example, rule out the hypothesis that “the loss-response mechanism” has failed to function properly. Where is “the loss-response mechanism?” Do you hook it up to a Rorschach to find out if it has malfunctioned? The DSM is a listing of behavioral covariations, and as Wakefield (1999a) admits, by deliberate design of so called theory neutrality, the DSM tells us nothing about what went wrong. The DSM is of no use as a trouble-shooting guide, because it almost never tells us where the trouble is. In HD analysis, the only thing the DSM tells us is the speculation that there is or must be trouble somewhere. In fact, the DSM tries to avoid telling us what is wrong, and according to Wakefield that is desirable. In mental health diagnosis, clinicians look at the behaviors and then assign a label. However, the label is by design of no use in telling us what went wrong, only that something or other went wrong. The only trouble that is shot with the DSM is the trouble or discomfort of not knowing how to explain the behavior. Instead of explaining the behavior and identifying the causes, we take false comfort in attributing the behavior to some wooly internal broken mechanism that cannot be specified or even checked. This is just psychoanalysis all over again. It is a recycling of what used to be called the medical model (Houts, 1989b).

There is a further problem with the trouble shooting guide metaphor for the DSM. The trouble shooting guide metaphor is not consistent with Wakefield’s repeated claims that the modern DSMs are analogous to the ICDs. To claim that the DSM is a trouble shooting guide breaks the analogy with the İCD. The İCD is not a trouble-shooting guide, and it is not used as one. The İCD is a
classification of diseases according to anatomical systems and their functioning. No physician examines the patient and then goes to the ICD to figure out what is wrong. The ICD is the outcome or result of extensive research. In contrast, the DSM is a declaration of disorders before the research has been done. The ICD is a collection of knowledge. The DSM is a set of conventions based on social consensus.

5. Does HD analysis describe attributions of disorder

At times Wakefield has asserted that the purpose of HD analysis is merely descriptive rather than prescriptive. In the foregoing analysis of how HD analysis fails to handle the value issues of the concept of dysfunction, I have presented the major the pitfalls of taking HD analysis as the best way to define mental disorders from a scientific standpoint. Controversies surrounding the descriptive claims for HD analysis have been discussed in some detail in the special section of the Journal of Abnormal Psychology that contained the debate between Lilienfeld and Marino (1999) and Wakefield (1999c,d) regarding a Roschian versus HD analysis of the concept of mental disorder. The issue of the adequacy of HD analysis to describe disorder attributions is at least resolvable by empirical research into the judgment processes of people. Here, I want only to note briefly that the claim that HD analysis provides an accurate description of the conditions for attributions to disorder is not supported by research that Wakefield himself conducted.

Kirk, Wakefield, Hsieh, and Pottick (1999) conducted an analogue study of the attribution of disorder by social work students in training. The judges read the same vignettes describing three adolescents who were presented as meeting DSM-IV criteria for conduct disorder along with three experimentally manipulated conditions of additional information. In the neutral or no additional information condition, the judges read the description of the behaviors. In the environmental reaction condition, the judges read the same description of behaviors along with additional information that normalized the behaviors by making them adaptive to current social conditions, such as gang violence in the neighborhood. In the third experimental condition called “internal dysfunction,” the judges read the same description of behaviors along with additional information that ruled out social and environmental circumstances. For example, the vignette subject’s family was in tact and normal sounding, the school and neighborhood were described as without violence, and the subject had a history of random temper outbursts. A better label for this condition would have been the absence of social and environmental factors rather than the internal dysfunction condition. The social work student judges did not see a mental disorder in the neutral presentation of behaviors 67% of the time. When given the obvious social and cultural influences information, the environmental reaction condition, the social work judges did not see a mental disorder 89% of the time. Finally, in the absence of social and cultural information that might “excuse” the bad behavior, the student judges saw a mental disorder 68% of the time, just the reverse of the neutral condition.

These outcomes were interpreted as supporting Wakefield’s claim that people infer there is a mental disorder when they infer a dysfunction, but in fact, the investigators did not report what inferences led to the differential frequency of seeing a mental disorder when antisocial behaviors were presented under different collateral information conditions. Based on the information provided in this study, a more consistent conclusion is that the social work students attributed anti-
social behavior to a mental disorder when they could not otherwise explain it based on current environmental conditions. In other words, the inference to mental disorder is an inference based not on knowledge of function or dysfunction, but an inference based on ignorance. Apparently, the subjects in this study inferred that a mental disorder was present when they were unable to attribute antisocial behavior to social learning factors. The process is not unlike the more general process of making mentalistic attributions in accord with folk psychology when the environmental contingencies that are maintaining the behavior are not obvious (Liegland, 1989; Mueller, 1995). An operational analysis of the social conditions for the use of mental disorder as an operant verbal behavior can show what the stimulus conditions and reinforcing consequences are and supply the meaning of the term in a behavior analytic sense of meaning (O’Donohue, 1989). Although it is beyond the scope of this article, a thorough operational analysis of the concept of mental disorder is a useful alternative to HD analysis.

6. Mental disorders as medical disorders: jurisdictional issues

Wakefield (1999b) has conceded that his HD analysis of mental disorders places mental disorders within the scope of medical disorders.

Because the psychological systems with which the DSM and the mental health professions are concerned are biologically designed, DSM-defined mental disorders are by definition a species of the general category of “medical disorder” (which encompasses diseases, traumatic injuries and all other failures of normal functioning), analogous to digestive disorders and circulatory disorders. Thus, the DSM does presuppose the “medical model” in the minimal sense that it is concerned with mental disorders (Wakefield, 1999b, pp. 1004–1005).

Leaving aside the obvious problems of arbitrarily stipulating concepts by definitional decree and the dubious phrase “biologically designed,” this is a rather clear statement that mental disorders are a subset of medical disorders, a position that was taken by Spitzer and some of the framers of the modern DSMs (Spitzer & Endicott, 1978). As previously noted, the statement that mental disorders are medical disorders was initially contained in a preamble to one of the drafts of DSM-III (Follette & Houts, 1996). This definition of mental disorders as a subset of medical disorders spawned a heated exchange between the American Psychological Association and the American Psychiatric Association over a three-year period preceding the publication of DSM-III. The following excerpts from a letter written by the President of the American Psychiatric Association to the President of the American Psychological Association on November 3, 1977, convey the issues then before the two professions.

... I would hope that you would be able to appreciate that as far as we are concerned, we have always regarded mental disorders as a subset of medical disorders. The only difference is that in DSM-III this will be explicitly stated. This is consistent with the entire approach taken in DSM-III of making explicit, concepts that have been only implicit or undefined....

You can continue to try to convince us that most of the mental disorders in the DSM-III classification are not medical disorders. You will not only fail to convince us, but we believe
that it is inappropriate for you to attempt to tell us how we should conceptualize our area of professional responsibility. You can try to convince us that even if we believe that mental disorders are medical disorders, we should not explicitly say so in DSM-III. You will not convince us of this either. We believe that it is essential that we clarify to anyone who may be in doubt, that we regard psychiatry as a specialty of medicine (J. Weinberg, personal communication, November 3, 1977).

In response to this letter, the American Psychological Association took two actions. First, the American Psychological Association established an emergency committee, the Morley committee, to devise and disseminate a competing nomenclature that could be used in place of DSM-III. Second and after consultation with a Washington law firm regarding the feasibility of a lawsuit on grounds of restraint of trade, the American Psychological Association made a request to the American Psychiatric Association to include in DSM-III a legally prepared disclaimer regarding the scope of practice in the mental health professions. Those actions taken late in 1977 became unnecessary when, in May 1978, Robert Spitzer sent a memorandum to the American Psychological Association informing them that the Task Force on Nomenclature and Statistics had decided to delete any reference to mental disorders as medical disorders because “to do otherwise would be non-productive” (R. L. Spitzer, personal communication, May 26, 1978). In subsequent writings, Spitzer described the decision to drop the reference to medical disorders as “agonizing” (Spitzer, 1985; Spitzer & Williams, 1982). On the one hand, the medical disorder statement was intended to be a statement to counter anti-psychiatrists like Szasz who had claimed that such concepts were mythical. On the other hand, the inclusion of such a statement would almost certainly mean that non-psychiatrists would refuse to use DSM-III. Spitzer and his committee chose the route of offending the fewest number of people to insure widespread adoption of DSM-III.

By 1979, the American Psychological Association disbanded the Morley committee and the project of devising an alternative to DSM-III. Eventually, Spitzer sought the endorsement of the American Psychological Association.

As you know, I feel partly responsible for some of the difficulties that have surrounded the development of DSM-III because of my original position that it was important to recognize that the DSM-III categories were a subset of medical disorders. It is by interacting with your group (American Psychological Association) that I have come to realize that not only are the “medical” assumptions of DSM-III not necessary, but they are actually a serious obstacle to the use of DSM-III by mental health professionals (R. L. Spitzer, personal communication, March 22, 1979).

The American Psychological Association withdrew the request to include a scope of practice disclaimer in DSM-III, and after numerous conflicts within the American Psychological Association regarding how to proceed, that organization issued a qualified endorsement of DSM-III.

The aforementioned history of the jurisdictional issues raised by asserting that mental disorders are medical disorders is at odds with Wakefield’s gloss on these matters.

The American Psychological Association apparently agreed [with Follette & Houts (1996) that defining mental disorders as medical disorders had jurisdictional implications] and argued
vigorously for the exclusion from the DSM-III of a statement that mental disorders are medical disorders. R. Spitzer and other DSM framers denied any such implication. Given that mental disorders are generally considered medical disorders whether stated by the DSM or not, and given that only a minority of patients with DSM disorders are treated by physicians, it would seem that Spitzer has been proven right (Wakefield, 1999b, p. 1005).

This rendition of the issues regarding the use of mental disorders in professional jurisdictional disputes presents as matters of fact, claims that are patently false. First, Spitzer clearly became concerned about the implications that defining mental disorders as medical disorders would have on other professions, most especially on their willingness to adopt and use DSM-III. Spitzer clearly changed his mind on the importance of calling mental disorders medical disorders in the context of quelling the opposition and gaining their support. It is simply not true that Spitzer denied the jurisdictional implications of defining mental disorders as medical disorders. Second, Wakefield’s statement that “mental disorders are generally considered medical disorders whether stated by the DSM or not” is a distraction and a falsehood. Who generally considers mental disorders to be medical disorders? How many of the 86,000 members of the American Psychological Association hold such a belief. How many of the 110,000 members of the National Association of Social Workers endorse such a belief? Wakefield’s statement is a distraction to trivialize the issue of professional jurisdiction as if there were no legitimate concerns about defining mental disorders as medical disorders.

A recent episode in the state of New York suggests that professional jurisdictional issues regarding use of a medical definition of mental disorders is still very much alive. The National Psychologist, a newspaper published by and for practitioners, reported the story. In spring, 1998, the legislature of New York State considered licensing bills for marriage and family therapists and proposed an omnibus bill for licensing of unlicensed professionals. The existing licensing law in New York for psychologists licensed only the title, psychologist, but it did not license the practice of psychology. In this context of reconsideration of licensing laws, the New York State Medical Society and New York State Psychiatric Association succeeded in introducing a bill that limited the scope of practice for psychologists to diagnosing and treating only non-medical conditions. If the bill had been passed into law, psychologists would have been precluded from using DSM-IV and CPT codes because they were defined as medical procedures for diagnosis and coding of service delivery. In effect, the contents of DSM-IV were declared to be under the scope of medical practice and exclusively under the scope of medical practice (Anonymous, 1999). The New York State Psychological Association (NYSPA) opposed the bill limiting the practice of psychology and introduced a new licensing law for psychology that specified the scope of practice. Dr. John Northman, chair of the NYSPA legislative committee, noted that “If you can’t use DSM-IV, you can’t get paid” (quoted in Anonymous, 1999). As of this writing, this particular effort to use DSM-IV to limit the scope of practice and to define mental disorders as medical disorders has not succeeded, and NYSPA is still lobbying to pass legislation about the scope of practice of psychology (J. Northman, personal communication, December 16, 1999).

What does the aforementioned analysis suggest? Wakefield’s HD analysis and definition of mental disorders are unabashedly in the tradition of making mental disorders a subset of medical disorders. Most of his writings, including the ones here commented on, have certainly cast a larger net than pathophysiological models of mental disorder. No mere physiological reductionist,
Wakefield has frequently talked about psychological and mental mechanisms that could be the locus of dysfunctions, and he has even allowed that experience could be the immediate cause for dysfunctions that are physiologically defined. It is curious that such a liberal all encompassing approach to defining mental disorders should come down to declaring that mental disorders are, after all, a subset of medical disorders. The reasoning appears to be along the lines that mental disorders are harmful consequences of biological failures of the human organism to behave as nature intended, and medicine is the field that documents and treats such biological failures. As has happened to others who preceded him, Wakefield has been caught between the Scylla of medical model exclusionism and the Charybdis of politically acceptable pragmatic inclusionism. On the one hand, if he is to maintain the support of Klein (1999) and Spitzer (1999) and other champions of mental disorders as medical disorders, he must emphasize the biological and medical picture of mental disorders. On the other hand, if he is to sell HD analysis to those who question the basic approach of the modern DSMs, he must define what is biological to include so called conscious and unconscious mental mechanisms as well as socially relevant events such as interpersonal rejection. This dialectic between the medical and the non-medical must be walked like a tightrope because there are real consequences even if unintended for leaning one way or the other. Wakefield is on the tightrope of preserving the status quo, which is a temporary truce between the warring mental health professions. HD analysis appears incoherent for this reason, waffling as it does between dysfunctions that are failures of physically defined mechanisms and ethereal will of the wisp mental mechanisms. HD analysis is ideology in the true sense of an idea that rises up from social conflict. This is no mere academic dispute.

7. Conclusions

For all its foibles and incoherencies as a defense of the modern DSMs, HD analysis has clarified a central issue about the concept of mental disorder and the role of the concept in the modern DSMs. There is something good in HD analysis, only it is not what Wakefield and his psychiatric supporters think it is. The major contribution of Wakefield’s HD analysis has been to show what is really needed in order to be able to say that a mental disorder as listed in DSM-IV is truly a medical disorder. By highlighting the concept of dysfunction, HD analysis has identified what would be required to place mental disorders on as sound a basis as we have come to expect for physical disorders or genuine medical disorders. By pointing to the concept of dysfunction as a key component in the generic definition of disorder, Wakefield has identified the component of syndromally defined disorders that can elevate them a step up on Scadding’s ladder of medical knowledge and diagnosis. To be sure, Scadding himself regarded the simplistic level of mere syndromal definition to be within the realm of medical disorders, and I am drawing a distinction that he did not draw. What is problematic about syndromal definition in mental disorders that is not nearly so problematic in the case of syndromal definitions of physical disorders is precisely the value-laden nature of mental disorders. In the case of mental disorders, all manner of obfuscation and pseudoscience has been and can be used to make undesirable behavior into a medical problem. By pointing to the requirement of dysfunction, HD analysis has actually identified a way to avoid manufacturing medical problems from normal variations in behavior and problems in living. Dysfunction if taken seriously and made a requirement for mental disorder diagnosis...
could help to resolve the problems of false positive attribution of mental disorder as Spitzer and Wakefield (1999) himself have recently pointed out. Unfortunately, Wakefield himself has missed the significance of his own achievement in this regard, because he has pursued an agenda of fundamentally accepting the modern DSMs and making minor improvements on them. In short, HD analysis has become an apology for the status quo and has outlived its usefulness (Sadler, 1999).

Ironically, the HD clarification of what is required for a syndrome to pass over into the realm of genuine medical disorder is also devastating for the standing of most of the disorders listed in DSM-IV. What if we took the criterion of dysfunction seriously? This is apparently, what Kutchins and Kirk (1997) had in mind when they commented as follows.

It is not that there are no such phenomena as mental disorders, that their existence is all a myth or psychiatric hoax. The point is that mental disorders constitute a small part of what is described in the current Diagnostic and Statistical Manual of Mental Disorders (Kutchins & Kirk, 1997, p. 264).

What if in order to be listed in the DSM-V, a syndromally defined condition had to meet the requirement of being even associated with (never mind caused by) an empirically verified, observable, and demonstrated broken function? How many disorders currently listed in DSM-IV could pass such a test? If this criterion were adopted for DSM-V, we would witness a precipitous drop in the number of diagnoses listed.

Wakefield’s HD analysis has also shown to what extent the modern DSMs are promissory notes rather than scientific achievements. By focusing attention on the concept of dysfunction, Wakefield has exposed the poverty of current knowledge regarding mental disorders. In order to make HD analysis into an apology for the modern DSMs, Wakefield has had to expand the concept of dysfunction well beyond its ordinary boundaries in medicine, and it is this groping for a broader concept of dysfunction that has been the demise of HD analysis. Let us face it: there is a difference between medical disorders and mental disorders. We do not see medical nosologists groping in the dark of misguided evolutionary speculation to obtain objective criteria for deciding when something is broken. As represented by the modern DSMs, psychiatry is not yet in the same ballpark as medicine just as psychology is not yet in the same ballpark as chemistry and physics. Pretending otherwise is silly. What is good about HD analysis is that it has shown us what to look for as a measure of scientific progress, namely, objective and verifiable broken mechanisms of the human body that might cause and maintain the pain and suffering of many of the behavioral covariations we currently regard as mental disorders. There is also harm in harmful dysfunction analysis.

So why all the fuss? HD analysis has generated a lot of heat in addition to some light. From the number of citations to HD analysis in this article alone, it is obvious that the question of defining mental disorders has created quite an academic stir. From an intellectual standpoint, I surmise that the controversies are numerous precisely because the problems encountered are old problems and ones that cannot be readily resolved. This is certainly true about the problem of value free versus value-laden views of scientific activity. The problem of defining good and bad behavior is also a perennial problem that lies at the heart of the concept of mental disorder.

Wakefield has noted that the concept of mental disorder is a hybrid concept, and this is what
is the source of the controversy. HD analysis locates the split in the hybrid concept as a split between a value component and a scientific component. We do not disagree about the value component, but we do disagree about the scientific component. As I have shown, Wakefield fails to establish the objectivity of the scientific component, but that does not mean that such a component cannot be established. In fact, we have examples of just such achievements in general paresis, epilepsy, Alzheimer’s disease, Huntington’s dementia, and a host of genuine medical disorders the symptoms of which are colloquially regarded to be mental. The problem with mental disorders as represented in the modern DSMs, the source of the controversies about mental disorders, is this split, the split between real medical disorders and pretend medical disorders. From an historical perspective, this is just the latest round in familiar struggle between skeptics and believers in the medical basis versus the social basis of certain human problems.

Where HD analysis has gone astray is to miss the social basis of what is currently called mental disorders. In trying to excavate a purely scientific component for the status quo in mental disorders, Wakefield has put his HD analysis to work as a kind of rational reconstruction of the modern DSMs. In doing so, he has provided a rationalization, an air of reason, to what has in fact been a social process of manufacturing medical problems out of life’s difficulties. Again, the problem with the modern DSMs is not that they do not contain any medical disorders. The problem is not that they do not contain some very good candidates for elevation to the status of genuine medical disorders. The problem is that the modern DSMs contain many so-called disorders that are patently not medical disorders and probably never will be regarded so in the future precisely because nothing is broken. What is wrong here is the insistence on a presumption that something is broken even when we cannot state what is broken beyond such platitudes as “anger regulation mechanisms, impulse inhibitory mechanisms, and cognitive mechanisms involved in mathematics learning” (Wakefield, 1999a, p. 985). Wakefield’s defense for this kind of sophistry is that we have to presume a broken function because that is what physical medicine did, and who can argue with such an approach. Such an appeal is based on a false analogy. The object of physical medicine was not social behavior. Presenting with a fever is not analogous to disobeying mom and dad and being nasty to your teachers.

Over the long run, the HD analysis focus on dysfunction may be helpful. In the immediate cultural environment of increasing medicalization of human problems, the use of HD analysis to justify the status quo is a waste. What is needed instead is an analysis of the concept of mental disorder that highlights the value issues rather than obscures them. Sadler (1999) noted that “it would be most interesting for Wakefield to apply as much analytic effort to the harm requirement as he has to the dysfunction requirement” (p. 436).

In this regard, it is important to keep in view that the modern DSMs have come about over the past two decades within a larger culture where we are witnessing massive changes to the health care professions (Starr, 1982). Not only are the modern DSMs touting medical concepts for human problems, our culture is awash in health care concerns and business enterprises based on those concerns and fears. The modern DSMs have found a niche in a world where managed care companies demand a medical diagnosis to authorize services. In addition, the pharmaceutical manufactures are selling a certain approach to human problems along with their medications (Healy, 1997; Valenstein, 1998). The old anti-psychiatrists were right about a few things. One of them was the position of skepticism regarding the applicability of medical thinking to certain classes of human problems. Until we can break out of the medical monolith, we will not be able to sort apart what is medical from what is not.
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