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# The diagnostic and statistical manual's new white coat and circularity of plausible dysfunctions: response to Wakefield, Part 1

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**Abstract**

Wakefield has claimed: (1) logical empiricist models of scientific progress support the view that expansions of the modern DSMs are consistent with such standards of progress; (2) diagnostic label proliferation of the modern DSMs is the same phenomenon as change in physical disease labels of the ICDs; (3) the concepts of disorder and dysfunction should not be separated. I show: (1) Wakefield has misrepresented traditional philosophy of science models of progress to serve his rhetorical aims; (2) Wakefield's claim that DSM label proliferation and ICD change are the same is empirically false; (3) failure to separate the concept of disorder from the concept of dysfunction leads to erroneous reasoning and label proliferation observed in the modern DSMs. © 2001 Elsevier Science Ltd. All rights reserved.

Readers encountering my exchanges with Wakefield for the first time in the current issue of *Behaviour Research and Therapy* can benefit from a brief chronology and background of previous exchanges. Such background is important for the context of current debates about the *Diagnostic and Statistical Manual of Mental Disorders* (DSM) and to evaluate the merits of Wakefield's (1992a, 1992b) harmful dysfunction formulation as an approach to defining the concept of mental disorder. In 1996, Follette and I (Follette & Houts, 1996) published an article that introduced a special section of the *Journal of Consulting and Clinical Psychology* devoted to critiques of and alternatives to the modern DSMs (DSM-III, DSM-III-R, DSM-IV). In that same journal Wakefield (1998a) subsequently published a critique of the original article, and upon invitation from the editor, we (Houts & Follette, 1998) published a brief response to Wakefield's first critique. These previous exchanges provided background for the current exchange.

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In a previous issue of *Behaviour Research and Therapy*, two additional articles by Wakefield (1999a, 1999b) extended his critique of the earlier work and provided his responses to our riposte (Houts & Follette, 1998) to his first critique, which had appeared in the *Journal of Consulting and Clinical Psychology*. My response here is in two parts. The present article addresses issues raised largely in Wakefield's (1999a) "Philosophy of Science and the Progressiveness of the DSM's Theory — Neutral Nosology: Response to Follette and Houts, Part 1." However, any partition of the issues raised by Wakefield's repeated defenses of his harmful dysfunction conceptualization of mental disorder is bound to be artificial, because the acceptability of the harmful dysfunction concept is one of the more important issues at stake in these debates. Accordingly, I have addressed the problems I see with Wakefield's defense of his conceptualization of mental disorder both in this response and in a second article. The second article is entitled "Harmful Dysfunction and the Search for Value Neutrality in the Definition of Mental Disorder: Response to Wakefield, Part 2." In that second article, I have analyzed in historical context and in more detail the issues pertaining to "objectivity" and "value neutrality" in various attempts to define mental disorder as a concept.

The present article is Part 1 of my response to Wakefield's most recent critique and contains four sections. First, I have recapitulated the basis for the original critique of the modern DSMs published in the *Journal of Consulting and Clinical Psychology* (Follette & Houts, 1996) and elsewhere (Follette, Houts & Hayes, 1992; Houts, 1989). Second, such a recapitulation has been important because in his current critique of the earlier work, Wakefield (1999a) has misrepresented the structure and substance of my claims, and he has then refuted that poorly conceived misrepresentation. I welcome the opportunity to clarify the context of my original claims, to correct Wakefield's exegesis particularly of Hempel, and to point out where I both agree and disagree with Wakefield's assessment of the status of the modern DSMs. Third, I have addressed the question: Are changes in the modern DSMs the same type of changes that have occurred in the comparable editions of the *International Statistical Classification of Diseases and Related Health Problems* (ICDs)? Answers to this question matter because Wakefield (1998a, 1999a), following Spitzer, has claimed that the expansion of diagnostic labels in the modern DSMs is analogous with the increase of diagnostic codes that occurred in the ICDs over a comparable time period. In this section, I have also addressed the various uses to which such claims have been put in order to highlight why it may matter if the evidence supports either comparability or noncomparability between DSM and ICD accretions of diagnostic codes. Fourth and finally, I have addressed issues raised by Wakefield's introduction of the concept of plausible dysfunction as the criterion for inferring that some aberrant behaviors indicate the presence of a mental disorder. I have focused particularly on the issues of circularity in positing dysfunctions and on the absence of any clear standards for formulating hypothesized dysfunctions or of any criteria for falsifying inferences to hypothetical dysfunctions. This final section of Part 1 of my response to Wakefield has raised concerns about his use of the concept of dysfunction and has served to introduce Part 2 of my response, which focuses in detail on the problems of claiming value neutrality in the definition of mental disorders.

In this article, I contend that Wakefield's (1998a, 1999a) analysis of my views and of the modern DSMs attempts two rhetorical moves: (1) to clothe the modern DSMs in the white coat of scientific success and progress exemplified by modern physical medicine and instantiated in ICD changes, and (2) to introduce a new criteria of "plausible dysfunction" in order to maintain

his harmful dysfunction formulation of mental disorders as a way to advocate for a medical analogy of disorders in the mental health field. I have concluded that the evidence fails to support Wakefield's claims for comparability of DSM expansion with ICD changes. His analogy between the modern DSMs and the ICDs breaks down upon close inspection, and therefore fails to support the rhetorical strategy of clothing the failures of the modern DSMs in the white coat of success associated with physical medicine. Although Wakefield might claim that he has not explicitly said that the modern DSMs have been following the same course as physical medicine, such a claim is clearly a subtext of his writings, and others have viewed his work as advocating such a position (D. F. Klein, personal communication, 20 December 1998; Spitzer, 1997). After refuting the claim that DSM expansion was comparable to ICD change, I have showed that continued adherence to a medical analogy in the mental health arena has lead to circular appeals to ever more ephemeral so called "dysfunctions."

## 1. Recapitulation of my initial critique of the modern DSMs

In Part 1 of his current critique, Wakefield (1999a) has consistently misrepresented my views, mostly through exaggeration and parody. To clarify my views, I have briefly outlined the original criticisms of the modern DSMs, especially those criticisms drawn from traditional philosophy of science models of scientific progress.

The structure of my approach began with the observation that mental disorder diagnostic labels have proliferated from DSM-I (APA, 1952) through DSM-IV (APA, 1994). Wakefield (1998a, 1999a) and I have agreed about this overall fact, even though we have disagreed on some of the less important particulars, such as whether to treat verbal modifiers of labels with the same numeric code as separate diagnostic labels in the effort to enumerate diagnostic label expansion. Such particulars aside, the major issue between us has been how to *interpret* the basic fact of diagnostic label increase across subsequent editions of the DSMs. What does it mean that we are witnessing such large increases in the number of officially named mental disorders over this relatively brief period? On the one hand, proponents of the modern DSMs have interpreted this fact as a sign that the mental health field has been making scientific progress by discovering heretofore unrecognized mental disorders and by making more significant and finer discriminations between existing disorders (Andreasen, Flaum & Arndt, 1992; Glass & Vergare, 1994; Guze, 1995; Klerman, 1986; Nathan, 1998; Wakefield, 1996; Weissman, 1987). On the other hand, opponents and critics of the modern DSMs have taken this proliferation of diagnostic labels to be a kind of social invention of mental disorders that expands the social and economic power base of mental health professionals (Caplan, 1995; Clark, Watson & Reynolds, 1995; Kendell, 1991; Kirk & Kutchins, 1993; Kutchins & Kirk, 1997; Schacht, 1985; Wylie, 1995; Zimmerman, 1990). Rather than take an immediate side in that scientific discovery vs social invention debate, I proposed an alternative analysis. I reasoned that those who were making the claim that the expansion of the DSMs was a sign of scientific progress as well as those who were making an opposite claim must have had some idea of what constituted scientific progress. I proposed an alternative analysis based on the prior question: What constitutes scientific progress? That is, if people are claiming that the fact of diagnostic label proliferation is a sign of progress, what is their standard of progress? Similarly, if people are claiming that the expansion of diagnostic labels

is a sign of the lack of scientific progress, what is the standard of progress invoked? How can we know if taxonomy proliferation is a sign of scientific progress? The very question presupposes that we have some standard or model of progress.

In order to find out how to interpret the fact of diagnostic label proliferation, I reasoned that it was first necessary to find a standard of scientific progress. This was exactly the reason why I first introduced historical and philosophical models of scientific progress into discussions of DSM expansion (Houts, 1989). Other commentators had alluded to these problems (e.g., Faust & Miner, 1986), but no one had explicitly used rationally reconstructed models of scientific progress to assess developments in the modern DSMs. The very idea of progress contained a normative element; what is the standard or ideal that defines progress? I stated my conclusion at the beginning of the 1996 article: "As a result of poorly explicated theory, there is little evidence that the DSM is producing scientific progress as judged by *some* philosophical ideals" (Follette & Houts, 1996, p. 1120 italics added). I had considered only one quite traditional formulation of scientific progress, one outlined in logical empiricist philosophy of science as articulated by Hempel (1965a) and others (Feigl, 1969; Nagel, 1961). I had selected this particular perspective on scientific progress for three reasons: (1) Hempel (1965a) had written specifically about the role of taxonomy in the sciences and had commented on psychiatric nosology quite specifically, (2) logical empiricism emphasized the role of theory in organizing observations and provided a logic for theory development, something that previous critics and proponents of the modern DSMs claimed to be important, and (3) I believed that many in the mental health disciplines still held beliefs about science that were best characterized as some version of logical empiricism (Krasner & Houts, 1984; Houts & Krasner, 1998). In retrospect, I may not have been as clear as I should have been. I never intended to reify or make absolute standards out of logical empiricist notions of progress. In fact, I have been quite aware of the limitations of such models of scientific development and in some respects favor more historicist philosophical (e.g., Feyerabend, 1981; Kuhn, 1970b; Lakatos, 1970; Laudan, 1977; Shapin, 1994; Shapin & Schaffer, 1985) and sociological (Brannigan, 1981; Collins, 1985; Gilbert & Mulkay, 1984; Knorr-Cetina & Mulkay, 1983; Latour & Woolgar, 1986; Latour, 1987) models of science, even though both of the latter contain many disagreements regarding concepts and standards of scientific progress (Fuller 1989, 1993; Gross & Levitt, 1994; Hesse, 1980; Holton, 1986; Hoyningen-Heune, 1993; Leplin, 1984; Longino, 1990; Suppe, 1974). My point was that in defending the interpretation that mental disorder label proliferation signified scientific progress or even the lack of it, one also had to put forward some model of scientific progress. It seemed to me that most of the claims that had been made regarding the expansion of DSM labels as signifying progress, or the lack of it for that matter, were implicitly endorsing a rather traditional view of what constituted progress. Logical empiricist ideals provided a relevant standard for assessing the phenomenon of diagnostic label expansion.

I claimed that according to a logical empiricist covering law model of scientific progress, the proliferation of diagnostic labels witnessed in the modern DSMs failed to signify progress. In other words, taxonomic progress such as occurred in biological taxonomy and physical chemistry was not replicated in expansions of the modern DSMs (Follette & Houts, 1996; Follette et al., 1992; Houts, 1989). Indeed and in retrospect, according to this type of model of scientific progress, the lack of any theory guiding the expansion of the DSM was bound to lead to failure in scientific progress, a point that had been alluded to in several other analyses found in the critical literature on the modern DSMs (Faust & Miner, 1986; Kendell, 1991; Margolis, 1994).

## 2. Wakefield's rhetorical strategies

Wakefield (1999a) has responded to the aforementioned claims with several disconnected counter arguments, some of which were substantive and some of which were parodies of my views designed to lead into reductions to absurdity. The first line of counter argument was to dismiss the relevance of philosophy of science for adjudicating the progressiveness of the mental health field as displayed in the expansion of labels in the modern DSMs:

> They [Follette and Houts] argued not only that the DSM is scientifically unprogressive but also that the DSM is responsible for the mental health field's general lack of scientific progress. An unusual feature of the critique was that, although its central claim concerned the effects of DSM's nosology on research progress, no attempt was made to evaluate the massive research literature based on the DSM. Instead, the critique was based entirely on philosophy-of-science principles … that were claimed to apply to all scientific disciplines (Wakefield, 1999a, p. 964).

This is a parody of my views. I have not ignored research literature on the DSMs, but examining that literature could never tell us if adding categories within a taxonomic system was generally a good idea or a bad idea, a sign of progress or the lack of progress. Interestingly, Wakefield himself has not cited "the massive research literature based on the DSM" to support his own claim that the modern DSMs have been scientifically progressive. Examining the research literature on the DSMs would indeed be helpful and mandatory if one wanted to assess the reliability and validity of diagnoses, but some relative standard for reliability and validity was not the issue others and I had raised. Instead, the issue was whether or not adding categories to a nosological system constituted scientific progress according to some known model of scientific development that contained a standard of progress. I simply noted that any claims about the progress or lack of progress signified by the expansion of the DSMs required some specification of a model of scientific progress with some standards that could be applied to the case of the DSMs. I outlined one of many possible models and assessed the modern DSMs relative to that ideal of progress. I never stated, nor do I believe that a logical empiricist model applies to all scientific disciplines. In fact, someone could make a case that even though such a model fairly and accurately summarized past developments in biology and physical chemistry, it may not apply to medicine or to the mental health field. However, Wakefield has never made that case in any detail, nor has he proposed some alternative philosophical or historical model of progress against which we can judge DSM expansions. All Wakefield has offered is a claim that the DSM expansions are analogous to changes in the ICD, a claim that I evaluate and refute later in the present article.

Other parodies of my views were presented in rhetorical appeals to reduce my claims to absurdity. For example, Wakefield (1999a) characterized me as claiming that: "The categories in a scientifically progressive classification system must be derivable from some overarching theoretical principles …" (p. 965). I never claimed that overall theoretical principles *must* be present for a taxonomy to be scientifically progressive. I merely noted that according to logical empiricist ideals of reconstructed history of science, that has been the case in biology and physical chemistry. If the DSM expansion is a sign of scientific progress as Wakefield and numerous others have claimed, then the standards of progress they are using must be different from logical empiricist standards of progress. However, in that case, the burden of proof is on Wakefield and others who

make similar claims to articulate what model of progress they are invoking. Those who claim that the proliferation of labels observed in the successive editions of the modern DSMs constitute scientific progress need to specify what their standards for scientific progress are because, as I have shown, appeals to traditional models of scientific progress like logical empiricism cannot support their claims for the progressiveness of the modern DSMs. In fact, I believe that most who make such claims on behalf of the modern DSMs do indeed hold views of scientific progress similar to those articulated in detail in logical empiricism, and my analysis has shown that this is misguided and contains a serious conceptual contradiction. The claim that diagnostic label proliferation signifies progress simply cannot be supported by a logical empiricist analysis of progress as extracted from the history of biology and physical chemistry. Wakefield and others who have claimed the mantle of scientific progress for the modern DSMs and pointed to growth in the number of diagnostic labels over the past half century to support that claim will have to look elsewhere for a model of progress.

Despite a certain lack of interest in philosophy of science principles, Wakefield (1999a) has presented a redaction of Hempel (1965a) that attempted either on the one hand to recruit Hempel to the cause of the modern DSMs, or on the other hand, to reduce Hempel's position to the absurdity of championing psychoanalytic concepts. Interestingly in Part 1 of his current response, Wakefield (1999a) reproached Hempel for seeming in 1959 to promote psychoanalytic concepts, concepts that today have been widely rejected in mental disorder definitions. Ironically elsewhere, Wakefield (1998b) himself criticized the modern DSM conception of major depressive disorder for failing to incorporate psychoanalytic insights regarding subjective meanings of loss. Apparently, the standing of psychoanalytic concepts in Wakefield's analysis of mental disorders varies with the rhetorical context.

Regarding Wakefield's (1999a) treatment of Hempel, I encourage readers to compare Wakefield's quotations from Hempel with Hempel's (1965a) original text. According to Wakefield's redaction, Hempel would not be bothered about the current state of affairs in psychiatric classification because Hempel clearly recognized the fact that progress in taxonomy began with a pre-theoretical phase during which descriptive classification preceded classification based on theoretical reduction of categories. Whereas Hempel did recognize that historically mature scientific taxonomies had most likely begun with a descriptive phase, *he regarded this mere descriptive phase as one that signified a lack of scientific progress*. This was the whole point of Hempel's essay on taxonomy; mere description based on symptoms would not suffice for progress to occur. Further, if a discipline remained at the "natural history" stage, then its concepts and terms would fail to achieve the necessary systematic import for progress to occur. Wakefield (1999a) has taken Hempel's analysis as sanctioning a halt at the descriptive stage of taxonomic development, and he has praised the modern DSMs because they have remained at that descriptive stage rather than make the mistake of prematurely embracing some theory. Hempel never in fact addressed the issue of prematurely embracing some theory. Instead, he advocated the introduction of theories that met minimal standards of specification and testability, and he even believed in the late 1950s that biochemical and psychodynamic theories could compete equally for hegemony in the mental health field. The point is that Hempel did not regard the pre-theoretical and purely descriptive stage of taxonomy as anything other than a necessary starting point of ordinary thinking, a "natural history" stage. Hempel had borrowed the term "natural history" from Northrop (1947) who had formulated a hierarchical model of knowledge where observation, description, and classification

preceded deductively formulated theory. For Hempel, the whole point of science was to get beyond this "natural history" stage; something that he believed had been accomplished in biological taxonomy and physical chemistry (see Hempel, 1965b,c).

Because he placed such emphasis on the role of theory in taxonomy, Hempel (1965a) actually applauded the use of psychoanalytic concepts in the definition of some mental disorders in DSM-I. This was, of course, consistent with his belief that progress required that classification move beyond mere description and "natural history." Wakefield (1999a) offered the following assessment of Hempel's (1965a) analysis of psychiatric classification circa 1959.

> In retrospect, then, Hempel was utterly wrong in his claim that the DSM-I's use of theory-laden psychoanalytic definitions of categories that had formerly been understood descriptively represented scientific progress. From the perspective of his own account of the stages of development of a scientific discipline, the source of his error was that he grossly misestimated the stage of development of psychiatric theory and research as being more advanced than it was. We need not make the same mistake. Hempel suggests that, prior to there being demonstrated theories that can serve to organize a classification of disorders, a taxonomy must be based on descriptive, symptomatological properties. That is exactly what we need now and it is what the DSM offers (Wakefield, 1999a, p. 974).

According to Wakefield's redaction, Hempel made a mistake in 1959 by not recognizing that the field of psychopathology should have remained descriptive and atheoretical for the next 40 years. This is a red herring, a distraction. The point of Hempel's analysis was to highlight the role of theory in taxonomy because some theory was generally preferable to none at all, and yes, even "wrong" theory (Hempel recognized the possibility of several competing theories) was preferable to no theory at all. Instead of presenting Hempel's actual views, Wakefield has conjured an antithetical doppelganger, that turns out to be a champion of Wakefield's own current version of the DSM cause for an atheoretical taxonomy, a taxonomy that eschews theory and remains at the natural history stage of mere description. In effect, Wakefield has turned Hempel's views precisely upside down in order to recruit Hempel to Wakefield's own rhetorical purposes.

The fact that very few of the shapers of the modern DSMs have currently endorsed psychodynamic theory and the fact that much of that theory was expunged with the appearance of DSM-III (Bayer & Spitzer, 1985) are facts that may be only marginally relevant to assessing whether or not the expansion of the DSMs constitutes scientific progress. Obviously, were we to take as the standard of progress the considered opinion of some reference group of experts, then depending on the reference group, the fact of the disappearance of psychoanalytic theory from the modern DSMs might count as a sign of progress or of lack thereof. Whereas various constituencies may be pleased or displeased with the eventual expurgation of psychodynamic theory from the modern DSMs, such sentiments have little bearing on judgments of scientific progress within a logical empiricist framework. Within the logical empiricist framework, some theory is better than no theory (provided there some constraints on types of theories), and some theoretical connection between concepts and disorders is better than mere description. That was, after all, the whole point of the logical empiricist analysis. To claim that the modern DSMs are scientifically progressive because they got rid of psychoanalytic theory could only have persuasive force if one assumed the Whiggish historical prejudice that however things have turned out up to now, the current state

of affairs must be good and therefore progressive. In contrast, within a logical empiricist framework, a proliferation of taxonomic labels and a retreat from all theory signal a lack of progress, full stop and end of story. That was the whole point. We are perfectly free to think that DSM-III ushered in an era of progress when all theory was eliminated from official psychiatric nosology, but when we make such a claim, we are then appealing to some other notion or standard of progress than the one outlined by Hempel and other logical empiricists. For example, someone could applaud the elimination of psychodynamic theory from the modern DSMs because the theory failed to meet minimal standards for a viable theory (Grunbaum, 1985; Grunbaum & Holzman, 1993) or because the DSMs will eventually replace psychological theory with evolutionary theory. Those types of arguments would be consistent with the logical empiricist analysis, but arguments applauding the diminution of all theory would not be consistent.

Despite the fact that Wakefield's redaction of Hempel is far fetched, there was a substantive claim made by Wakefield, namely that the field of psychopathology is currently in a stage of development where classification by theory (etiology) is premature. Note however, that according to a Hempelian model of scientific progress, this would mean that the field of psychopathology has been going in circles. Supposedly, the field moved from some earlier state of classification by description before the first DSM (Grob, 1991). From this type of mere description, the field produced a more theoretical classification with the advent of the DSM (DSM-I) and continued theoretical classification with the second edition (DSM-II) (see for review Houts, in press). However, with the introduction of the modern DSMs (DSM-III), the field appears to have gone full circle and back again to mere description. One can only conclude that according to a Hempelian model of scientific progress, certainly no progress has occurred. Such a judgment is consistent with the judgment of the eminent historian of American psychiatry, Gerald Grob (1998) who has noted:

> The history of psychiatric thought over two centuries offers a striking example of a cyclical pattern that has alternated between enthusiastic optimism and fatalistic pessimism, between an emphasis on environment and one on heredity, and between somatic and psychogenic interpretation. Every generation, moreover, insisted that the specialty stood on the threshold of fundamental breakthroughs that would revolutionize the ways in which mental disorders were understood and treated (Grob, 1998, pp. 216–217).

Wakefield (1999a) would have us believe otherwise. His grounds, however, must be other than an appeal to logical empiricist models of scientific progress, because according to such a Hempelian model of progress, the modern DSMs are not only not advancing, they may in fact be going around in circles and therefore getting nowhere fast.

What other model of scientific progress can be invoked to support the claim that the expansion of diagnostic labels evidenced in the modern DSMs is a sign of scientific progress? Some have suggested the example of physical medicine (Guze, 1995; Klerman, 1986), even though Wakefield (1999a) has most recently stated that he did not intend to claim a direct parallel between psychiatric nosology and physical medicine nosology. Wakefield's actual position regarding the parallel status between physical medicine disorders and mental disorders has been very confusing because he has repeatedly drawn the analogy between the way that physical medicine has defined physical disorders and the way that DSMs have defined mental disorders. He has also alluded to the

possibility that the cases of physical medicine and mental disorders were alike in that neither one could be subsumed under a Hempelian model of progress as follows:

> Unlike other areas of scientific inquiry, the study of disorders is not concerned with how a domain is naturally ordered by parsimonious laws, as in physics, nor with parsimonious functional explanations of the existence and structure of various mechanisms, as in biology. Rather, taking a functional understanding of the organism as a baseline, medicine studies how things go wrong with such functioning. Functional failures tend to be intrinsically diverse, especially in complex systems, such as the mind, that possess many linked types and levels of mechanisms that interact in unpredictable ways. In such a domain, a parsimonious theory of breakdowns is unlikely (Wakefield, 1998a, p. 847).

In effect, Wakefield (1998a) previously proposed to exempt taxonomies of disorders from evaluation according to criteria proposed by Hempel, but in contradiction to that position, his most recent critique published here offered a misreading of Hempel according to which such taxonomies were after all evaluated by "Hempelian" criteria. Regardless of different uses to which Wakefield has put the comparability claim, the claim that mental disorder taxonomies are just like medical disorder taxonomies has figured strongly in assessing the scientific progress of the modern DSMs. The comparability claim deserves careful analysis.

The idea that physical medicine has advanced scientifically is intuitively appealing and seems almost irrefutable on the face of it (Porter, 1997). The way that proponents and defenders of the modern DSMs have used this claim is to say that what is happening in the mental health field is just like what happened in the physical health field some time ago. The fundamental claim is that the mental health field is now going through what the physical medicine field went through several decades ago. According to this analogy, the mental health field is a branch of medicine that is in a nascent stage of development, and in the future, the mental health field will judged to be just as progressive as physical medicine is judged today. As the argument goes, what is required for the mental health field to reach that stage of higher scientific development is to stay the current course, for psychiatry to follow in the footsteps of internal medicine, cardiology, gastroenterology, and the other medical specialties. This is the standard rhetorical appeal of the advocates and authors of the modern DSMs, those whom Blashfield (1984), following Klerman, has labeled the neo-Kraepelinians.

This claim that the development of the modern DSMs is following in the footsteps of the development of physical medicine has taken many forms and has been put to many uses in both recent and older debates about how to define mental disorders in a scientific and objective way that avoids intrusion of social and cultural values. I have considered many more of those uses and the contexts for such appeals to physical medicine in Part 2 of this current response to Wakefield. For the immediate task of assessing whether or not the diagnostic label proliferation of the modern DSMs constitutes scientific progress, I have focused on a particular claim introduced by Wakefield (1998a) who credited Spitzer for the idea.

## 3. How DSM expansion differs from ICD growth

Wakefield (1998a) asserted that a comparison of DSM increases in diagnoses for mental disorders from DSM-II through DSM-IV demonstrated that increases of mental disorder diagnoses were comparable to increases in diagnostic categories for circulatory and digestive diseases in the International Classification of Diseases (ICD) from the 8th edition of the ICD (ICD-8; World Health Organization [WHO], 1967) through the 10th edition of the ICD (ICD-10; WHO, 1992). Wakefield stated:

> Thus, during this time period, which covers the claimed expansive period from the advent of the third edition of the DSM (DSM-III; American Psychiatric Association, 1980) through the DSM-IV, the rate of growth in the DSM categories was comparable with or lower than the rates of growth in the other two specialties [circulatory and digestive diseases] (Wakefield, 1998a, p. 848).

Wakefield credited Spitzer, a chief architect of the modern DSMs, for pointing out this similarity to him. In response, I pointed out that ICD increases in disease labels were neither quantitatively nor qualitatively the same as increases in DSM mental disorder labels (Houts & Follette, 1998). I had considered this so obvious upon cursory inspection of the ICDs that I regarded my claim about the non-comparability of DSM and ICD changes to be altogether uncontroversial. Nevertheless, in his present extended critique, Wakefield (1999a) has again asserted that increases among mental disorder labels in the DSMs have been comparable to increases among codeable diagnoses of diseases in the ICDs over an equivalent historical period.

In what follows, I have first addressed in considerable detail the empirical claim about comparability between ICD changes and the DSM changes. In other words, does the evidence support Wakefield's claim that changes, which have occurred within the DSMs and ICDs during the period 1967–1994, are of the same magnitude and type? After first settling the empirical and interpretive issue, I have then addressed issues related to the various contexts in which the comparability claim has been used. That is to say, why should we care if the DSM changes either are or are not comparable to ICD changes. What is the probative value of examining the ICD changes with regard to illuminating the recent and consistent expansions of the DSM?

## 4. Quantitative analysis of ICD changes

Table 1 shows the number of unique disease codes for the circulatory and digestive system sections respectively of ICD-8 and ICD-10. Without attending to details, Wakefield (1998a, 1999a) has repeatedly stated that diseases of the circulatory system increased by 200%, and those of the digestive system increased by 123% from ICD-8 to ICD-10. Table 1 clarifies the nature of the *so-called increase* in diagnoses within the ICDs. Wakefield apparently obtained the 200% increase in circulatory system diagnoses by subtracting the number (126) of diagnostic numerical codes in ICD-8 from the number (384) of diagnostic numerical codes for these diseases in ICD-10 and dividing the resultant number (258) by the number (126) of diagnostic numerical codes

Table 1
Number of diseases of the circulatory and digestive systems from ICD-8 and ICD-10

| | Circulatory system | | Digestive system | |
|---|---|---|---|---|
| | *n* | % of total | *n* | % of total |
| Unique ICD-8 disease codes | 126 | | 172 | |
| Unique ICD-10 disease codes | 384 | | 378 | |
| ICD-10 disease codes that *never before appeared* in ICD-8 | 102 | 27 | 73 | 19 |
| ICD-10 disease codes that *previously appeared* in ICD-8 | 282 | 73 | 305 | 81 |

in ICD-8. Wakefield similarly obtained a so-called increase in diagnostic labels for digestive system diseases within the ICDs.

This procedure and the resultant so-called percentage increases are completely misleading for two reasons. First, most of the changes between ICD-8 and ICD-10 comprised the assignment of new numerical codes in ICD-10 for diagnoses that were already listed and named in ICD-8. In other words, there was not a substantial increase in new diagnoses, only a re-numbering of already existing diagnoses. Second, when one compares in detail this type of change to the type of change that occurred in the expansion of DSM diagnoses, there is just no comparability. From a purely descriptive standpoint, there is just no analogy between the changes in the ICDs and the diagnostic label proliferation of the DSMs. Changes in the two diagnostic systems are qualitatively very different and therefore not at all analogous.

Considering changes in the circulatory system sections of ICD-8 and ICD-10, only 27% of the ICD-10 diagnoses failed to appear previously in ICD-8. The new diagnoses that did appear were often ones due to advances in medical treatment and technology. For example, a number of new diagnoses were introduced to describe events following myocardial infarction. This was probably due to the fact that more people survived heart attacks in 1992 than in 1967. Also, in 1992 as compared to 1967, these patients probably underwent more sensitive testing which resulted in locating specific weaknesses in the hearts of survivors. Advances in medical treatment and in diagnostic instrumentation and resolution produced the conditions for new physical maladies to be observed and to be named. Similarly, new diagnoses appeared in the section on cerebral infarction, no doubt due to technological advances in imaging the location of cerebral vascular accidents. Interestingly, in the circulatory system sections of ICD-8 and ICD-10, 73% of the new diagnostic codes were comprised of conditions already recognized as diseases in ICD-8, and these were merely given unique numerical codes in ICD-10. I have presented below a detailed example to illustrate qualitatively what happened for over 70% of the differences between ICD-8 and ICD-10. Clearly, the majority of so-called “new” diagnostic codes in the circulatory system section of the ICDs to which Wakefield has referred are not new at all. Wakefield’s (1998a, 1999a) procedure for enumerating and evaluating the changes in ICD classification of circulatory system diseases has led to serious error, and the better method I have illustrated above can be applied to the ICD sections on the digestive system.

Using the aforementioned better method for analyzing ICD changes, a comparable pattern was

replicated for changes in diseases of the digestive system between ICD-8 and ICD-10. Only 19% of the ICD-10 codes in this section represented new diseases that had not been previously named and listed in ICD-8. In most cases, the introductions of new diagnoses for digestive disorders were due to new etiologies being discovered, new treatments being developed, and new diagnostic technologies being introduced. For example, Crohn's disease and diverticular disease, both of which were listed in ICD-8, were expanded to provide specific numerical codes for different parts of the digestive tract. Enhanced diagnostic procedures resulted in greater specification of a known disease. Alcohol induced chronic pancreatitis was added to diseases of the pancreas in ICD-10. However, just as was the case with diseases of the circulatory system, fully 81% of the so-called "new" diagnoses of the digestive system were not new at all and had been named and listed as separate diseases under larger aggregate numerical codes in ICD-8.

During the period 1967–1992, changes in the ICDs were due primarily to the re-numbering of old diagnostic labels rather than to the introduction of entirely new diseases and conditions. When truly new labels and disease conditions were introduced, they were mostly attributable to technological advances in medical assessment and treatment and occasionally to the discovery of new conditions and diseases defined by etiology. The nature of this change in the ICDs should be contrasted with the changes that have occurred from DSM-II to DSM-IV over a comparable period. The best way to illustrate and highlight such drastic differences between ICD change and DSM expansion is to consider in detail the qualitative differences that can be demonstrated by examining two concrete examples of label change, one from the ICDs and one from the modern DSMs.

## 5. Qualitative comparison of ICD change and DSM expansion

As I noted, many of the disorders that appeared to be separately codeable in ICD-10 had been listed under a single code in ICD-8. Table 2 shows a typical example for a section that describes diagnoses for ischemic heart disease. ICD-10 provided for nine numerical codes in this section, whereas ICD-8 provided for only one code.

All nine of the ICD-10 codes had appeared by exact name or equivalent in the ICD-8. Not a single new disease label was produced even though a single numerical code in ICD-8 was transformed into nine separate numerical codes in ICD-10. Following the logic of Wakefield's (1998a, 1999a) procedure for enumerating and evaluating the changes of diagnoses in the ICDs, one would calculate that for this specific case, there would have been an 800% increase in the diagnostic labels for ischemic heart disease during the period 1967–1992. Such a conclusion would be patently false. Seven of the nine diagnoses in ICD-10 appeared verbatim in the same section in ICD-8 (section 412), and the other two had previously appeared in the ICD-8 under different sections (sections 410 and 414). *Whereas Wakefield's procedure would have resulted in the false conclusion of an 800% increase in diagnoses in the ICDs, the actual increase in diagnoses for ischemic heart disease between ICD-8 and ICD-10 was zero*! No new disease conditions were identified, and no new diagnostic labels were introduced in this case from 1967 to 1992. Something altogether different happened in the DSMs from 1968 to 1994.

The changes in medical diagnosis displayed in Table 2 should be contrasted with the changes in psychiatric diagnosis displayed in Table 3. Table 3 shows the listing of mental disorders in

Table 2
Changes in chronic ischaemic heart disease category from ICD-8 to ICD-10[a]

| ICD-8 (1967) | ICD-10 (1992) |
|---|---|
| 412 Chronic ischaemic heart disease | I25 Chronic ischemic heart disease |
| Aneurysm of heart | Excludes: cardiovascular disease NOS (I51.6) |
| Arteriosclerotic heart (disease) | I25.0 *Atherosclerotic cardiovascular disease, so described* |
| Cardiovascular: | I25.1 *Atherosclerotic heart disease* |
| arteriosclerosis | Coronary (artery): |
| degeneration | • atheroma |
| disease | • atherosclerosis |
| sclerosis | • disease |
| | • sclerosis |
| Coronary artery: | I25.2 Old myocardial infarction |
| arteriosclerosis | *Healed myocardial infarction* |
| atheroma | Past myocardial infarction diagnosed by ECG or other special investigation, but currently presenting no symptoms |
| disease | |
| sclerosis | |
| stricture | |
| Healed myocardial infarct | I25.3 *Aneurysm of heart* |
| | Aneurysm: |
| | • mural • ventricular |
| Ischaemic: | I25.4 Coronary artery aneurysm (listed under 410 in ICD-8) |
| degeneraton: | Coronary ateriouvenous fistula, acquired |
| heart | Excludes: congenital coronary (artery) aneurysm (Q24.5) |
| myocardium | |
| heart disease | |
| Post-myocardial infarct syndrome | I25.5 Ischaemic cardiomyopathy |
| Any condition in 410 (acute myocardial farction) specified as chronic or with a stable duration of over 8 weeks | I25.6 Silent myocardial ischaemia (listed under 414 in ICD-8) |
| | I25.8 *Other forms of chronic ischaemic heart disease* |
| | Any condition I21-I22 and I24 — specified as chronic or with a stated duration of more than 4 weeks (more than 28 days) from onset |
| | I25.9 *Chronic ischaemic heart disease, unspecified* |
| | Ischaemic heart disease (chronic) NOS |

[a] Note. Diagnoses italicized in ICD-10 column are those given a new and separate numerical code in ICD-10 even though they appear in ICD-8. under the single numerical code of 412.

DSM-II and DSM-IV for sleep disorders. In DSM-II, there was only one listing for sleep problems (306.4 Disorder of sleep), and this occurred in Section VII, which was devoted to Special Symptoms. That section of DSM-II stated:

> This category is for the occasional patient whose psychopathology is manifested by discrete, specific symptoms. An example might be anorexia nervosa under *Feeding disturbance* as listed

Table 3
Changes in sleep disorders category from DSM-II to DSM-IV

| DSM-II (1968) | DSM-IV (1994) |
|---|---|
| 306 Special symptoms not elsewhere classified | Primary sleep disorders |
| | Dyssomnias |
| 306.3 Disorder of sleep | 307.42 Primary insomnia |
| | 307.44 Primary hypersomnia |
| | 347 Narcolepsy |
| | 780.59 Breathing-related sleep disorder |
| | 307.45 Circadian rhythm sleep disorder |
| | Delayed sleep phase type, jet lag type, Shift Work Type, Unspecified Type |
| | 307.47 Dyssomnia NOS |
| | Parasomnias |
| | 307.47 Nightmare disorder |
| | 307.46 Sleep terror disorder |
| | 307.46 Sleepwalking disorder |
| | Sleep disorders related to another mental disorder |
| | 307.42 Insomnia related to … indicate axis I or II disorder |
| | 307.44 Hypersomnia related to … indicate axis I or II disorder |
| | Other sleep disorders |
| | 780.xx Sleep disorder due to … indicate the general medical condition |
| | .52 Insomnia type, .54 Hypersomnia type |
| | .59 Parasomnia type, .59 Mixed type |
| | _._ Substance-induced sleep disorder refer to substance-related disorders for substance-specific codes |
| | Specify type: Insomnia type, Hypersomnia type, Parasomnia type, Mixed type |
| | Specify if: with onset during intoxication, with onset during withdrawal |

below. It does not apply, however, if the symptom is the result of an organic illness or defect or other mental disorder. For example, anorexia nervosa due to schizophrenia would not be included here (APA, 1968, pp. 47–48).

In DSM-II, sleep problems were not mental disorders in and of themselves, except on very rare occasions. Instead, sleep problems were conceptualized as due to physical conditions such as medical illness, or occasionally they were side effects of mental disorders. Such a conceptualization of sleep problems served to reduce the number of sleep problems that could be considered mental disorders. In fact and in retrospect, the adherence of the first two editions of the DSM to psychoanalytic theory actually constrained the number of mental disorders that could be separately listed (Houts, in press). Contrast that older conceptualization of sleep problems with the proliferation of mental disorder diagnoses devoted to sleep problems in DSM-IV, which has been reproduced in the far right column of Table 3.

DSM-IV continued the expansion of DSM-III-R where sleep disorders were first introduced as a new major category of mental disorders to be coded on Axis I. It is important to note that DSM-III did not contain a separate section devoted specifically to sleep disorders. DSM-III listed only Sleepwalking Disorder and Sleep Terror Disorder within the section on childhood disorders. Moreover, DSM-III contained the following explanatory note regarding the listing of only two sleep disorders:

> Of the many disorders of sleep, DSM-III includes only these two because of their marked behavioral manifestations, because of the frequency with which they come to the attention of a mental health professional, and because by tradition, they are thought of as mental disorders (…). (A new classification of Sleep and Arousal Disorders appears in Appendix E.) (APA, 1980, p. 383).

Here was an explicit statement that whereas there were other kinds of sleep problems, in DSM-III as in DSM-II, these other sleep problems were not considered mental disorders and were therefore not given the status of separate diagnoses on Axis I. In fact, the authors of DSM-III reproduced as an appendix the 1979 version of the classification of sleep disorders previously published by the Sleep Disorders Classification Committee of the Association of Sleep Disorder Centers (ASDC, 1979). This approach seen in DSM-II and DSM-III should be contrasted with the approach of DSM-IV.

If one considers all the permutations of the 14 types of sleep disorder listed in DSM-IV and displayed in Table 3, the codeable number of distinct mental disorder diagnoses for sleep problems increased into the hundreds. None of these appeared in DSM-II. This marked proliferation of mental disorders from DSM-II to DSM-IV was therefore very different from the mere coding changes that occurred within the ICDs from ICD-8 to ICD-10. What could account for this increase of mental disorders under the heading of sleep disorders?

## 6. The manufacture of mental disorders and the case of sleep disorders

Wakefield (1999a) has asserted that the increase in mental disorders seen over sequential editions of the modern DSMs has been the same phenomenon as the *so-called increase* in medical diagnoses observed over sequential editions of the ICDs. He has stated:

> In fact, a cursory examination of the changes from the DSM-II to the DSM-IV reveals that a widening of the scope of disorder *cannot* be a major cause of the increase in categories. Mostly, the increase is due — exactly as in the physical medicine specialties — to making finer distinctions among conditions already considered disorders …. The DSM-II's one category for "disorders of sleep" is replaced by 15 (sic) DSM-IV categories, from primary insomnia to breathing-related sleep difficulties, all of which would have been diagnosed as DSM-II disorders of sleep (Wakefield, 1999a, p. 980).

Wakefield's general point has been that expansion of the modern DSMs is qualitatively the same as what happened with changes in the ICDs, and he attempted to illustrate that claim by looking

backward from the DSM-IV and reading into DSM-II those disorders found only in DSM-IV. The claim was made quite explicitly despite the fact that no references were cited to back up the general claim or the specific cases mentioned, such sleep disorders. As Wakefield stated:

> Unlike the ICD categories noted by Houts and Follette (1998), most of the new DSM categories were not explicitly mentioned as uncoded variations in the DSM-II, although some were passingly referred to in the text (e.g., four new DSM-IV categories of dissociative disorder were mentioned in DSM-II's description of its category of hysterical neurosis, dissociative type). However, this is a matter of style, not substance. In the vast majority of cases, either the textbooks at the time of DSM-II clearly indicated that the undifferentiated conditions were considered to fall under the coded categories, or we can confidently infer that a condition unrecognized at the time would have been so categorized. The point is that, just as in Follette and Houts's explanation of the changes in ICD, the new DSM categories did not represent an expansion of the domain of disorder but rather a refinement of diagnostic distinctions among disorders (Wakefield, 1999a, p. 981).

> the above analysis shows that the recent increases in number of diagnostic categories in both psychiatry and other medical specialties do largely represent the same phenomenon of classificatory refinement of known disorders (Wakefield, 1999a, p. 982).

Is this actually true? Is this historically accurate and supported by evidence? If we examine textbooks at the time before DSM-III-R, will we find that the sleep disorders introduced with DSM-III-R and elaborated in DSM-IV were in fact recognized as mental disorders? Will such an examination of the evidence lead to the conclusion that we can confidently infer that such hypothetical clinicians at the time of DSM-II would have recognized DSM-IV sleep disorders as mental disorders and also as undifferentiated instances of the global DSM-II diagnostic category of Disorder of Sleep?

The aforementioned quote from DSM-III regarding the limits of when sleep disorders might be considered mental disorders clearly contradicts Wakefield's (1999a) claims. In DSM-III, there was a very clear statement that most sleep disorders were not considered mental disorders and therefore were not included in DSM-III. In addition, I examined textbooks from the period surrounding DSM-II in the late 1960s and compared them as they progressed through various editions of the modern DSMs. For example in their text on diagnosis and drug treatment, Klein and Davis (1969) made no mention of sleep disorders. In his textbook on the use of psychiatry in general medical practice, Hofling (1975) also made no mention of sleep disorders. In a textbook on interpersonal approaches, Chapman (1975) noted that a category for sleep disorders was contained in DSM-II, but this text contained no other mention of sleep disorders and did not elaborate any further. In the 9th edition of his textbook, Kolb (1977) listed a classification of sleep disorders taken from a source other than DSM-II, but it was quite unlike what subsequently appeared in DSM-IV. In the period surrounding DSM-II, there was little or no mention of sleep disorders, and this was consistent with their relative lack of importance in DSM-I and DSM-II. In fact, based on the textbooks at that time, we have no reason whatsoever to believe that sleep problems that are today recognized to be sleep disorders would have been recognized as mental disorders and classified under the DSM-II diagnosis of Disorder of Sleep.

An examination of sequential editions of the *Comprehensive Textbook of Psychiatry* provided additional evidence that sleep disorders within the modern DSMs went from being regarded as problems mostly unrelated to mental disorders to being mental disorders per se. In the first edition, which was published at a time when DSM-I was in effect and just prior to the publication of DSM-II, references to sleep and to sleep problems occurred in several contexts. In a chapter on sleep and dreams, Dement (1967) noted that sleep problems could be indications of "psychic disturbance," but he regarded the primary dimension of classification to be not mental disorder but whether or not the behavior (insomnia, sleepwalking and talking, nightmares, narcolepsy) occurred during rapid eye movement sleep or some other stage of sleep. In a chapter on psychoneurotic disorders and consistent with psychoanalytic thinking, Lieff (1967) presented sleep paralysis, sleep walking and talking, enuresis, sleep drunkeness, and sleep hallucinations as dissociative phenomena. Similarly, Linn (1967) noted that various sleep problems could be manifestations of psychiatric disorders. Importantly, none of the aforementioned authors regarded sleep problems to be mental disorders in and of themselves. Instead, sleep problems were regarded either as unexplained medical problems or as secondary effects of primary mental disorders. This conceptualization of sleep problems at a time just prior to the appearance of DSM-II was extremely different from the conceptualization of sleep problems as mental disorders seen in DSM-IV.

In the second edition of the *Comprehensive Textbook of Psychiatry*, which was published when DSM-II was in effect and prior to DSM-III, Kales and colleagues noted that sleep disorders occurred most often among children, and they listed the following as sleep disorders: somnambulism, night terrors and nightmares, enuresis, narcolepsy, hypersomnia, and sleep apnea (Kales, Kales & Humphrey, 1975). Once again, these authors recognized that various sleep disturbances could be part of the normal course of other psychiatric disorders, but they did not regard as psychiatric disorders conditions classified separately as sleep disorders. Interestingly in that same edition of the *Comprehensive Textbook of Psychiatry*, Spitzer and Wilson (1975) contributed a chapter on diagnostic classification in which they noted that DSM-II had replaced the DSM-I diagnosis of somnambulism with the more generic diagnosis of other disorders of sleep. They described the general category as: "for a small list of symptoms occurring in the absence of any other mental disorder. Most of the symptoms are more likely to be seen in children than in adults" (Spitzer & Wilson, 1975, p. 843). In other words in 1975 and prior to DSM-III, a chief architect of the modern DSMs, Robert Spitzer himself regarded sleep disorders to be mental disorders only for rare instances that applied mostly to children. How therefore could we possibly be confident that some hypothetical clinician from this period would have classified today's DSM-IV sleep disorders as mental disorders and as variations of the DSM-II diagnostic category of Disorder of Sleep? Wakefield's claims cannot be supported by the textbooks of the time, and even Spitzer himself did not behave as Wakefield's imaginary clinician circa 1975 was alleged to have behaved.

With the third and fourth editions of the *Comprehensive Textbook of Psychiatry*, sleep disorders were presented surrounding the appearance of DSM-III. The third edition of the *Comprehensive Textbook of Psychiatry* was published the same year as DSM-III and contained a chapter on sleep disorders by Hartmann (1980). No mention was made of DSM-III or of DSM-II. Instead, sleep disorders were conceptualized as medical problems of unknown etiology and otherwise as secondary problems of mental disorders. In other words, sleep disorders were not mental disorders per se. In 1980 and echoing a Hempelian model of scientific progress, Hartmann noted that sleep disorder classification would need to move more toward classification by pathophysiology and away from mere description.

> At present workers in the field agree about the diagnosis and the treatment of a number of specific syndromes and illnesses, but they are not yet in complete agreement as to the overall classification of the sleep disorders. As knowledge of the chemistry and the physiology of sleep increases, it will eventually be possible to classify the sleep disorders logically in terms of their basic chemical and neural pathology, and they will form logically related groupings (Hartmann, 1980, p. 2015).

The fourth edition of the *Comprehensive Textbook of Psychiatry* was published after the appearance of DSM-III but before DSM-III-R and therefore before the appearance of a separate section for sleep disorders in the modern DSMs. This fourth edition contained a chapter on sleep disorders in which the author, again Hartmann (1985), warned of the dangers of mental health professionals treating sleep problems with simplistic applications of either sleeping pills or psychotherapy. Signaling a clear break with the idea of classifying sleep disorders as mental disorders, Hartmann noted:

> Despite the difficulties in classification, the field is obviously moving in the direction of a medical-model delineation of separate disorders. Sleep disorders medicine, whether the world is ready for it or not, is moving in the direction of becoming a medical field in its own right, bearing some relationship not only to psychiatry but to neurology, pulmonary medicine, cardiovascular medicine, and others (Hartmann, 1985, p. 1247).

Hartman presented the Association for Sleep Disorders Centers (ASDC, 1979) diagnostic system and noted in passing that the nomenclature had been reproduced as an appendix in DSM-III. For Hartmann (1985), sleep disorders were clearly conceptualized as disorders in their own right and only occasionally related to mental disorders.

The fifth edition of the *Comprehensive Textbook of Psychiatry* appeared in 1989 after publication of DSM-III-R where the modern DSMs devoted an entirely new section to sleep disorders for the first time. In that edition of the *Comprehensive Textbook of Psychiatry*, a chapter on sleep disorders warned against the dangers of relying on the DSM-III-R classification of sleep disorders.

> DSM-III-R does not attempt to be a complete nosology of the sleep disorders and neglects polysomnographic criteria for diagnosis. The danger of this approach is that it may confuse or give a false sense of assurance to psychiatrists who are making a diagnosis by patient history alone (Karacan, Williams & Moore, 1989, p. 1131).

The authors advocated use of the ASDC nomenclature instead of DSM-III-R. Finally, in the latest and sixth edition of the *Comprehensive Textbook of Psychiatry*, which was published after DSM-IV, the authors of the sleep disorders chapter specifically cautioned against using DSM-III-R and DSM-IV diagnoses for sleep disorders. They stated:

> The fourth edition of DSM (DSM-IV) represents an improvement; however its adequacy and practicality remain to be tested. Initial field trials comparing DSM-IV and the International Classification of Sleep Disorders (ICSD) found moderate agreement overall for the differential

diagnosis and coding of patients presenting with insomnia (Williams, Karacan, Moore & Hirshowitz, 1995, p. 1374).

These authors clearly advocated for the newly revised ACSD diagnostic system, which had been adopted by the World Health Organization and incorporated into ICD-9. The new classification system was published by the American Sleep Disorders Association in 1990 (ASDA, 1990), prior to DSM-IV.

Even after the publication of DSM-III-R and DSM-IV where sleep disorders were incorporated into official psychiatric nomenclature as mental disorders, those medical and other professionals who specialized in sleep problems rejected the concept that sleep disorders were to any significant degree primarily mental disorders in and of themselves. They also rejected the DSM-III-R and DSM-IV classification of sleep disorders because they were inadequate and led to mistaken notions about the role of mental disorders in sleep problems. Interestingly, the one field trial to assess DSM-IV classification of sleep disorders was able to obtain a sufficient sample for measuring reliability for only four diagnoses, and the median kappa for first diagnoses was only 0.38 (Buysse et al., 1994). To be sure, the classification of sleep problems has been a complicated matter and one that has remained controversial between sleep disorder specialists and their psychiatric rivals. For example, there has been considerable controversy over the most frequent sleep complaint, insomnia. That controversy has concerned the extent to which insomnia was a problem unrelated to mental disorders vs the extent to which insomnia was a symptom or consequence of some mental disorder, and such controversy lies at the heart of the expansion of mental disorder diagnoses in the area of sleep problems.

As we have demonstrated with the foregoing detailed analysis of the historical case of sleep problems, Wakefield's (1999a) claim that there has been continuity and gradual elaboration of sleep disorders as mental disorders, his analogy to the ICD changes for medical conditions, is completely false. There was little or no continuity, nor was there some gradual detailing and exposition of categories with finer and finer discrimination in the classification of sleep disorders as Wakefield had claimed there was. The history has simply illustrated a different story. In fact, before DSM-III-R, sleep disorders were generally not regarded to be mental disorders. The declaration that sleep disorders were mental disorders occurred with DSM-III-R and was then further elaborated in DSM-IV. This expansion has never been explained in the DSMs. One searches in vain for the breakthrough research which demonstrated that finally after over 40 years, these sleep problems were in fact mental disorders. It is as though sleep problems became mental disorders overnight sometime in 1987. If as we have shown that Wakefield's version of the history of DSM expansion is not correct, then what did happen? How did we end up with so many new mental disorders suddenly overnight?

The answer to this question would take much more space than is currently available and would take us into a long digression away from assessing the central claim that Wakefield has made regarding comparability between DSM expansion and ICD changes. A short answer to the question is that organized psychiatry in the form of the APA Work Group to Revise DSM-III voted to declare that the sleep disorders that had formerly been presented as an Appendix in DSM-III were promoted to a new section of mental disorders for Axis I in DSM-III-R. Voting, not theory, not research made sleep disorders into mental disorders sometime between 1985 and 1987 (APA 1985, 1987). This familiar procedure for moving disorders from speculative status into full

inclusion without any clear standards has been noted by critics of the modern DSMs (Blashfield, Sprock & Fuller, 1990). DSM-IV has classified as separate mental disorders those sleep problems due to medical conditions, those due to other psychiatric conditions, and those due to substance use. This is analogous to claiming that fever due to measles is a separate disorder from fever due to typhoid. Fever is a symptom of both diseases and not itself a separate disease. Leaving aside such obvious multiplication of mental disorders, what remained was the DSM-IV sleep disorders listed under the heading *Primary Sleep Disorders*. What made these problems mental disorders in 1994 (DSM-IV) when they were not in 1968 (DSM-II) or 1980 (DSM-III) for that matter? DSM-IV stated: "Primary Sleep Disorders are those in which none of the etiologies listed below (i.e., another mental disorder, a general medical condition, or a substance) is responsible. Primary Sleep Disorders are presumed to arise from endogenous abnormalities in sleep–wake generating or timing mechanisms, often complicated by conditioning factors" (APA, 1994, p. 551). In other words, what made these problems mental disorders was their *unknown etiology*. That unknown etiology was hypothesized to be an internal disturbance of physical controls of sleeping and waking, and those controls may have been influenced by the history of the person's interactions with the environment.

What has been made clear from the aforementioned presentation regarding sleep disorders as mental disorders is that what are currently regarded as sleep disorders in the form of mental disorders would never have been recognized as mental disorders several decades ago. The case of sleep disorders has illustrated very clearly that there is considerable discontinuity in the classification of mental disorders from DSM-I to DSM-IV. Further, that kind of discontinuity was not found in the ICDs over the equivalent period.

## 7. On the contexts for comparing DSM expansion with ICD changes

Wakefield's (1998a, 1999a) claim that there was some analogy between DSM expansion and the changes in the ICDs over comparable historical periods has been shown to be patently false. An empirical comparison of DSM expansion with ICD change has clearly demonstrated that the magnitude of diagnostic label proliferation for mental disorders observed in subsequent editions of the modern DSMs is far greater and of a completely different type than the so-called increase of diagnostic labels for physical disorders observed in the ICDs for a comparable period of time. Having settled the empirical matter, it is also important to review the rhetorical uses for this false claim that subsequent editions of the DSM have been accruing new diagnoses just as the ICDs have done.

Wakefield (1999a) has corrected me regarding his use of the comparability claim. Whereas I had understood him to be using the comparability claim to bolster a position that the modern DSMs are merely following in the footsteps of physical medicine and are therefore progressive, he has noted that he was using the comparability claim to falsify the Hempelian idea that scientific nomenclatures progress when the taxonomic labels have been reduced in number rather than when they have increased in number. In other words according to Wakefield (1999a), the Hempelian model could not possibly be correct because physical medicine, as a discipline that has demonstrated scientific progress, has also increased its number of disease labels and therefore presents a clear counter example to the general claim of progress by taxonomic label reduction. Examining

the comparability claim in this context raises several questions. What in fact has gone on in physical medicine? Are the modest additions of disease labels observed in the ICDs and exemplified in the aforementioned discussion and analysis clear counter examples of a Hempelian model of scientific progress? What do we mean when we say that medicine has progressed scientifically, and by what means has that progress been achieved?

What has happened in the history of physical medicine is rather complicated and subject to different levels of analysis than is revealed by a mere inspection of ICD labels from ICD-8 to ICD-10. I contend that the application of a Hempelian model to the case of judging scientific progress in the label proliferation within the DSMs is instructive. In contrast, applying the same model to the ICDs may be less instructive exactly because what has occurred in the development of physical medicine has been quite different from what has occurred in the mental health field. That is to say, despite their best efforts to don the white coat of scientific progress in physical medicine and their repeated claims merely to be following in the footsteps of their physical medicine colleagues, the Neo Kraepelinian leaders of the modern DSMs have engaged in practices of diagnostic expansion that are quite different from those seen in physical medicine.

A first thing to note is that unlike the modern DSMs, the official classification of diseases, the ICDs, have added relatively few actual new diagnoses of pathological conditions in the period 1967 to 1992. When new disease entities have been named, they have most frequently been modifications of existing diagnoses that specified some more refined anatomical location, and this has been due to refinements in diagnostic assessment technology. As was noted, the availability of the endoscope made possible the refinement of certain intestinal disorders just as technological advances in cardiology led to the identification of conditions that would otherwise have gone unrecognized because people did not survive as long in previous decades. In physical medicine, one can see a technologically driven process where new refinements of existing disease conditions were added because of greater acuity in the instrumentation used to conduct physiological and functional examination. The parallel process very rarely occurred in the modern DSMs, and by contrast, the modern DSMs added numerous new mental disorders without any input from some refinement in diagnostic technical acuity (e.g., Frotteurism, Identity Problem, Intermittent Explosive Disorder, Kleptomania, Mathematics Disorder, Nightmare Disorder, Pathological Gambling, Sexual Disorder NOS, Voyeurism, and Unspecified Mental Disorder [nonpsychotic]). As the previous analytic comparison between the ICD sections on chronic ischemic heart disease and the DSM sections on sleep disorder illustrated, the quantity and quality of diagnostic label change in physical medicine is very different from the expansion of the DSMs.

One may still ask as Wakefield has implied, if a Hempelian model of scientific progress is applicable for disciplines other than biology and physical chemistry and such a model can be applied to the modern DSMs, where does the Hempelian model apply to physical medicine and the ICDs? The answer to this challenge is rather complicated and requires both an historical perspective and analysis at a somewhat different level than the level of physical disease labeling exemplified by the ICDs. From an historical point of view that takes into account the long history of physical medicine preceding the ICDs, there is a sense in which modern disease nomenclature reflected in the ICDs has in fact produced a theoretical reduction of disease labels. For example, the discovery of infectious microbes and viruses greatly reduced the domain of illnesses classified by symptom topography alone. The clinical science of medicine progressed from a classification of physical symptoms to a classification of etiologies because of advances in basic research in

microbiology. Research in such basic sciences as endocrinology, immunology, and molecular genetics extended the concept of disease as deviation from physiological norms and accounted for numerous diseases that were classified as failures of normal physiological functioning. Although the etiology of many of these latter conditions remains unclear, there are objective physiological reference points for defining such conditions as hypertension. Now, there are numerous different types of cancer classified according to cell type, for example. However, we can anticipate that such classification will recede into the background in terms of importance once the more specific genetic mechanism abnormalities that produce each type of cell have been discovered. Cancers will eventually be classified by etiological process rather than physical topographical appearance. That is the kind of theoretical reduction that is expected under a Hempelian model of scientific progress in taxonomy. Physical medicine has shown signs of reduction in disease labels that are consistent with a Hempelian model of progress in taxonomy, but these signs have been distinctly more subtle than can be detected directly by examination of changes within the limited window from ICD-8 to ICD-10.

Physical medicine has followed a different path of development than has official mental health diagnosis. The modern DSMs have introduced labels for mental disorders on the basis of committee voting rather than on the basis of known etiology or objectively defined failures of functioning. Wakefield (1999a) has argued that the modern DSMs have been scientifically progressive because they have approached the issue of mental disorders as objectively defined behavioral signs that may be attributed to some plausible dysfunction. Wakefield's concept of "plausible dysfunction" needs to be contrasted with physical medicine's observed dysfunctions in order to highlight yet another reason why the modern DSMs cannot be clothed in the white coat of progress associated with physical medicine.

## 8. Circularity of plausible dysfunctions

Wakefield (1999a) has reaffirmed that his harmful dysfunction formulation of the basic concept of mental disorder and the modern DSM definition both require the inference of a dysfunction to explain (provide a causal account of) the overt behavioral "symptoms" that constitute what may be directly observed of the disorder. Wakefield (1999a) stated: "To be neutral on whether a dysfunction is causing the symptoms would be to be neutral on whether there is a disorder, defeating the DSM's purpose" (p. 967). If no dysfunction can be inferred, then no disorder can be said to exist. I have noted that there are serious problems of circularity of reasoning if the concept of dysfunction cannot be separated from the concept of disorder (Houts & Follette, 1998). The nature of the problem is epistemological. How do we know that some presentation of behaviors is a disorder? Wakefield (1999a) has answered this question by stating that we know that some cluster of behaviors is a disorder because we *infer* that the behaviors have been caused by a dysfunction. From an epistemological point of view, such a response merely shifts the question back to the issue of dysfunction. All right, so we know that some cluster of behaviors constitutes a disorder rather than normal behavioral variation or non disordered deviation from normal behavior because we infer a dysfunction as the cause of those behaviors. It is important to note that this line of argument has shifted the burden of proof from the presumed disorder to the presumed dysfunction. The obvious next question is: How do we know that there has been a

dysfunction? Here is where the circularity enters. The obvious circular move is to answer this second question regarding how we know there is a dysfunction with the response, we know there has been a dysfunction because we cannot otherwise explain the presence of the cluster of behaviors and we have a list of plausible dysfunctions from which to choose. In other words, the grounds for knowing that there is a dysfunction is, we see the behaviors that we see and we cannot imagine any other explanation for the cause of those behaviors except for the explanation that there is a broken process or mechanism inside the organism. We look at the behavior, and we *imagine* that something that we cannot see or have not yet seen has gone wrong or broken down. We *imagine* that something has broken inside the organism, and that broken thing that we imagine and believe in is what we also believe has caused the cluster of behaviors we now label a disorder.

In logical terms, the aforementioned reasoning is a form of abduction (Peirce, 1956) or inference to the best explanation (Harman, 1965). To clarify the issues, some discussion of this type of reasoning is useful. Peirce described this type of reasoning as follows.

> Long before I first classed abduction as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis — which is just what abduction is — was subject to certain conditions. Namely, the hypothesis cannot be admitted, even as a hypothesis, unless it is supposed that it would account for the facts or some of them. The form of inference, therefore, is this:
>
> The surprising fact, C, is observed;
>
> But if A were true, C would be a matter of course,
>
> Hence, there is reason to suspect that A is true.
>
> Thus, A cannot be abductively inferred, or if you prefer the expression, cannot be abductively conjectured until its entire content is already present in the premises, “If A were true, C would be a matter of course” (Peirce, 1956, pp. 151–152).

According to Peirce’s formulation, we may infer some hypothesis from a set of observations so long as the inferred hypothesis is already known to be capable of explaining the observations. In other words, there is some limit on the nature of the hypotheses that may be inferred to explain the observed facts, and not just any hypothesis will do. Applying this to the case of mental disorders, we can formulate the following.

> The surprising set of behaviors, C (disorder), is observed;
>
> But if A were broken (dysfunction), C would be the expected result,
>
> Hence, there is reason to suspect that A is broken.

The key issue is: What is the basis for claiming that when A is broken, this broken mechanism or dysfunction causes C, the observed set of behaviors. In order for this type of logical argumentation to be valid, we would need independent evidence that the hypothetical statement “if A were broken, C would be the expected result” was in fact true. We would need to know that

when you break A, this causes C and breaking A reliably produces the outcome C. Wakefield has failed to meet such criteria, except perhaps in his automotive examples.

Wakefield (1998a, 1999a) has repeatedly used automotive metaphors to illustrate his claims about the relationship between dysfunction and disorder, and he has drawn analogies between the modern DSMs and trouble shooting manuals for automobiles. When an automobile presents a surprising set of behaviors (e.g., it pulls to the right when stopping, and the steering wheel vibrates and catches when turning left), we may be warranted in reasoning that something has gone wrong with the front end alignment, or perhaps the front tires are defective in some way. The breakdown in the analogy with mental disorders becomes obvious when we ask for the basis for inferring that one or another hypothesized broken part has produced the deviant steering behavior. In the automotive example, an inference to the alignment or an inference to the tires is warranted because we know from engineering that those are two candidate causes for the steering anomaly. We can, in fact, deliberately misalign the front end or deliberately damage the front tires and repeat this experiment many times to establish the causal connection between these particular kinds of breakdowns and the resulting "misbehavior" of the automobile. What is even more important from our knowledge of engineering is that we can rule out an inference to the effect that a gremlin under the hood was producing this steering misbehavior by alternately putting an iron bar in the steering mechanism at just the right moment. The science of physics and the applied science of engineering as instantiated in the design, manufacture, and functioning of the automobile provide a body of knowledge that can lead to identification of the cause or causes of the misbehavior of the car's steering. A major difference between the automotive example and the human behavior and mental disorder example is that we simply do not have the comparable knowledge base in the human behavior and mental disorder example. We have not designed and manufactured human beings, and we have not designed them to perform certain behaviors. When humans engage in harmful misbehavior, we typically do not know where to look to find out what is broken that might have caused the harmful behaviors. A knowledgeable auto mechanic can look inside the car to see if something is broken. No mental health professional has comparable knowledge to look inside humans to identify what is broken in the search for causes of harmful behavior. What is more important, in the human examples, there is always a competing hypothesis that can oppose the hypothesis that something inside has broken.

That competing hypothesis, that alternative explanation is normal learning processes operating within a particular behavioral history of a particular organism. In other words, the cluster of behaviors that we identify as being harmful is not a disorder because there is no broken function but only normal variation of behavior. The automobile analogy breaks down because automobiles do not learn. Moreover, the competing hypothesis of normal behavioral variation does not commit us to searching for broken functions inside the organism but directs attention instead to the behavioral history of the misbehaving organism.

A major problem with Wakefield's question begging retreat to dysfunction is that the imputation of dysfunction has no bounds and cannot be eliminated. Setting the criteria for mental disorder to a criterion of dysfunction does nothing to limit what may be considered a mental disorder because any behavior can pass Wakefield's criterion of "plausible dysfunction." Given such lack of constraint on the multiplication of inferred mechanisms, we can always imagine something broken inside the organism. If what we first imagine does not seem believable to the right people, then we can imagine something else that is believable to the right people. An examination of

Wakefield's own writings suggests that any number of dysfunctions may count as plausible. In discussing what might be broken inside people who are behaving in a manner consistent with the label of depression, Wakefield (1998a, 1998b) has repeatedly referred to a broken "*loss-response mechanism*" and "*grief and mourning mechanisms*" (Wakefield, 1997c). In another context, Wakefield noted that "… bad penmanship is not a disorder in itself, but bad penmanship caused by a dysfunction in one of the *mechanisms that enable children to learn to write* is a disorder …" (Wakefield, 1996, p. 649). Similarly, Wakefield has inferred from pica behaviors that "*mechanisms designed to constrain food choices* are dysfunctional" (Wakefield, 1997c, p. 254). In effect, whenever one is confronted with unusual and harmful behaviors, Wakefield's analysis does not preclude the inference of some type of broken mechanism that caused the behaviors. There appear to be no rules or constraints on the type of hypothetical mechanism that may be inferred, and Wakefield has explicitly rejected such requirements.

> The epistemological objection is based on the assumption that, to know that there is a dysfunction, one must know the dysfunctional mechanisms and their evolutionary history. This assumption is false. To know that a dysfunction exists, one need only have sufficient indirect evidence — for example, surface evidence that indicates or correlates with the existence of internal dysfunction — to infer that some mechanism is failing to perform as designed (Wakefield, 1997c, p. 255).

In this passage Wakefield has used the word "know" in a very peculiar way. Harman's (1965) treatment of inference to the best explanation can clarify the error in Wakefield's reasoning.

Harman (1965) noted that a claim to know requires that an inference be both true and warranted. A final belief might be true, but it could be true by accident and not logically warranted. Such a belief would not count as knowledge. Similarly, a final belief might be false but also warranted or logically correct; nevertheless, such a warranted but untrue belief would not count as knowledge. In other words, in an abductive inference chain, one cannot claim as knowledge the final outcome of the chain unless the intermediate hypotheticals can also pass the test of knowledge. Harman gave the following example that is pertinent to Wakefield's aforementioned dismissal of epistemological criticisms of his harmful dysfunction analysis.

> … suppose I read on the philosophy department bulletin board that Stuart Hampshire is to read a paper at Princeton tonight. Suppose further that this warrants my believing that Hampshire will read a paper (somewhere) tonight. This belief is also warranted. Now suppose that, unknown to me, tonight's meeting was called off several weeks ago, although no one has thought to remove the announcement from the bulletin board. My belief that Hampshire will read a paper at Princeton tonight is false. It follows that I do not *know* whether or not Hampshire will read a paper (somewhere) tonight, even if I am right in believing that he will. Even if I am accidentally right (because Hampshire has accepted an invitation to read a paper at N.Y.U.), I do not *know* that Hampshire will read a paper tonight (Harman, 1965, p. 92 italics added).

In Wakefield's treatment of the requirements to "know" if a dysfunction exists, Wakefield does not require that the belief in a broken mechanism be either warranted or true. Instead, in Wakefi-

eld's analysis one is free to infer that there is a dysfunction purely and simply because of surface evidence.

Wakefield's rationale for this muddled type of reasoning is once again a rhetorical appeal to the success of physical medicine. The familiar refrain that the DSMs are just doing what good, successful physical medicine has done was couched in rhetorical terms as follows.

> The requirement that we wait for science to establish the nature of a dysfunction before concluding that there is a dysfunction would have crippled the development of physical medicine. As the history of medicine amply illustrates, it is often possible to recognize from symptoms that a disorder exists long before the nature of the dysfunction is known (Wakefield, 1999a, p. 984).

It is important to note that Wakefield offered no examples from medicine, nor did he refer to any histories of medicine. When in the recent history of medicine has the scientific community *concluded* that there was a dysfunction without knowing the nature of the dysfunction? What has happened in the history of medicine is that dysfunctions have been *hypothesized*, and through a process of rigorous research in basic biological science and epidemiology, various ones of those hypotheses have been discarded whereas others have withstood the test of time. Any conclusion that a dysfunction existed has come after the dysfunction has been identified, not before. That process of conjecture and refutation observed in the history of physical medicine is altogether different from the faulty logic that Wakefield has advocated for the mental health field.

In the case of mental disorders, Wakefield (1999a) has claimed that it is possible to falsify an imputed dysfunction as follows: "… the attribution of disorder ultimately involves a broad theoretical hypothesis that the cause of the symptoms involves a dysfunction, and this hypothesis can be falsified" (p. 967). But how can this imputation of dysfunction be falsified if the dysfunction is not specified ahead of time? If a particular hypothesized dysfunction fails to be supported by etiological investigations, what is to prevent the invention of another one to take its place? Where is the line between normal behavioral variation and disorder to be drawn? According to Wakefield's logic, one can declare that some presentation of behaviors constitutes a disorder, and a disorder is by definition different from normal behavioral variation because the disorder means that the presentation of behaviors has been caused by a dysfunction, and the presumed dysfunction does not have to be proven but only hypothesized to exist. According to this line of reasoning, once a set of behaviors has been declared to be a disorder, then there is not any way to declare them not to be a disorder. The supply of hypothesized dysfunctions is infinite as is well illustrated by the imaginative invention of mechanisms in Wakefield's own writings. This is the line of circular reasoning and ad hoc hypothesizing that leads to proliferation of mental disorders. Wakefield has identified a major cause of the proliferation of mental disorder labels in the modern DSMs.

Wakefield (1999a) has also attempted to remove from the DSMs any responsibility for construct validity of diagnostic labels by advocating theory neutrality and shifting the burden of validity to case formulation rather than diagnosis. He introduced a distinction between diagnosis and case formulation in order to defend the theory neutrality of DSM:

> A diagnosis in the DSM's sense identifies only what is "wrong" or pathological in the individual's functioning, as indicated by manifest symptoms that are a consequence of an internal

> dysfunction. A case formulation offers vastly more information about the etiology and context of the disorder and encompasses many nonpathological features that caused or influence the prognosis of the disorder (Wakefield, 1999a, p. 968).

This deserves careful analysis. There is circularity in the reasoning. Wakefield is saying that the purpose of diagnosis is to identify what is wrong or pathological in functioning, but that is never in fact done. What is identified is a set of behaviors that are declared to be a disorder because it is inferred that something unspecified has gone wrong to cause those behaviors. Wakefield then assigns the task of stating what has gone wrong to case formulation rather than to diagnosis. With this artificial gerrymandering of diagnosis and case formulation, the difficult work of providing construct validity for diagnosis is shifted elsewhere.

## 9. Concluding remarks

Despite his numerous attempts to cast the diagnostic label proliferation of the modern DSMs in a progressive scientific light, Wakefield's efforts fail repeatedly because his analogies between the DSMs and physical medicine simply fail to hold. Wakefield's misreading of traditional philosophical models of scientific progress not withstanding, according to such rationally reconstructed criteria for progress in scientific taxonomy, the label proliferation observed in the modern DSMs is not progressive. To be charitable, we can only conclude that the history of nomenclature in the mental health field has thus far illustrated a field that may be going in circles if anywhere at all.

Wakefield's harmful dysfunction analysis has benefited the field in that he has identified what may be a source of the lack of progress. The modern DSMs have relied on a conceptualization of mental disorder that requires identification of some internal dysfunction to explain each disorder. What has been missing is any clear criteria for making judgments about dysfunctions. Absent such criteria, any and all human behavior can be included under the rubric of mental disorders. Such proliferation of mental disorders has been a scientific and public embarrassment for the mental health sciences. One way to stop the proliferation of mental disorder labels is to require more of the DSMs in terms of specifying the scientific basis for declaring that something has gone wrong inside the organism.

Although it is not currently fashionable to speak of the social and value laden basis of mental disorder diagnosis, we should not forget that the mental health field has a very long history of struggling to define mental disorders in an objective manner that is truly analogous to the manner in which physical medicine has defined physical disorders. Much to his credit and in his first formulations of the harmful dysfunction approach, Wakefield (1992a, 1992b) recognized the central role of values in defining mental disorders. In his more recent defenses of the DSM and of his own harmful dysfunction approach, Wakefield (1999b) has focused on the concept of dysfunction as a possible source from which to leverage mental disorders away from the social and value issues and to place mental disorders on a more objective footing. I do not believe that he has succeeded in finding an objective definition of mental disorders, but that is the subject of a second response that will be forthcoming.

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