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Models of Scientific Progress and the Role of Theory in Taxonomy Development: A Case Study of the *DSM*

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The proliferation of categories in recent editions of the *Diagnostic and Statistical Manual of Mental Disorders* (e.g., 4th ed.; *DSM;* American Psychiatric Association, 1994) is discussed as an indication that the underlying classification scheme is inadequate and unlikely to produce the scientific progress originally envisioned. In any nosological system, it eventually becomes necessary to reduce the number of categories by an organizing theory that describes the fundamental principles underlying the taxonomy. The *DSM* has put itself in an awkward position by claiming to be atheoretical. Although taking such a tack had historical advantages to promote the acceptability of the 3rd edition of the *DSM,* it now limits the progression of science. It is argued that the *DSM* should not be used as the basis for guiding scientific research programs because it emphasizes primarily behavioral topography rather than providing an explicit theory that would allow for an evaluation of scientific progress. Theoretically driven taxonomies should be allowed to compete on the basis of how successful they are at achieving their specified goals that might include illuminating etiology, course, and response to treatment. Such systems are not likely to attend primarily to behavioral topography alone and would probably organize behavior differently than the current categorical syndromes seen in the 4th edition of the *DSM.*

Since publication of the third edition of the *Diagnostic and Statistical Manual of Mental Disorders* (*DSM–III;* American Psychiatric Association, 1980), the taxonomy proposed by the American Psychiatric Association has become more dominant than anyone would have reasonably predicted at the time. Scientific review boards organize funding along categorical lines described in the *DSM,* journal titles reflect these categories, and third-party reimbursement frequently requires *DSM* diagnoses before rendering payments. The powerful influence of the *DSM* since 1980 has been surprising, given the relative lack of impact made by the first and second editions of the *DSM* (*DSM–I* and *DSM-II,* respectively; American Psychiatric Association, 1952, 1968). In spite of the *DSM*'s widespread adoption, we argue that it is a flawed classification system on which to base a research program.

In this article, we make the following points about the editions of the *DSM*s, from *DSM–III* to *DSM–IV* (the modern *DSM*s; American Psychiatric Association, 1980, 1987, 1994):

1. The modern *DSM*s have claimed to be atheoretical and have done so for two pragmatic reasons. First, to be acceptable to the broader mental health community, explicit statements of the implicit underlying model were avoided. Second, the existing psychoanalytic theory that still influenced *DSM–II* was scientifically inadequate and rather than fight for a replacement theory, it was expedient to ostensibly scrap theory altogether.

2. Any classification that is ultimately successful entails some level of theory. Failing to specify the theory causes basic definitional problems that limit the utility of the classification system.

3. As a result of poorly explicated theory, there is little evidence that the *DSM* is producing scientific progress as judged by some philosophical ideals.

4. The modern *DSM*s may avoid explicating their theoretical underpinnings, but the underlying ontologies of a weakly stated medical model are easily deducible from their content.

5. There are disadvantages to having an atheoretical classification system. It slows research and makes science more difficult. This problem could be improved by the strengthening of multiple theory-based research programs.

Two factors contributed to the acceptability of the *DSM*s and deserve some attention. First, beginning with the *DSM–III* (American Psychiatric Association, 1980), the orthodoxy of psychoanalytic theory as an explanatory model for the taxonomy of mental disorders was abandoned in favor of a more ecumenical approach that was stated to be "atheoretical." Second, the neo-Kraepelian movement imported the methods and promise of a medical model that emphasized formal classification into the field when psychology was sympathetic to methods that seemingly emphasized empiricism and reliability (Blashfield, 1984).

The Decision to Present the *DSM* as Being Atheoretical

The modern *DSM* task forces have worked in an environment that invoked competing goals. On one hand, the *DSM–III* committee worked to achieve a document that would be generally acceptable to a wide constituency. On the other hand,

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the *DSM* is an organ of the American Psychiatric Association and, as such, reflects the underlying model of traditional medicine. However, because of the past unproductive link between psychoanalytic psychiatry and the medical model, the medical model was in some disfavor as an organizing principle for psychiatric classification. By medical model, we are content to use the characterization as given by Blashfield.

> The medical model is a perspective that has implicitly permeated the mental health field. From the perspective of the medical model, all mental disorders are *diseases*. The persons afflicted with these diseases are called *patients;* they need treatment from *doctors; diagnosis* is an essential first step if one is to prescribe the best *therapy* and to predict the natural *course* of the patient's disorder. Severely disturbed patients need *medication* and perhaps *hospitalization;* their care should be paid for by *health insurance policies* [all italics in original]. (1984, p. 26)

At about the same time as the *DSM–III* Task Force was working, there was an even stronger position taken about the nature of underlying theory of how to approach the study of "mental illness." This position was staked out by the neo-Kraepelians, several of whom were consultants to the *DSM–III* project. Among other tenets of those holding this point of view, Klerman (1978) explicitly stated the neo-Kraepelian position that psychiatry was a branch of medicine, psychiatry treats people who are sick and who require treatment for mental illness, there is a boundary between the normal and the sick, there are discrete mental illnesses, and the focus of psychiatric physicians should be particularly on the biological aspects of mental illness.[1] This perspective represented a strong biological model of mental illness with more ontological entailments than the medical model.

Establishing the Jurisdiction of Professional Authority

Just how the *DSM* was presented and how it defined its focus, mental disorders, were not at all trivial issues. One of the functions of the definition of a mental disorder was to define who was enfranchised to participate in the various roles of the delivery of mental health services (Moore, 1978).

Because *DSM* has been so widely accepted, it is easy to forget the initial controversies surrounding early proposals for the definition of a mental disorder. At one point, Spitzer offered a proposal to define mental disorders for *DSM–III* that was explicit in declaring that all mental disorders were medical disorders. Spitzer and Endicott gave the following definition, which had first been presented in a professional presentation in 1976.

> A medical disorder is a relatively distinct condition resulting from an organismic dysfunction which in its fully developed or extreme form is directly and intrinsically associated with distress, disability, or certain other types of disadvantage. The disadvantage may be of a physical, perceptual, sexual, or interpersonal nature. Implicitly there is a call for action on the part of the person who has the condition, the medical or its allied professions, and society. A mental disorder is a medical disorder whose manifestations are primarily signs or symptoms of a psychological (behavioral) nature, or if physical, can be understood only using psychological concepts. (1978, p. 18)

At first, Spitzer and Endicott seemed not to understand that this wording raised the hackles of psychologists and insisted that their definition was not speaking to the issue of who would be responsible for treatment or research of conditions declared to be mental disorders under this basic definition (Millon, 1986). However, the professional jurisdiction issue came to a head when Spitzer, who was head of the American Psychiatric Association group developing the *DSM–III*, proposed that his paper containing the aforementioned definition be included as an appendix to the *DSM–III*.

The American Psychological Association formed its own task force (the Task Force on Descriptive Behavioral Classification) to respond to *DSM–III* proposals, and there were rumblings of legal action if the American Psychiatric Association persisted in retaining the statement about medical disorders, either as an appendix or as part of a foreword, which was also considered. After rather heated exchanges between the respective presidents of the two associations, the Spitzer group finally agreed to drop any references to mental disorders being a subset of medical disorders from the final draft of the *DSM–III*. This "resolution" of the jurisdiction issue resulted in effectively halting the opposition of the American Psychological Association to the *DSM–III*, including the abandonment of efforts to develop an alternative psychological approach to classification.

Early objections, both within and outside the *DSM–III* task force, to an explicit statement in the text of *DSM–III* that the medical model was the organizing principle for the *DSM* was successful at preventing the document itself from containing the offending words (Millon, 1986). The framers of the *DSM–III* understood that if there were a theoretical statement, there would be little chance of the document being widely accepted. In addition, there would have been the problem of how to treat psychoanalytic theory that had survived into the *DSM–II* (see Millon, 1986, pp. 42–47). So overtly, the document was presented as syndromally based and atheoretical. Thus, in the *DSM–III* and *DSM–III–R*, the atheoretical nature of the document was described using the following language.

> The approach taken in DSM-III [same words in the *DSM–III–R*] is atheoretical with regard to etiology or pathophysiological process except for those disorders for which this is well established and therefore included in the definition of the disorder. Undoubtedly, with time, some of the disorders of unknown etiology will be found to have specified biological etiologies, others to have specific psychological causes and still others to result mainly from a particular interplay of psychological, social and biological factors.
>
> The major justification for the generally atheoretical approach taken in DSM-III [same words in the *DSM–III–R*] with regard to etiology is that the inclusion of etiological variables would be an obstacle to use of the manual by clinicians of varying theoretical orientations, since it would not be possible to present all reasonable etiological theories for each disorder. (cf. American Psychiatric Association, 1980, p. 7; 1987, p. xxiii)

In effect, the framers of the *DSM–III* succeeded in avoiding any direct confrontation with their professional opponents, and in doing so they developed a strategy that has been successful in subsequent editions of the *DSM* to ensure its widespread adoption. The strategy of co-opting the opposition has required that

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[1] See Blashfield (1984, pp. 27–37) for an explication of these and other neo-Kraepelian propositions as well as an interesting "family tree" of the participants in the movement.

the *DSM* continue to seemingly espouse an atheoretical approach to the general definition of mental disorder and to definitions of specific disorders.

The result remains that the *DSM* has been explicitly atheoretical but implicitly a medical model. This inconsistency, rather than representing a grand conspiracy of the medical establishment to subtly take over the mental health field, may actually result in a diminution of scientific credibility for the *DSM* as an organizing nosology from which to study behavior. *DSM* adherents seem oblivious to the need for theory to organize classification and, thus, appear to go blithely along believing that the *DSM* is atheoretical, will remain atheoretical, and that it is good to be atheoretical. None of these things is true. In the next section we examine the predictable and unfortunate legacy of the decision to present the modern *DSMs* as atheoretical.

The Role of Theory in Classification

Blashfield (1984) has stated that "classificatory systems and theories are necessarily intertwined" (p. 80). Theory and classification reciprocally influence one another. Initially science can and does classify objects on the basis of readily observable properties without being guided by theory. However, without theory, categories proliferate, and any atheoretical system will eventually fall of its own weight as will classification systems that are based on inadequate theory (Faust & Miner, 1986). Popper made this clear.

> The belief that we can start with pure observations alone, without anything in the nature of a theory is absurd . . . . Twenty five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: Take a pencil and paper; carefully observe, and write down what you have observed. They asked of course, *what* [italics in original] I wanted them to observe . . . . Observation is always selection. It needs a chosen object, a definite task, an interest, a point of view, a problem. (1963, p. 46)

However, the two immediate predecessors to the *DSM-IV* claimed the virtue of being atheoretical and *DSM-IV* is silent on the issue of its underlying theory. This makes answering the following essential question difficult.

What Is a Mental Disorder?

One cannot begin to develop a taxonomic system without an inclusive definition of what the object of study is. Whereas the *DSM-IV* does not directly espouse a medical model, the explicit language about the atheoretical nature of the *DSM* is absent from the *DSM-IV*. However, there is no affirmative statement clarifying the underlying theory of the *DSM-IV*. The inconsistent strategy of importing a medical model as the basis for the *DSM*, although still keeping the basic theory unspecified to gain acceptance and ward off criticism, has costs that can be seen when the weaknesses are examined with regard to how it attempts to identify the unit of study, the "mental disorder."

The problem of defining a mental disorder and related concepts such as mental illness has been the focus of many heated debates throughout the history of the mental health field, and recent *DSM* approaches to the definition of mental disorder and critiques of those illustrate the continuation of this debate. In general, one may approach the problem of definition from either a lexical or a stipulative standpoint (Moore, 1978). In the lexical approach, one asks how a term is actually used. By examining usage using a philosophical analysis such as Wittgenstein might or a behavioral analysis (Skinner, 1945), one may inductively extract some definition of mental disorder by discerning how professionals and the public use "mental disorder" in their respective discourses (O'Donohue, 1989b). In the stipulative approach, one asserts how the term ought to be used and judges the utility of the stipulated definition according to how well the definition fulfills certain purposes. One purpose that Moore (1978) has noted is that definitions of mental disorder need to be able to adjudicate hard cases. Recent *DSM* efforts to define mental disorder have taken a stipulative approach.

Adjudication of Hard Cases in the DSM

The authors of the *DSM-IV* have recognized the need to continue the stipulative definitions of the *DSM-III* and *DSM-III-R* to "guide decisions regarding which conditions on the boundary between normality and pathology should be included in *DSM-IV*" (American Psychiatric Association, 1994, p. xxi). This quotation expresses the purpose of adjudicating hard cases, and the following definition of mental disorder has been offered in the *DSM-IV*.

> In *DSM-IV*, each of the mental disorders is conceptualized as a clinically significant behavioral or psychological syndrome or pattern that occurs in an individual and that is associated with present distress (e.g., a painful symptom) or disability (i.e., impairment in one or more important areas of functioning) or with a significant increased risk of suffering death, pain, disability, or an important loss of freedom. In addition, this syndrome or pattern must not be merely an expectable and culturally sanctioned response to a particular event, for example, the death of a loved one. Whatever its original cause, it must currently be considered a manifestation of a behavioral, psychological, or biological dysfunction in the individual. (1994, p. xxi)

As Wakefield (1992) has pointed out, this definition can be summarized as "harmful dysfunction." A condition is a mental disorder if it has negative consequences for the person and also signifies a dysfunction. The idea of dysfunction is crucial to the definition, because something is not a disorder unless something has gone wrong in the person. Klein has made a cogent argument for the importance of dysfunction as the basis for the concept of disease as used in mental disorders and mental illness as well.

> Modern science has developed the concept of objective underlying disease processes, demonstrating that the inference that something has gone wrong is not simply arbitrary. Disease is defined here as covert, objective, suboptimal part dysfunction, recognizing that functions are evolved and hierarchically organized. It is argued that disease is not simply an arbitrary social evaluation but is derivable from the concept of optimal biological functioning, within an evolutionary context. (1978, p. 70)

In their analyses of the *DSM* approach to defining mental disorder, which was introduced by Spitzer (Spitzer & Endicott, 1978; Spitzer & Williams, 1982), both Wakefield and Klein have made it clear that the *DSM* approach appeals to the concept of dysfunction to provide an objective biological basis for demarcating mental disorders from other behavior.

Interestingly, both Wakefield and Klein have noted that the *DSM* approach has not been explicit enough in specifying criteria for determining when something (the part) has gone wrong or has "dysfunctioned." The original Spitzer definition of mental disorder carried over in the past three editions of the *DSM* has relied on statistical infrequency as a proxy for dysfunction. Both Wakefield and Klein have pointed out that this approach to specifying when a dysfunction is present lacks the rigor needed to obtain an "objective" determination of dysfunction. They have proposed instead that the long sought after objective basis for defining mental disorders in a nonarbitrary way can be found in evolutionary theory.

> A careful analysis of the concept [of dysfunction] leads to the conclusion that the most viable approach is based on the notion of evolutionary design. (Wakefield, 1992, p. 236)

> A dysfunction exists when a person's internal mechanisms are not able to function in the range of environments for which they were designed. (Wakefield, 1992, p. 243)

> Can we arrive at a standard that is not simply an expression of personal preference, but is given to us by the biology of the situation? I propose that evolutionary theory allows us to infer such a standard—suboptimal functioning—and further helps us to objectively specify the optimum. This often allows us to state that something is biologically wrong, not simply that it is rare or objectionable (Klein, 1978, p. 50).

This type of proposal to resolve the perennial issue of social values intruding into definitions of mental disorders merits careful analysis because it is a bold claim and likely to continue to be a common strategy of those proponents of the *DSM* who argue that the expansion of diagnostic categories is based on objective science rather than social convention. Arguments about part dysfunction are based on assumptions that an organism or system (O) is "naturally inclined" toward some endstate (ES), and this is facilitated by a part (P) that has some effect (E) (Moore, 1978). In this type of analysis of function, we "know" that the function of the heartbeat (P) is to circulate blood (E) because we assume that the body (O) inclines toward health (ES). The beating of the heart has numerous other effects like making sounds in the chest and a pulse at the wrist. We declare circulation to be the function of the heart because we can establish a necessary link between circulation and health. The body will not maintain itself in the endstate of health unless circulation occurs. The point is that a particular effect of a part that we declare to be the function of the part depends on what we take to be the endstate that is being maintained by means of some homeostatic regulation of the system. Analysis in terms of part dysfunction requires being able to state what the causally necessary relationship is between a function we declare to be the function and some endstate we assume to be the "natural" endstate of the system.

Two problems arise when one attempts to take this part dysfunction analysis from physical medicine into the mental health field as both Wakefield and Klein attempt to do. The first problem concerns the specification of end states. As Moore (1978) has noted, although "there are homeostatic mechanisms in the body which might allow one to pick out one consequence of an activity as its function, there are no unproblematic homeostatic mechanisms 'in the mind'" (p. 103). Moreover, even in physical medicine, the identification of endstates of the body is parasitic on specification of an ultimate endstate such as health, which is culturally rather than biologically defined. "All medicine, with its functional organization of persons, reflects our society's judgments of well-being, and is in that sense normative [or value laden]" (Moore, 1978, p. 103). It has been argued elsewhere that there is little foundation on which to build an argument that we know what endstate is entailed when we speak of mental health or psychological well-being (Follette, Bach, & Follette, 1993). So far, it is almost purely the absence of psychopathology. This state of affairs makes the approach of Wakefield and Klein circular.

The second problem with appeals to part dysfunction to define mental disorders concerns the invocation of evolutionary theory to supply "objective" endstates for various behaviorally defined functions such as perception, memory, anticipatory anxiety, and rational thought. Boorse (1977) has claimed to be able to justify an "objective" definition of part dysfunction in the case of physical disease by assuming that the statistically average human body and its parts function to ensure survival and reproduction, and he has defined disease as deviation from some idealized average. Boorse's argument is: What is healthy is what is natural, and that what is natural is what we can now observe and infer to have been the product of natural selection. As is evident in Boorse and in the previously quoted material from Wakefield and Klein, their appeals to evolutionary theory are appeals to a view of evolution that has been soundly rejected. To take the application to mental disorders, the idea that selection operates at the level of behavioral processes so that organisms are gradually transformed and the functions of their various parts are "designed" toward some grand perfection was not endorsed by even Darwin himself (Gould, 1977; Mayr, 1988). The "engine" of selection is reproductive fitness, and some function may be selected toward that end only to turn out to have some other totally different function later. It is not possible to know the function of some part or process by appealing to evolutionary history. Contemporary evolutionary theory has emphasized that random variation and fortuitous selection make it impossible for us to be able to read off the "design" of various parts of organisms from their evolutionary history. As Tattersall (1995) has noted: "It is likely, then, that we are simply not in a position to discern all the important events in cognitive development that accompanied human physical evolution—and that presumably fed back into it" (p. 242). Moreover, the Panglossian pipe dream to extract norms of functioning from evolutionary theory to define objectively what is a dysfunction is also counter to the spirit of evolutionary theory that asks instead why some apparently dysfunctional behavior survived the process of natural selection unless it was, in the first place, functional. Thus, we are left an unsatisfactory solution to the problem of developing a definition of mental disorder that can establish boundary conditions.

The arguments of Klein, Wakefield, and Boorse and our responses to them are of academic interest, but these discussions are not the kinds of considerations that went into the definition of mental disorders found in the *DSM-IV*. Those considerations are summarized as follows:

> The ever-increasing number of new categories meant to describe the less impaired outpatient population raises the question of where psychopathology ends and the wear and tear of everyday life begin. DSM-IV answers this question somewhat tautologically by emphasizing the requirement that the condition cause clinically significant impairment or distress, but it does not clearly operationalize the term *clinical significance* [italics in original]. The evaluation of clinical significance is likely to vary in different cultures and to depend on the availability and interests of clinicians. (Frances, First, & Pincus, 1995, p. 15)[2]

It is difficult to see how one can discern the validity of diagnostic categories when the authority responsible for forming them readily recognizes that they are culturally influenced judgments subject to the changes of the interests of those constructing them. We do not cite this quote to disparage the efforts of the *DSM-IV* Task Force, and most certainly not to make it less likely that descriptions of the context in which framers made their decisions will be shared in the future. Rather, the purpose is to make it clear that some of the most basic and important issues forming the foundation of the *DSM* are flawed and embedded in a social context. We started this section by stating that one of the goals of a definition of a mental disorder was to help identify boundary conditions. On this issue, the framers have concluded that the current definition fails.

> In summary, psychiatric disorders are neither homogeneous nor divided by clear boundaries. The two most important issues to understand in using DSM-IV are that 1) there is considerable heterogeneity of the presentations encountered even within each disorder, and 2) the boundaries between disorders are often fuzzy; many patients have presentations that fall through the cracks and cannot be comfortably forced into any of the DSM-IV categories. (Frances et al., 1995, p. 19)

As a result of starting out with an atheoretical position while still seeking to honor a biological heritage, the term *mental disorder* has become one of the weak links in any justification to preserve the *DSM* as a scientific organizing mechanism. In the creation of a taxonomy, the definition of the object of study is obviously crucial. However, the history of the *DSM* has significantly compromised the definition. The problem is well stated in the *DSM-IV Guidebook*.

> The use of the term *mental disorder* in the title of DSM-IV (The *Diagnostic and Statistical Manual of Mental Disorders*) is an anachronistic preservation of the Cartesian view. This term appears increasingly silly as we learn more and more about the physical correlates of thought, emotion, and psychopathology. The term most frequently suggested as an alternative to replace *mental disorders* has been *brain disorders*, but this is equally unfortunate and reductionist in the opposite extreme. Preferable terms for the universe of conditions defined in DSM-IV would be *psychiatric disorders* or *psychological disorders*, but neither of these is feasible because of the possible professional turf conflicts they might incite among psychiatrists, psychologists, and other mental health professionals. Unfortunately, we could not come up with a better term than *mental disorders* and thus it survives in DSM-IV [all italics in original]. (Frances et al., 1995, p. 16)

So far, we have said that, although there is an implicit model behind the modern *DSM*s, for practical (however short-sighted) reasons, it has not been stated explicitly. The cost is that the history of the *DSM* is not consistent with some models of scientific progress.

Models of Scientific Progress

What are the implications of the modern *DSM*s being explicitly atheoretical yet implicitly based on a weakly explicated medical model? We argue that the benefits of avoiding conflict over which theory should be offered as a basis for clarifying the *DSM* structure has been outweighed by the costs of a scientific endeavor that is failing.

At first inspection, what seems stunning about changes in the *DSM* from 1952 to the present is the massive expansion of the manual in both physical size and the number of diagnostic labels included. Seeing the subsequent editions from the *DSM-I* to the *DSM-IV* on a bookshelf makes an instant impression. The expansion is also evident in Figure 1, which shows the growth in the *DSM* of the number of pages and diagnoses from 1952 to the present. As the interval between subsequent editions has become shorter and the scope of human behavior that can be diagnosed has become wider with each new edition, commentators have become more and more skeptical about claims that the *DSM*s represent growth of scientific knowledge. Critical commentary has ranged from the charge that the American Psychiatric Association has found the goose that lays golden eggs in the form of publication profits with each new edition (Kendell, 1991; Kirk & Kutchins, 1992; Zimmerman, 1990) to more subtle criticisms based on philosophical and historical hypotheses about how taxonomies should look when science is progressing toward more and better knowledge in a domain of inquiry. In this section, we focus on the question of whether the steady expansion of the *DSM* is consistent with any model of scientific progress articulated in philosophical and historical studies of science.

One important philosophical analysis of the role of taxonomy in the progress of science was offered by Hempel (1965) in an invited address that he delivered in 1959 to the group of psychopathology researchers organized by Zubin in New York. Hempel's essay has been widely cited in discussions of various editions of *DSM*, but the full extent to which *DSM* expansions have failed to square with Hempel's model of scientific progress has not been summarized. It is important to note that Hempel's model of scientific progress was one that declared progress in knowledge when more and more phenomena were brought under more and more general covering laws. The covering law model of scientific explanation also provided a model of scientific progress. Scientific explanations were viewed as logical deductions that took the form of a statement of initial conditions along with some law of nature from which the observed phenomenon could then be logically deduced. For example, the observation that a wall painted white subsequently turned black

[2] We often cite the *DSM-IV Guidebook* in this article as a source to uncover what the framers of the *DSM-IV* were considering at the time the document was being prepared. We recognize that this source does not capture all the points of view being debated at the time. However, it is authored by the *DSM-IV* Task Force chair and published by the press of the governing body behind the *DSM-IV*. Thus, we think of it as one source of information about the "original intent" of the framers of the *DSM-IV*.

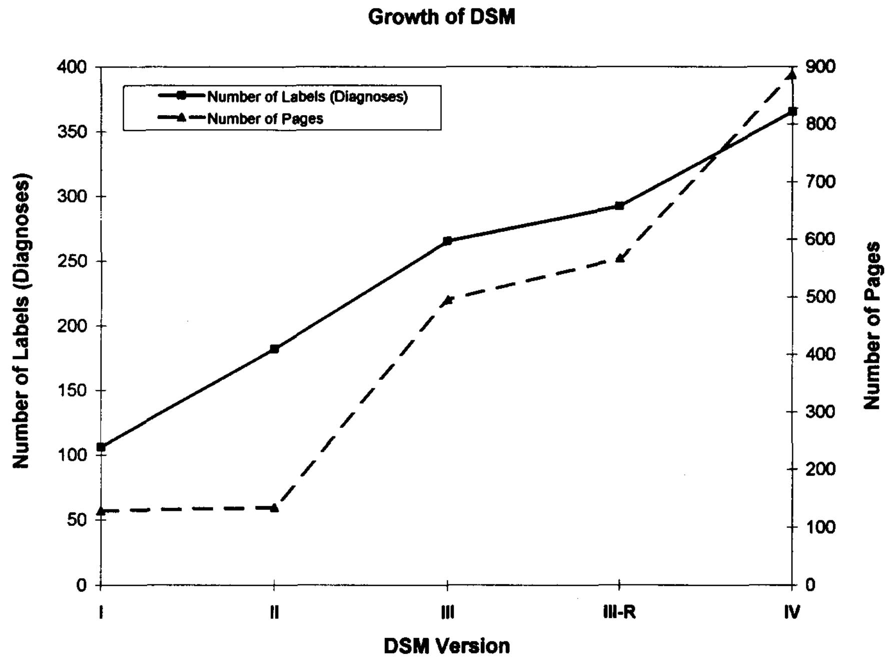

Figure 1. The growth of successive editions of the *Diagnostic and Statistical Manual of Mental Disorders* (DSM) Versions I, II, III, III–R, and IV are the first, second, third, revised third, and fourth editions, respectively.

could be explained by noting that the paint contained lead carbonate and that the gas heater in the room emitted sulfur. These two initial conditions, along with the general law that sulfur and lead carbonate combine to form lead sulfide, permit the logical deduction that the wall turned black by forming a patina of lead sulfide (Kim, 1967). The grander concept of scientific progress in Hempel's logical empiricism was the parallel concept of theory reduction, not to be confused with physiological reductionism (see Weinberg, 1995, on grand reductionism vs. petty reductionism). Progress was said to have occurred when the concepts and terms of one level of explanation could in turn be explained by the concepts and terms of a broader level of explanation. For example, one could talk about the formation of lead sulfide at the level of physical chemistry and describe regularities with which certain elements combined, but once the laws of atomic theory were articulated, then one could deduce the regularities of physical chemistry using initial conditions of atomic weights and laws of physics.

In Hempel's model of logical empiricism, scientific progress occurred when more and more general covering laws could explain the observed phenomena. Specifically, with regard to taxonomy, Hempel (1965) noted that progress occurred when classification systems were reduced by the innovation of a theory to account for the variety of observations. For example, primitive biological knowledge was organized in terms of description and categorization based on surface features of organisms. Before Darwin, complex taxonomies emerged, and the number of categories needed to account for living things proliferated. With the rise of evolutionary theory, classification in terms of surface features was replaced by classification based on phylogenetic and later, genetic concepts. The role of taxonomy changed from one of mere information storage and retrieval to one of providing evidence for a theory of origins (Mayr, 1982). Taxonomies no longer did the job of explaining but instead became the thing to be explained. According to Hempel's model of scientific progress, progress occurs when there is a reduction by theory of the number of taxonomic categories. The mere proliferation of categories is a sign that progress is not, in fact, occurring.

From the perspective of logical empiricism, the recent expansions of the *DSM* to include more and more diagnostic labels to account for more and more human behavior as mental disorders does not suggest scientific progress. Kendell recognized this basic point when he commented as follows.

> Other branches of medicine and other fields of learning did not progress by a dogged pursuit of better and better classifications of their subject matter. They did so by acquiring new technologies, by developing radically new concepts, and by elucidating fundamental mechanisms. (1991, p. 301)

The proliferation of diagnostic categories observed over succeeding editions of the *DSM* is not consistent with a traditional model of scientific progress, yet proponents of the *DSM* have routinely claimed that expansions of the *DSM* are a sign of scientific progress. The fact that taxonomic systems have historically shown proliferation of categories only to topple of their own weight is recognized by the creators of the *DSM-IV*, who describe the taxonomic system of Boissier de Sauvages, which "listed over 2,400 species of diseases in which each species was essentially a separate symptom" (Frances et al., 1995, p. 5). In a recent review of the *DSM-IV*, Guze (1995) said "we are impressed by and worried about the phenomenal growth in the number of diagnoses now recommended" (p. 1228). The basis of the worry should be that this growth is understood to be an indication that the taxonomy is not flourishing but foundering because a system that merely enumerates symptoms and then syndromes cannot exhibit simplification by the application of an organizing theory. The modern *DSM*s have previously been stated to be atheoretical and may now be considered to be only a weakly explicated medical model. As noted earlier, the atheoretical nature of the modern DSMs may be a political legacy that arose from an initial compromise with the American Psychological Association during the drafting of the *DSM–III*, but it is ultimately a scientific millstone and a dead end.

If the outcomes of *DSM* revisions have not been consistent with what would be expected from philosophical and historical analysis of scientific progress, what about the methods used to make revisions in the *DSM?* Once again, a close examination of the procedures has shown rather drastic inconsistencies with Hempelian ideals. Margolis (1994) has noted that the aim of taxonomy within a covering law model of scientific knowledge is to collect taxonomic categories that have some constancy precisely because they denote natural kinds of phenomena that can be explained by laws of nature. According to this ideal, the taxonomic categories are not mere prototypes induced from repeated observations and common consensual usage; rather, they are instances of lawlike generalities. What makes the categories stable is not mere social convention but certain empirical regularities that can be subsumed under lawlike generalizations (see Meehl, 1995). However, as Margolis has noted, the problem with the approach to taxonomy in the *DSM* is that it explicitly aims to base taxonomy on social consensus and deliberately eschews any appeal to theoretical constructs. Hence, procedures for introducing new diagnostic labels and for revising old ones guarantee slippage of the categories rather than stability. What is worse, the slippage goes unnoticed, as Margolis has noted.

> The very use of the Manual creates the false, altogether misleading, completely artifactual, impression of the strict constancy of the Manual's diagnostic categories. In a word, the perceived constancy of the taxonomy cannot but be an artifact of historically changing professional perception. Its apparent constancy cannot be justifiably anticipated to remain hospitable to the progressive discovery of pertinent law-like regularities. (1994, p. 110).

Thus, while adopting the accouterments of logical empiricism such as "operational definition" and "scientific progress," the modern *DSM*s have also abandoned the substance of that philosophy of science.

The *DSM–IV* struggles with the issue of how it is that the process of syndromal classification can "discover" real disease entities. Those who engage in attempts to classify often cite the ideas of Thomas Sydenham, the 17th-century British physician who believed that by observing uniform presentations of symptoms, one could eventually identify diseases that had independent existences that would manifest themselves similarly across individuals (Frances et al., 1995, pp. 4–5). The *DSM–IV* claims to have an instrumentalist or pragmatic epistemology (Frances et al., 1995, pp. 13–14). The truth criterion for evaluating such an epistemology is judged by whether it is effective or ineffective with respect to achieving stated goals. The difficulty with the *DSM–IV* is that goals are not stated in such a way that would allow one to evaluate whether the *DSM–IV* is working as intended.

The *DSM–IV* would have to be considered a success with respect to allowing for enhanced description, which is certainly one of the goals of a classification system. However, there is little to point to for examples of success in making more effective our ability to predict or explain what we are trying to classify. More than that, there is no established mechanism by which new information is evaluated to determine whether the DSM–IV approach is working. We have argued earlier that the proliferation of categories is consistent with an assessment that scientific progress is not occurring. It is possible that some organizing theory will come along some time far in the future and help start collapsing categories. However, there is no reason to believe that this will be recognized because there is no agreed-upon way to determine how categories are to be evaluated.

Already there is evidence that the process of deciding what and how to include or exclude categories or axes from the *DSM–IV* is going awry. First, although the review process for evaluating new and existing categories started with a scientific review of the existing research literature, the Task Force still ultimately used consensus as the method for adjudicating the inclusion or exclusion of candidates. This issue was discussed by Frances et al.

> One often asked question is how much DSM–IV really is based on empirical evidence versus its being the result of the same kind of expert consensus that informed DSM-III, DSM-III-R, and ICD-10. For many issues this is a false dichotomy. Very rarely in any science, and almost never in the clinical science of psychiatry, do empirical data stand up and say "this is the only way I can be understood!" All scientific judgments require some combination of evidence and interpretation that results in the formulation of new hypotheses that are then subject to the collection of more evidence and interpretation, and so on. Although based on empirical data, DSM–IV decisions were the result of expert consensus on how to best interpret the data. Moreover, a number of decisions were not based on data at all. These fell into two categories. Some decisions were broadly conceptual (e.g. elimination of the term *organic* [italics in original]), where others entailed no more than detailed editing of phrases to increase clarity of language. (1995, p. 34)

Noting this point is important because one needs to realize that ultimately this scientific endeavor is the actions and opinions of the 27 members of the *DSM–III* Task Force. Their decisions were based on input from workgroups, but the workgroups tended to recommend including more categories whereas the task force tried to hold the line on proliferation. The fact that this *DSM–IV* document is ultimately a human endeavor is not the basis of the criticism. The criticism is that the epistemology described for the *DSM–IV* is instrumentalist or pragmatic where effective action is the truth criterion. We wonder when the definition of "effective" will be sufficiently fleshed out so that it can inform the decision-making process.

The next troubling change that is emerging from the revision process is that new axes are being considered with no identifiable principle behind what would or does make them appealing. Consider first the defensive functioning scale that may be used to supplement diagnoses on Axis II. This scale lists 31 defense mechanisms that the clinician may note at the time of evaluation. These defense mechanisms include those based on psychoanalytic theory (e.g., repression, reaction formation, sublimation, projection, etc.), which was rejected in the *DSM–III*. Namely, the *DSM* tried to exclude phenomena that were based on theories that were generally untestable and led to unreliable categories. However, here they are in the *DSM–IV* as components of a new axis being considered for further study. How did they get there for further study when psychoanalytic theory had not demonstrated its utility as a basis for an empirically based nosology in the years before the *DSM–III?*

The other scale to appear in the *DSM–IV* as a potential new axis to be used as a supplement to Axis V is the Global Assessment of Relational Functioning (GARF) Scale, a rating scale ranging from 0 to 100 points for rating functioning in the domains of family or other relational units in the areas of problem solving, organization, and emotional climate. That the *DSM–III* Task Force would see merit in considering relationships as important is not at all the issue. The issue is what kind of evidence seemed important enough to justify its inclusion. Again, we look to Frances et al. for a justifying principle.

> Practitioners with a primary interest in the family/systems approach to diagnosis and therapy also have felt relatively disenfranchised by the *DSM* approach. The *DSM* system is by definition a classification of mental disorders as these present in individual patients. In contrast, family/systems therapists often view the relational system (rather than any one individual involved in it) as the target of diagnosis and intervention. Therefore, they have been frustrated by the lack of utility of the *DSM* system for describing families seen in their practices. (1995, p. 81)

A case can be made (see Fruzzetti, 1996) for why relationship variables matter. What is not clear is how the task force decides when and how to include them. It would be surprising if including them as a descriptive axis satisfies whatever unit of analysis concerns more systemic researchers would have about the *DSM.* If one considers the reintroduction of psychoanalytic defense mechanisms and the logic behind inclusion of the GARF, it is increasingly difficult to see how one of the principles for inclusion or exclusion of categories or axes into the *DSM* can be called anything other than appeasement. Indeed, it appears to be the strategy used with the American Psychological Association in the late 1970s: include the opposition.

Ontological Problems and Issues

Stop and think about the virtues and problems with *DSM–IV.* We are well past decrying its existence. People classify, scientists classify. The issue is what should be the basis for such classification. The framers of the *DSM–IV* continue to think that "syndromes cluster together in some meaningful way, which perhaps reflect a common etiological process, course, or treatment response" (Frances et al., 1995, p. 17). This idea reflects the faith that studying these syndromal clusters will reveal the true state of nature (i.e., disease entities will become known). What seems to have gone unnoticed is that, despite claims that the *DSM* is atheoretical, it cannot be. If one examines the aforementioned quotation, it specifies those things we agree to study and thereby accept as propositions of a theories' underlying ontology. O'Donohue has pointed this out clearly.

> First, problems presuppose an ontology in that the problem statements make reference to certain kinds of entities . . . Different metaphysics result in the framing of different kinds of problems: 'What sin or demonic possession caused this speaking in tongues, and what penance or prayer can remedy it?' versus 'What physiologic problem caused this delirium and what physical-chemical intervention can remedy it?' (1989a, p. 1465)

Thus, using the *DSM–IV* to guide a research program entails accepting that there are syndromes, that there are particular meaningful clusters of symptoms, that they have a common etiology that is uniquely identifiable, that they will unfold in a consistent manner, and that they will respond to a treatment. Do all who support *DSM*-guided research agree with those ontological assumptions? Is it working for the implied goals of identifying etiology, course, and response to treatment?

The fact is that there is hardly any evidence that this approach is producing useful information on etiology or course except where it is already built into the diagnostic category such as in the cases of posttraumatic stress disorder (etiology), or dysthymic disorder (course). The evidence most commonly referred to for success of the medical model to identify etiology is the genetic data, indicating that for some categories there is evidence of heritability. There are two problems with this type of evidence. First, there is no major diagnostic category where twin or adoption studies follow any specifiable genetic transmission pattern. Second, current behavioral genetics have done little to thoughtfully address the way nonbiological factors may lead to an expression of any genotype. Take, for example, someone who is socially isolated and rejected, and therefore depressed because he or she is judged by our culture to be ugly. Few would argue with the proposition that physical form has a genetic component. However, the mechanism by which the genotype would manifest a depressed phenotype is purely environmentally mediated. Furthermore, the standards of physical attractiveness differ greatly within cultures over time and between cultures, meaning that the evidence over time in this example would not be stable. In the standard adoption and twin studies, an unattractive person would consistently be rejected and therefore experience isolation and depressive symptoms. The underlying mechanism of the "disease" in this case is how others value physical attributes at a particular time and place. However, the results of this hypothetical study would show up as supporting the heritability of depression. It is well known in the social psychology literature that physical attractiveness is a powerful predictor of the responses of others. As long as these data were collected within a culture, the environment would not be "credited" with being an important variable in this hypothetical experiment. However, is this kind of finding really to be taken as evidence of an underlying disease process? The point of this example is to show that importing ontological assumptions and medical methodologies acts very much like a theory. By using and supporting the *DSM,* we implicitly agree to these assumptions.

What other implicit assumptions are we agreeing to? One is that a taxonomic approach that classifies on the basis of the topography of a problem presumably illuminates a common process. In fact the *DSM–IV* is inconsistent with its own ontology in this area. When discussing the polythetic approach used in the *DSM–IV,* Frances et al. stated:

> there are more than 100 different ways for the criteria of Borderline Personality Disorder to be met, and two patients may each have presentations that meet the criteria for Obsessive-Compulsive Disorder without sharing even a single criterion for the diagnosis. (1995, p. 19)

The result is that we get a heterogeneous group of people all called the same thing. Treatment outcome studies based on selecting subjects using DSM-like criteria consistently fail to show significantly large treatment differences that would help us understand etiology and inform treatment selection. Take, for example, the results of the NIMH Treatment of Depression Collaborative Research Program (Elkin, Parloff, Hadley, & Autry, 1985). The results of this multimillion dollar study suggest that it makes relatively little difference what treatment this well-studied group of individuals receive (Elkin et al., 1989). This is hardly a surprise. A syndromal classification system assumes that a depressive is a depressive is a depressive. However, there are several well-developed accounts for how depression might come about, (e.g., biological, behavioral, cognitive–behavioral, and interpersonal theories, etc.). If one assumed that depressive symptoms were one possible endpoint from a number of etiological pathways and that any group of persons with depression contained a number from each pathway, then comparative outcome studies are forever doomed to get equivalent results because those who might have had a biological cause might respond to medication but not those who were interpersonally unskilled, and so on. So far there is little evidence that there are common etiological pathways that describe a uniform course or response to treatment for any reasonable proportion of the *DSM–IV* categories. Even the notion of uniqueness of symptoms clustering to reveal an underlying problem finds little support. In the National Comorbidity Study (Kessler et al., 1994), over half of the participants who received one diagnosis over the course of a lifetime had at least one other diagnosable disorder as well.

In short, although there is little explicated theory apparent in the *DSM–IV*, the data on the ontological targets of research show little reason to remain enthusiastic about this type of research program. However, the *DSM–IV* and alternatives could fare better if they abandoned theory-neutral positions in favor of better delineated theoretically organized classification. We now turn to that issue.

Theory-Based Research Programs

We have traced some of the reasons why the modern *DSM*s have claimed to be atheoretical. We have further argued that this choice may be part of the reason why there has been little payoff from using the *DSM* to guide research. The obvious alternative is to develop more theory-based research programs.[3] The modern *DSM* movement has laid out a weakly stated medical model, but it is clear that stronger, more explicit statements are possible, and there are many who would like to see them made. For example, when defending the *DSM–III*, Klerman stated:

> In my opinion, the development of DSM–III represents a fateful point in the history of the American psychiatric profession. . . . The decision of the APA [American Psychiatric Association] first to develop DSM–III and then promulgate its use represents a significant reaffirmation on the part of American psychiatry to its medical identity and its commitment to scientific medicine. (p. 539, as cited in Kirk & Kutchins, 1992)

Klerman is hardly alone in his view of the relationship between the *DSM* and the medical model. Guze has been even more explicit about the goals and strategies for the DSM.

> Taken together [referring to the *DSM–III* and *DSM–III–R*] DSM–IV represents a major change in American psychiatry. The emphasis and attention to psychiatric diagnosis reflects a broad redirection toward the goal of reintegrating psychiatry into medicine generally. Diagnosis has been the foundation of medicine for centuries, and its renewed emphasis in psychiatry expresses the movement toward the medical model for psychiatric disorders. Those of us who believed strongly in the need for this change in direction and who participated in the emphasis on psychiatric diagnosis cannot help but feel satisfied at the results of our efforts. (1995, p. 1228)

Notice the unqualified endorsement of the medical model and the explicit statement of the goal of integrating psychiatry into medicine using the *DSM* as a primary vehicle for doing so. Guze even recasts the term mental disorder as "psychiatric disorders," a move Frances had said was indicative of a "turf conflict." However, a turf conflict is not what should be occurring. Instead, there should be an explicit statement of the theory that proponents of *DSM–IV* hold. That theory is not merely the medical model that Blashfield described, which is more about who should deliver services and how. What is coming more to the forefront is a biological model of behavior.

A biological model, if well explicated at a theory level, complete with explicit statements of ontology and epistemology, would allow for researchers and clinicians to more clearly determine whether they agreed with the model. A biological model would not be stated as a simple dualism.[4] It would be a monistic model where overt and covert behaviors are biological functions that an organism does. Issues arise out of important substantive questions having to do with the specifications of the particular assumptions of the model. Consider the following questions: In such a model, is a lesion or organic malfunction a necessary and sufficient cause of a mental disorder? Is it a malfunction or normal function when an environmental event alters the subsequent functioning of an organic part as some recent research suggests happens with trauma victims? Does such an alteration

[3] For those who are reading this article outside of the context of the special section, one should note that, although this article sets forth a critique of the *DSM* as an organizing taxonomy for guiding behavioral research, we recognize that criticizing in the absence of an alternative is no longer sufficient. Thus, this article, along with the introductory article to the special section, frames the critical issues, but six other articles also appear in this same section that offer specific examples of how alternative behaviorally based classification would lead to very different organizations and conceptualization of the current *DSM* syndromes. Although none of these offerings addresses all the concerns raised here, they indicate that a worthy competitor to *DSM* that is theoretically coherent could emerge. We refer the reader to these other articles rather than try to condense what an alternative system would look like into a space that would not allow us to adequately illustrate the differences.

[4] Because we are not proponents of a biological model for most forms of clinically relevant behavior, we do not presume to offer our presentation of the biological model as being of exactly the same form and substance as its more ardent proponents might present. We present this version of the model for discussion purposes. The model does not represent a "strawperson" argument because it possesses some interesting intellectual features.

imply that new historical experiences could not remediate any such alteration or does the level of intervention have to be biological? What is the status of mental disorders for which no dysfunctional part can be found? What is the interpretation of an environmental intervention when the overt manifestations of a mental disorder remit but the organic lesion or malfunction persists? One could go on. Notice that these questions invoke a complicated set of ontological assumptions, and the nature of the data to resolve these questions might be very different depending on whether you adhered to a biological model or not.

What is distressing about the way the *DSM* is progressing is that we believe what was once characterized as a medical model is becoming more of a biological model resembling what we have just described. The unfortunate thing is that this model is surreptitiously creeping into the position of being the theoretical foundation for the *DSM*. It has not "earned" this right on the basis of a priori specifiable results that would allow a dispassionate observer to evaluate the evidence to support or refute the degree to which the model has been successful. This medicalization phenomenon shows up in direct ways with the requirement for most Axis I disorders to determine that the problem is "not due to the direct physiologic effects of a substance (e.g., a drug of abuse, a medication) or a general medical condition." Nonphysicians cannot make this determination. With the latest iteration of the *DSM*, it will be interesting to see whether physicians will finally demand that only they can diagnose. Where are the data to suggest that this is a requirement that enhances the scientific utility of the *DSM–IV*? Was there an alarming rate of missing medical conditions that "caused" Axis I diagnoses? Was there evidence that persons who formally ran the diagnostic laboratory tests (of which few are sensitive and specific) produced better outcomes for their clients? The medicalization goes on. There are now a host of iatrogenic complications that are due to medications included in the *DSM–IV* section on criteria sets provided for further study. That a drug side effect is now being considered a mental disorder is interesting, but the process or principle by which these mental disorders emerge for study is bizarre. Our point is that the nature of evidence that would be used to evaluate these changes still seems to be up to the judgment of the *DSM* task force who, in an atheoretical or weakly stated theoretical system, cannot be adequately informed about what principle guides their decision making.

The *DSM–IV* is having a profound influence on the way behavioral science is conducted. The *DSM* system has become the de facto standard for defining what to study and how to report data. Of course, one can choose to ignore these categories, but review boards are rarely sympathetic to such attempts. One can study members of diagnostic categories who exhibit comorbidity with other diagnostic categories, but even this will often draw concerns that one is now confounding different syndromes and will not know how to generalize the results. This implies that syndromes have not just reliability but now validity as well and that it is an a priori assumption that it is important to keep research on clients with different syndromes separate.

The degree of influence of the *DSM* is way out of proportion with the science supporting it. From a scientific point of view, the *DSM*'s claim to fame is that it is now possible to produce reliable groups of clients for research purposes. However, the reliability of the *DSM–IV* is still to be established (see Spitzer, 1991, for a discussion of issues related to changing procedures for assessing reliability in the *DSM–IV*). In previous editions, reliability has been overstated (see Kirk & Kutchins, 1992). There is reasonable reliability when one identifies a participant as being a member of a large diagnostic category, such as schizophrenia, but falls significantly when one looks at lower level distinctions such as schizoaffective disorder.

In our view, the *DSM* is not progressing as a reasonable research program might. Categories proliferate in what appears to be the ad hoc manner that Popper and Lakatos suggest characterizes a degenerative research program. The organizing portion of the research program (*DSM*) does not account for more anomalous findings as it changes. It just grows. This is not the fault of the *DSM* task force as it has not been their goal to explain why the *DSM* grows at it does. However, the scientific research community has an obligation to ask what is happening here.

What Needs to Happen?

The *DSM* has done its work by abandoning an untestable psychoanalytically based taxonomy and emphasizing some desirable features of new classification systems, namely reliability. However, the *DSM–IV*, is undisciplined with respect to explicating its theoretical position, as we have said earlier. This makes it impossible for science to evaluate whether it is achieving its goal better or worse than any alternative. It is time for alternative classification schemes to emerge to compete with one another. One of the contenders ought to be a strongly articulated biological model, not a weakly stated medical model that is more about economic and social power than it is about relevant principles. Other contenders must emerge. Certainly, behavioral theories do exist as do cognitive theories of important behavioral problems. Anyone can and should participate in efforts to better account for important clinical problems.

The entry fee to participate in creating classification systems ought to entail a well-articulated theoretical position that describes the ontology and epistemology behind a theory. To avoid scientific relativism, it is desirable to have different theoretical positions agree on some common measures of what would constitute probative data for specifiable scientific goals. It should be possible to describe how to compare treatment utility and cost-effectiveness. This would not be a simple task, but a dialogue should begin to define the goals of various theory-based treatment programs. The cost of avoiding these problems is to have a monolithic research program, underwritten by pharmaceutical houses and government institutions without the means to allow for identifying when the program has degenerated. Alternative theory-based research programs must address how well their programs can be exported to other researchers. Thus, issues of reliability in participant selection, treatment delivery and fidelity, and outcome measurement must be thoughtfully addressed in any competing classification system.

In order for a competition of ideas to occur, the *DSM–IV* needs to be relegated to the status of "a," not "the" way of organizing scientific research. Funding agencies must be able to specify the criteria for the evaluation of research proposals in terms of goals more meaningful than simply following *DSM–IV* categorical guidelines. Failing to do so stifles intellectual competition when the track record of the *DSM* for identifying course, etiology, and response to treatment, its implicit goals, hardly justifies limiting competing ideas.

Behavioral scientists cannot continue to idly let scientific research proceed along its current lines without stopping to examine what is occurring. The ontological assumptions of a biological model need to be examined and challenged. Behavioral scientists have become too complacent with accepting the *DSM* structure because they can find ways to coexist with it. They have become comfortable recognizing (appropriately) that behavior is emitted from a biological organism functioning within an environment, without examining the emerging ontological position of the biological monists that seems to relegate external influences on behavior to a secondary level of importance. Behavioral, cognitive–behavioral, and systemic theorists have traditionally and successfully examined manipulable social, interpersonal, and cognitive variables as they interact with, and on, a whole organism. To fail to put forth a coherent research program that considers behavior (pathological or otherwise) as a situated act in context looses the psychological level of analysis. Biological theories are free to do so, but psychological theorists are unwise to let this happen.

It is important to allow research to be organized by theory and not require that syndromes be the focus of study (for a related discussion, see Persons, 1986). The study of syndromes presumes that there is an orderliness in nature that will manifest itself at an overt behavioral level. We have already doubted this supposition, and the framers of the *DSM–IV* share this skepticism. The admirable quest for interrater reliability has turned to behavioral frequency counts and duration to achieve its goal. However, there is too much evidence that behavior can occur for a wide variety of reasons, differ across cultures and settings, and function under multiple sources of control. To most behavioral scientists, this is almost axiomatic. Only to those looking for the most proximal biological causes of an overt behavior does it appear as if there is a single cause for a particular behavior. There is no doubt that there is a final causal biological pathway to explain "how" a behavior happens. Knowing the physiological pathway will not in any useful way explain how and why a particular behavioral event occurred apart from understanding the interaction between the organism and its environment. Except in the world Aldous Huxley described, a medication cure for a problem that is primarily outside the organism insulates society from having to fix a problematic environment.

Does this mean that we are espousing the elimination of the biological model? Of course not. Where there is a biological defect that is not a normal response to an environmental challenge, the biological model may prove useful. However, it is a value judgment as to whether one should medicalize depressive behavior in response to social isolation even if mood can be affected by medication. That debate is one of social values, not scientific truth.

Scientific knowledge could be aided if a clearly formed biological theory of behavioral problems were explicated in detail. It is likely that if the proponents of a biological model developed the model and a set of research premises that were not artificially softened so it could pass for being inoffensive, it would provide a better framework to guide research. We assume a central premise would be the identification of specific organic changes that accompany specific symptoms that are not generally a predictable response to controllable environmental stimuli. This definition, or whatever one those so inclined would agree on, should allow a more precise level of prediction about in whom and when a problem would emerge (etiology), how long the defect is present (course), and how intervention returns the organism to normal functioning (treatment).

Even those who subscribe to a biological model might do well to broaden their scope of inquiry beyond syndromes, because unlike physical medicine, we submit there are few identifiable behavioral homeostatic mechanisms to suggest that symptoms should cluster together. For example, one might expect a variety of behavioral manifestations from a serotonin dysfunction that might include depressive behavior, obsessive-compulsive behavior, suicidal behavior, sleep disturbance, and so on. If one started from the proposition that a biological theory would entail statements of how serotonin functions normally, one would predict a variety of possible influences that cross the current syndromal boundaries. The current boundaries constrain even a real biological model.

Does the biological model then have any primacy in explaining behavior? No. Any competing theory can be the organizing principle for research. Cognitive behavioral psychology makes predictions and provides interventions for a significant number of current syndromes. Cognitive theories argue that a variety of clinically relevant problems can be organized differently under a theory that says dysfunctional thoughts produce a variety of psychological problems that have a common etiology and response to treatment. This theory has been the basis for treatments of disorders that include depression, anxiety, personality disorders, marital distress, and substance abuse (Beck, 1988; Beck & Emery, 1977; Beck, Emery, & Greenberg, 1985; Beck et al., 1990, Beck, Rush, Shaw, & Emery, 1979). Again, a theory-based approach would lead one to pay less attention to syndromes and more to the identification of conditions where one would be expected to show problems and how to treat them rather than attending first and only to the topography of the problem. Other articles in this special section demonstrate how a behaviorally based theory would lead one to understanding symptom pictures much differently.

What prevents us from conducting theory-based testing now? To some degree the answer is that we can. It has been argued elsewhere that some improvements in outcome designs could yield more information than currently results from typical syndromally organized designs (Follette, 1995). However, simply adding theory-specific predictions of etiology and treatment response within an arbitrary syndrome unnecessarily complicates our taxonomy. If a taxonomy can be reduced by theory, it should be for parsimony's sake and because the explication of principles to explain problems allow us to see commonalities that might otherwise be obscured.

Why should we do this now when theory-driven classification has already been rejected by the authors of the *DSM–III*? There are two reasons. The first, we have addressed. By seeming atheoretical when there really is an implied model and method (e.g., the biological model), we weaken the likelihood of that model working optimally. Second, besides the political reasons for presenting an atheoretical disguised biological model document, the theory that had survived into the *DSM–II* was primarily psychoanalytic. At a practical level, Popper (1961) and Grunbaum (1984), although for different reasons, recognized that psychoanalytic theory failed to provide a basis for a scientific research program. Rather than selectively abandon psychoanalytic theory in the *DSM–III*, all theory was ostensibly removed. This was a big mistake.

The time has passed for science to be conducted in a theoretical vacuum. We have learned that some types of theories are not a reasonable basis on which to base a research program. Psychoanalytic theory had two faults. The first was that its adherents apparently offered it on the basis of an appeal to authority and a truth criterion of coherence in explanation to those who offered the theory. The second was that it was not developed so that it could be judged to be a progressive or degenerative research program (Lakatos, 1970). That was an important lesson. The scientific community can demand better characteristics of new pretenders to the throne, and it should. A biological theory of behavior should not invoke appeals to authority to gain recognition. It needs to develop its theoretical propositions so it can be evaluated against competing theories. Those who hold other theoretical positions must not be reluctant to offer competing theory-based research programs. Most of all, the scientific community must not allow any single scientific paradigm to go unchallenged. There is no example in science where a single account of a complex phenomenon has withstood the test of time. We have no doubt that a monistic biological understanding of human behavior needs to be challenged. One cannot look at the state of our science and argue that we have progressed so far since 1980 that we should embrace a monolithic approach to organizing scientific research.

Summary

In this article, we have argued that the *DSM*s since *DSM–III* have claimed to be largely atheoretical. This strategy seems to have been adopted to minimize opposition from those concerned about the medicalization of the mental health field. Although this course has facilitated the widespread adoption of the *DSM* as a taxonomic system, the lack of theory has led to the proliferation of diagnostic categories with poorly explicated guidelines for evaluating the rationale behind the decisions that affect the structure and criticism of the *DSM*. Many researchers and philosophers of science have stated the need for theory-driven taxonomies if research programs are to progress. We have further said that, in spite of the efforts to make the *DSM* seem atheoretical, it is clear that it entails ontological and epistemological assumptions that show it to be a weakly explicated medical model. Writings by those involved with shaping past and future versions of the *DSM* show that the system will ultimately move to embrace a biological model. This article argues that science would progress better if explicit theory-based models were offered openly rather than surreptitiously and that criteria for evaluating and comparing models should be clearly stated so ideas could compete fairly. This would mean that a strongly stated biological model should be put forth to compete with similarly stated behavior, cognitive, systemic, or other models. It would no doubt be difficult to compare the results of these efforts, but the task must be undertaken. Without theory-driven models to guide the interpretation of data, it is not likely that any empirical "truth" will emerge.

The interpretation of the process behind the evolution of the *DSM*s we describe in this article is offered to call attention to a social-scientific process that we believe is not in the best interest of the entire community of behavioral scientists including those who are biologically oriented. There is nothing in this article that will not allow the most useful approach to understanding behavior, abnormal or otherwise, to emerge. Thus, no useful idea is disadvantaged by our suggestions. What we have recommended will facilitate that process. The evaluation of the *DSM* can only reasonably occur when conceptual and theoretical problems it solves are balanced against those it produces (Laudan, 1977, p. 68). This is difficult to assess in the absence of a clearly stated ontology and epistemology. Assuming that the scientific goals of those who wrote the *DSM*s are to establish a foundation that leads to a progressive science, the competition of ideas should be welcome. The proponents of a particular theoretical position are not necessarily the best people to evaluate evidence counter to their position. Unless there are freely competing theories, an unnecessary stagnation of ideas can occur. Feyerabend has made this point.

> many facts become available only with the help of alternatives, [then] the refusal to consider them *will result in the elimination of potentially refuting facts as well* [italics in original]. More especially, it will eliminate facts whose discovery would show the complete and irreparable inadequacy of the theory. (1982, p. 42)

The complement to Feyerabend's quote is that attempts to refute existing theory may provide novel evidence that could support it as well. The current form of the *DSM* does not lend itself to adequate challenge. Intended or not, the function of the *DSM* has been, in part, to stifle noncategorical taxonomic systems. It seems as if many have grown complacent with the *DSM* or even accepted it rather uncritically. We hope this article will cause people to reconsider not asking more from ourselves and others in the scientific community.

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