References


Commentary by Joseph Zubin, Ph.D.*

It's a pleasure to comment on Professor Kendall's article since I have known him from the days when he served on the United States-United Kingdom team in our project to compare diagnosis in New York and London. I have long admired his penetrating thinking and incisive methods of analysis. His statements about the need for revision of DSM-III and ICD-9 are clear, quite persuasive, and demand attention. To capture the required attention, he follows Bertrand Russell's injunction that whatever is worth stating is worth overstating. I find myself in general agreement with his views except when the tendency to overstate creeps in.

His high hopes for "operational" definition and multiple axes I share but I believe he would agree they are now only hopes and whether hopeful sailing may be better than arriving in this case, only the future can tell. First of all, as has been pointed out by several observers, "operational" is hardly a suitable description of the Research Diagnostic Criteria. At best, they are arbitrary but useful specifications based on a tentative consensus, rather than on the operations one undertakes to identify or measure an entity or a concept. It might be better to call them consensus standardized criteria rather than operational criteria, ready to be overthrown by the first zephyr of demonstrable etiology or markers. Of course, in this respect, they are no worse off than some of our other beliefs and findings.

There is one underlying problem that Kendall is no doubt aware of but which does not surface in his article, the problem of developing a nosology for the marketplace of clinical practice as contrasted with one to meet the needs of research. As far as I understand it, even DSM-III is still market-oriented to some extent, with all the compromises that clinical exigencies and third-party payments involve. As is well known, the moment such extraneous nonscientific values enter the picture, science suffers. By the same token, ICD is even more practice-oriented, especially in the Third World countries. Is there room for a nosology that is free of market needs and truly research-oriented, one in which biological and social psychiatry, markers, and etiology can be free to roam? Perhaps DSM-IV should appear in two versions, practice-oriented and research-oriented. Then, the practice-oriented form can follow ICD-10, while the research-oriented form can serve the same function in psychiatry that private schools serve in the United States—as front-runners for innovations for the public schools. This research-oriented version can have special axes for treatment, evaluation, vulnerability markers, etiologic indicators, and so on.

Another problem that Kendall presents is perhaps unintended—the exclusive limitation of DSM and ICD to psychiatry, leaving no room for the other mental health professions. If that exclusiveness continues, there will indeed be a need for establishing criteria for how the different mental health professions should divide up the field and what mutual referrals should be made between them. A shy lad may very well be treated more satisfactorily by a counselor than by a psychiatrist. The fact that he refers himself or is brought to a psychiatrist makes him no more a psychiatric problem than a heart condition which lands in the lap of a gynecologist becomes a gynecologic problem. As has been said previously, mental health and illness are too important to be left to only one discipline. There is no more reason to confine classification in mental disorders to psychiatry than to limit classification in ichthyology to fishermen, in navigation to sea captains, and in agriculture science to farmers.

In general, there is no greater need for defining mental disorder rigorously than there is for defining physical disorder, and I do not know of any rigorous definition of the latter either. Both conditions have penumbras of doubt. Furthermore, rigorous definition is no more needed in psychiatry than in biology, for if it were needed in the latter, the definition of species would go by the board as Julian Huxley has pointed out (1).

The rather intriguing suggestion to define psychiatric diagnosis by what psychiatrists treat reminds one of the earlier definition of psychologic tests as tests that psychologists give. Perhaps, limiting psychiatry to major psychotic and neurotic disorders and leaving the problems of everyday living to the other mental health professions is
as good a solution as any, if a definition of psychiatric disorders is required.

Another point that may have been overstated is the distinction between etiology and markers. Looking for the Holy Grail of etiology is virtuous but searching for nonetiological yet identifying markers seems to be either taboo or not as desirable. I see no justice in this. Why should we neglect such identifying characteristics as evoked potentials, pupillography, smooth pursuit eye movements, crossover indexes and cross-modality indexes in reaction time, span of attention, continuous task performance, and so forth, some of which have already been found to characterize schizophrenic probands as well as unaffected siblings and relatives? The fact that they are not pathophysiologic or have no direct etiologic connection does not make them less capable of serving to identify the presence of schizophrenia or vulnerability to schizophrenia.

Although it is true that there are no royal roads to finding etiology or markers, there is a possibility that information processing paradigms may serve as a comprehensive strategy for discovering markers. Because much of psychopathology deals with deviation in behavior, which presumably has a stimulus of some sort, investigation of the way psychiatric patients process incoming information from external or internal sources may reveal just where and when in the sequence of the processing the deviations take place (2,3). Depending on long-term prognosis as a criterion for differentiating diagnostic categories or on response to specific treatment as another criterion, although certainly useful, may also be overstated. With so many ecological, nosocomial, and iatrogenic factors involved in determining outcome, prognosis can hardly serve as a basic factor in the differentiation of diagnostic categories. As for response to specific therapies, the example of Stockings' proposal (4) to utilize this criterion to differentiate between dyslygolic (response to insulin) and dysoxic (response to ECT) ought to give us pause.

The disappointment in the accomplishments of multivariate statistics is another overstatement. No one would disagree with the statement that thus far factor analysis and discriminant functions have not yielded new insights, though typology probably has been more fruitful. However, one must remember that statistical treatment belongs to the realm of verification, not to the realm of discovery. Lithium was not discovered by a statistician, neither were the neuroleptic drugs, nor the antidepressant agents, nor behavior modification. However, verifying their usefulness through investigation in which many variables have to be considered is a task best left to the very technique that Kendall suggests has not paid off. Furthermore, if the causes of mental disorder are indeed multiple, dismissing multivariate statistics because they have not yet, or not always, paid off or because they have been misused is like eliminating the stars lest they be worshipped by idolators. Although statistical techniques are essentially for verification rather than for discovery, it must be pointed out that in arriving at discoveries a certain degree of statistical, even multivariate, strategies is involved albeit informally. Thus, the discovery of Neptune was guided by certain statistical—mathematical considerations in the mind of Leverrier (1846) even though he may not have used computers or formal statistical techniques. Perhaps only in such well-developed fields as astronomy can one hope for discoveries guided by statistics and until psychopathology develops further, Kendall may be right in his judgment of the value of statistics. But it must be remembered that the realm of discovery consists of options and possibilities that have to be weighed by probabilistic approaches, formally or informally. The scientist's function may be epitomized as changing a possibility to a probability and whether it is done informally or formally through statistics makes little difference for the outcome.

In summary, I have nothing but admiration for Kendell's admonitions to the architects of DSM-IV and ICD-10, but I do have certain reservations on his requirements for rigorous definitions of mental disorder, his critique of markers and multivariate statistics, and depending so much on long-term prognosis and treatment outcomes as validation criteria for diagnostic categories. I also find the exclusion of nonpsychiatric mental health professions, or at least their not being considered, as an unnecessary limitation. Perhaps the most stimulating effect of being permitted to review this article resulted for me in the suggestion that there be two versions of nosology—one practice-oriented and one research-oriented. As a matter of fact, the research version is actually already in existence, because one of the functions of research is to probe the boundaries of the proposed categories and in the process create new categories, and this goes on against the vague boundaries of DSM-III.

What is being proposed by Kendall is a more formal recognition of the current state of affairs and as such it presents a platform on which future developments can be built.

I believe that the other axes besides Axis I require further development. They will need to be
provided with well-developed interviewing methods and measuring instruments to permit investigation of their reliability and validity. Perhaps, too, new axes such as a vulnerability marker axis, an episode marker axis, and a residual marker axis may be needed to integrate the newly developing knowledge in these areas.

References

Commentary by Michael Shepherd, D.M., F.R.C.P., F.R.C. Psych., D.P.M.*

This rather discursive essay touches on three separate but related topics: the medical politics of psychiatric classification, the substance of the International Classification of Diseases (ICD) and the Diagnostic and Statistical Manual (DSM), and the attainment of a more satisfactory nosology of mental disorders.

On the first issue there is little more to be said. It has generated more heat than light, underlining in the process the need for an internationally acceptable classificatory schema. The DSM is certainly no substitute for the ICD. Kendell himself, along with experts from several countries, has pointed to the many weaknesses of its design and content in a recent publication (1).

In drawing up ICD-10, however, the World Health Organization (WHO) is unlikely to accept Kendell’s contention that psychiatric glossaries are not classifications of disorders so much as classifications of the problems psychiatrists are consulted about, for its major concern will surely be with mental disorder encountered by the medical and other caring professions outside the mental health services. Studies conducted 25 years ago by the Royal College of General Practitioners in Britain first showed that about half the problems brought to primary care doctors could not be given a satisfactory diagnostic label within the framework of the ICD, a finding that has already led to an adaptation of the ICD for use in general medicine (2). Within a public health perspective it is the large number of such disorders, most of them now allocated to the dustbin Section XVI, “Symptoms, Signs and III-defined Conditions,” which call most urgently for attention.

Here, rather than the definition of mental disorders and the place of the V code, is the immediate challenge ahead, but it will not be met by adopting the standpoint from which Kendell appears to base his assumptions of future progress. The imminence of a quantum leap in our understanding of the workings of the human brain; the claim that the identification of “underlying” (treacherous word!) biological abnormalities would mean that “clinical dilemmas and controversies would be solved, or rendered irrelevant, overnight”; the assertion that etiologic research lies mainly in the hands of laboratory scientists who will have mastered the intricacies of cerebral mechanisms within two generations at most—all this bespeaks a naive reductionism and an unrealistic expectation of what can be achieved in biological psychiatry (3). The nosologic difficulties still confronting those branches of medicine to which the basic sciences have been applied more successfully than in psychiatry argue in favor of scientific caution in classifying the mental disorders.

References

Commentary by William T. Carpenter, Jr., M.D.*

Kendell’s article is informative, and in general I agree with his remarks. For innovations in diagnosis to find widespread applica-

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tion, it was necessary for a strong national organization to take the lead, and the APA can be proud of its achievement. We now need to be responsive to Kendell’s call for a return to the international fold, both to enable the major accomplishments of DSM-III to come to fruition in an international framework and to regain the advantages of a shared international nomenclature. Now that major innovations have been introduced, resistance to future developments should be lessened, and the atheoretical nature of DSM-III should facilitate