


**Acknowledgments**

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**Comments on Blashfield’s Article**

Since Roger Blashfield was candid enough to reveal that he had unsuccessfully submitted this manuscript to five other journals before it was accepted by *Schizophrenia Bulletin*, I want to note, at the beginning, that I had been asked to review the manuscript three times, including the last time by *Schizophrenia Bulletin*, and that I had recommended acceptance each time. I think I did so for several reasons. He had made an interesting observation (there are never too many of these); it was flattering and reassuring to see documented our own impression about the impact of our article and our work; and it might reinforce such impact by making it explicit and objective.

At the same time, I had a vague sense of uneasiness: Would Blashfield’s article lead to some backlash from the field? Would it stimulate an organized effort against the importance of diagnosis in psychiatry on the part of many who have never accepted the premises underlying the medical model for psychiatric disorders? Would this backlash focus on the sociological aspects of the phenomenon (what Blashfield refers
to as noncognitive features) as a means of trying to undercut the validity of what we have been trying to do instead of dealing with the scientific or "cognitive" aspects? Despite these misgivings, I voted for publication, partly in obedience to my sense of intellectual integrity and partly out of curiosity to see what the field would make of his article.

As a sociological observation, his article is tantalizing and somewhat frustrating, because it is ahistorical and because it ignores the broad sociological context of the field at the time our article was written and published. Blashfield has nothing to say about the state of American psychiatry during the 1950s, 1960s, and early 1970s. By not tracing the drift of American psychiatry away from its medical base during the 25 years before our article's publication, and by not relating our ideas to those in ascendency at the time, he runs the risk of creating the impression, though he warns against it, that our article arose out of some secret cabal or conspiracy designed to establish our ascendency.

A sociological analysis may be interesting and legitimate, but it should certainly try to consider major relevant sociological forces if it is to achieve validity. A sociological study of an idea or viewpoint, to be most effective, should relate the idea or viewpoint to others that are connected historically and contemporaneously. Otherwise, a "noncognitive" analysis will, in the final analysis, be no more than a footnote.

Perhaps of greater moment is Blashfield's decision not to deal with the "cognitive (internal, scientific) force" of our article. After reviewing the evidence, he concluded that the impact of the article was not explained by the status of the journal in which it appeared, by the reputation of its authors, or by the fact that it was a methodologic article. These were the only hypotheses he tested. He nowhere suggests that the impact of the article may have resulted from the fact that it met an important scientific need at a time when American psychiatry was slowly and imperfectly groping its way back to medicine. By deliberately choosing not to deal with any question of scientific substance, Blashfield appears to build a major edifice on a single, ultimately flimsy support.

The scientific question—and finally the sociological question—was and is: Can we have a scientific psychiatry without a serious, systematic effort to develop a valid classification of psychopathology? Our answer was and is: No! We proposed a new beginning in a discipline that for a generation had rejected our premise. The impact of our article, in my opinion, reflects only that psychiatry wants to reidentify with its medical base, by committing itself anew to that quintessential medical activity: diagnosing illness. Physicians have always recognized that diagnosis is not enough, but they have also intuitively and explicitly recognized that diagnosis is the keynote of medical practice and clinical research. We proposed that the situation is not different in psychiatry, and many colleagues now agree.

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Describing the history of diagnostic systems, Menninger (1963) coined the phrase “the epidemiology of taxonomy” to suggest the processes by which new concepts spread. In this report, Blashfield effectively describes a current manifestation of these processes. Such an attempt to understand factors that determine the development and extension of concepts in psychiatry is extremely important for the perspective it provides on the way we carry out practice and research. Analogous issues are involved in considering support or attack of the “medical model,” and other key concepts.

A major issue raised by Blashfield’s report is very similar to a question repeatedly discussed in the field of history: Is it the person, or in this instance, the persons, who generate a situation, or is it the times more generally? Tracing the recent shifts in American diagnostic practice, Blashfield makes an impressive argument for the role of persons involved, their positions, and their productivity. These seem important in providing considerable impetus for the acceptance of our current descriptive diagnostic concepts.

But such concepts exist in a temporal context, and it is equally important in understanding their development to consider this context. If, as has been suggested, science develops in cycles and is mediated to some extent by geographic or academic “schools,” then one can see any shift (or paradigm shift) as reflecting a segment of such a cycle. Thus, Louis’ 19th century contribution in promoting a descriptive approach to tuberculosis contrasted with the previous orientation toward speculative theorizing, and Claude Bernard’s similar approach to general medicine around the same time had a comparable role. Both efforts appear to reflect recurring shifts similar to the recurring shifts in the opposite direction. Such opposite shifts are reflected by the periodic flowering of theoretical structures focusing on underlying processes (e.g., those by Köhler, Piaget, Freud) in order to progress beyond “blind empiricism.”

A problem with such theoretical structures has been the tendency to expand them far beyond their empirical base, and then, sometimes using concepts that cannot be tested, to dominate practice, research, education, financial resources, and prestige. But the same can be said for more purely descriptive approaches. These have been accused of being sterile, rarely progressing beyond counting and describing, and coming to dominate their era far beyond what is empirically justified. Of course, even descriptive approaches are more theoretical than is often claimed, since crucial assumptions are involved—often implicitly—about the nature of acceptable data (e.g., excluding vague feelings) and how the collecting and clustering of data should take place. If, for example, one reads Lara Jefferson’s (1964) description of what it is like to be insane, and then Kraepelin’s textbook accounts of psychoses, it appears that Kraepelin almost totally ignores the characteristics of psychotic disorder as someone like Lara Jefferson experiences them.

When the cycles between descriptive and theoretical emphasis are recognized, a particularly important contribution of Blashfield’s report is that it suggests one way in which the pendulum swing begins and then develops the potential for becoming extreme. This extreme swing tends to disqualify first the preceding orientation and then, ironically, to disqualify itself as it becomes accepted as the complete solution and is ultimately challenged. Although such swings from one extreme to the other may or may not reflect an optimal way for science to progress, one of their serious disadvantages is that they inhibit the synthesis of the orientations involved. If, as seems most likely to me, both descriptive and theoretical, process-oriented approaches are essential but incomplete in themselves, the swinging pendulum makes especially difficult any attempts at synthesis that might best reflect the entire condition to be studied.

Blashfield’s article makes a useful point, but a more complete picture of the situation in my opinion needs also to include the hunger that the field has had to proceed beyond the vagueness and pseudo-communication that marked psychiatric diagnosis in the 1950s and 1960s, a vagueness especially severe in the United States. Such limitations in diagnostic concepts and reliability severely impeded systematic research, or even learning. Although American psychiatry during that period made major contributions in psychological, developmental, and humanistic approaches for even the most severe types of psychopathology (led by Meyer, Sullivan, Fromm-Reichmann, Menninger, and many others), often we had seriously distanced ourselves from a more testable empirical base. Thus, what Blashfield calls a neo-Kraepelinian tradition appears to reflect the pendulum swing away from an extreme in the opposite.
direction. In fact we may now have gone even further than Kraepelin, who employed such concepts as the "will," which we have shunned, apparently because of their imprecision.

The question, it seems to me, is can we have both? Can we maintain an active respect and investment in descriptive criteria and reliable data, while including an equal respect for the subjective experiences of psychiatric disorder, for theory and postulated underlying processes? Failure to use both orientations is perhaps the most unscientific approach of all.

Ignoring the range of data that appears to be relevant to our field or denying that we have conceptual structures which select and organize our perception, learning, and teaching seems a most inadequate reflection of reality. "Colleges" do seem to exist and to have an important effect, but they exist within a context, and to deal with this context, we must recognize its vicissitudes over time.

References


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It is always useful to have the attention of the field called to the less obvious forces which generate a particular direction in science. As Blashfield points out, there may be strong scientific bases for the influence of the article by Feighner et al., but there are, also, other sociologic forces at work which serve to increase its popularity and its palatability to the field at large. The article did not, after all, present a major scientific discovery, a "breakthrough"; it did not even report a new innovative method which could be expected to open the doors of discovery. The Feighner criteria are, in the simplest of terms, a set of definitions of already accepted concepts which are "operational" in nature, and which permit common language and common criteria to be applied in the description and classification of patients and their disorders. As Blashfield points out, such systems had already been developed by others—systems which appeared to be further along in their development and whose reliability had already been tested; yet, these systems received little attention or application. The Feighner system has benefited from the fact that it came out of a traditional and increasingly resurgent school of psychiatry, the neo-Kraepelinian, that it was backed by a respected, highly influential university department of psychiatry, and that it was published in the most prestigious of psychiatric research journals.

There are other reasons, scientific and sociologic, which Blashfield overlooks, which may underlie differences in the influence of various research efforts in the field of classification, and they are worth noting:

- The Feighner criteria came along at just the right time.

Psychobiologic research, which had had new life breathed into it by the introduction of the major tranquilizers in the 1950s, was in serious need of objective, scientific classification systems for the mental disorders. The standards in this field were increasingly being raised by the influence of the new scientific community in psychiatry, by the National Institute of Mental Health (NIMH), and by the psychiatric research journals, particularly the Archives of General Psychiatry and the Journal of Psychiatric Research.

- NIMH placed increased emphasis in the early 1960s on the need to improve methodology generally, in the field of clinical research, but particularly in its program in psychopharmacology (Cole and Gerard 1959). This meant improved methods for the "primary prerequisites" (Kety 1968) in the development of a science, i.e., description and classification, and, secondly, for the evaluation of changes in psychopathology. This increased attention began with an early influential conference on psychopharmacology (Cole and Gerard 1959) and continued for the field of clinical research generally (Katz, Cole, and Barton 1968), and later specifically for the affective disorders (Williams, Katz, and Shield 1972).

The field of psychiatric research is actually much smaller than it appears. Most of the sound work in progress is supported through research grants from the NIMH. The scientific groups and the Institute staff that monitor the award of these grants require that the methodology in these areas be sound. Investigators have had to strengthen these basic aspects of their pro-
that are not easily accessible to psychiatrists. Psychologists are also less influenced generally by what appears in psychiatric journals. By the same token, clinical psychiatrists tend to be uncomfortable with the kind of quantitative approaches developed by psychologists for what they see as essentially "judgmental" problems. They tend to believe that diagnosis is a "psychiatric" not a "psychological" problem, and that psychiatrists are in a better position, through experience and perspective, to solve it. This, again, is one of those social rather than scientific issues to which Blashfield alludes.

Finally, it is worth commenting on the most complex of the issues surrounding classification: the potentially conflicting purposes and interests of scientists and clinicians in developing such systems. It is too often assumed that the purposes for which classification systems are developed are the same for clinicians, for scientists in psychopathology, and for epidemiologists. Morton Kramer, the former head of Biometrics at NIMH, has been eloquent on the confusion created by this assumption, and one has only to review the separate perspectives of the various disciplines as presented in a section of the Report and Methodology in Classification in Psychiatry and Psychopathology (Katz, Cole, and Baron 1968) to appreciate the size and significance of the gaps in their conceptualization of the issues of classification and diagnosis. The clinicians, for example, need a system which will communicate easily, and which will assist in predicting the results of treatment. Scientists see classification as an ultimate objective in which the "natural" order is represented and, in the case of psychopathology, as based primarily on etiology. These different objectives result in different values as to which studies and which evidence are worth considering. All these perspectives are obviously important.

Despite these conflicting elements, there have been major developments in this field over the past 25 years; new evidence and new systems have been produced in research from several of the disciplines. Perhaps it is time again to go beyond the assembling of committees to discuss the relative values of DSM-III and ICD-9, Feighner and the RDC, the Present State Examination (Wing, Cooper, and Sartorius 1974) and the SADS, and to examine the accumulating evidence on the nature of the depressive disorders and of schizophrenia, psychological and biological, with a view toward developing more objectively grounded empirical systems of classification.

We should be somewhat beyond the development of the RDC phase, the "operational definitions" in this field, and be able to review new evidence in the service of creating the most effective systems for these various objectives.

It may be time to rise above the social issues, to reexamine the problem in a more scholarly context, possibly a new NIMH sponsored conference on "the state of research on classification in psychopathology."

References

Roger Blashfield's concern is focused on the frequent citation of the 1972 paper by Feighner, Robins, Guze et al., which he sees partly as cause and partly as effect of the rise to power of a "neo-Kraepelinian" school of psychiatry based on a St. Louis-New York axis. His stance seems to me to fall uneasily between that of disinterested commentator and unsuccessful competitor. The former tries to offer a detached commentary on the social process by which a group of like-minded scientists achieves a position of dominance, but the latter cannot quite conceal his resentment at being on the losing end of the "Matthew effect" he describes.

From an imperfect vantage point on the far side of the Atlantic, it seems to me that, although Blashfield's factual account is quite accurate so far as it goes, his analysis of why the influence of the St. Louis school increased so dramatically in the 1970s is inadequate and incomplete. Eli Robins and his colleagues in St. Louis had been energetically pursuing essentially the same type of research for many years before 1972, and had been almost totally ignored, at least in North America. Nor were they themselves responsible for the revolution that brought them to power. As Thomas Kuhn has emphasized, scientific revolutions are always preceded by a crisis; an established paradigm is only replaced by a new one because it is manifestly failing in some crucial respect. Psychiatry's crisis was caused by the repeated demonstration that the reliability of psychiatric diagnoses was horrifyingly low, that key terms like schizophrenia were being used imprecisely and indiscriminately, and that in consequence fruitful research was seriously handicapped. The need for operational definitions of diagnostic terms had been pointed out by Carl Hempel as long ago as 1961. Members of the St. Louis school were the first to put his advice into practice, partly because it could readily be incorporated into their existing research tradition, and partly because they had the energy to do what other people like myself were merely advocating. Things rapidly gathered momentum from that time on for a variety of reasons, of which a convergence of interests between Spitzer's group in New York and Eli Robins' protégés was only one. Klerman at ADAMHA must have had a considerable influence on the direction and scale of government research funding in the 1970s, and his lineage was Yale and Harvard rather than St. Louis or New York. Other workers like Mendels in Philadelphia and Abrams and Taylor at Stonybrook were also part of the movement despite having no prior allegiance to either Washington University or the New York State Psychiatric Institute. In this context there is nothing very surprising about the large number of citations the Feighner article obtained. As Blashfield himself says, methodological articles tend to be cited more frequently than those concerned with research findings if they describe an important technical innovation, and I imagine the Rorschach test, the MMPI, the
Hamilton Rating Scale, and the EPI all still show a healthy lead over Feighner’s article.

The “invisible college” phenomenon Blashfield describes is, as he says, characteristic of the way scientific movements arise and develop. It has obvious utility and is familiar to all of us, if not under that title. The “Matthew effect” to which he also refers is more contentious because it implies, without openly stating, that those who are not members of the dominant school are discriminated against in the competition for research funds and publication in prestigious journals. It would be idle to pretend that personal considerations never influence the decisions of editors or grant-giving bodies, but intellectual considerations are probably more important than personal rivalries in favoring those who subscribe to the dominant paradigm. Those whose ideas are out of tune with the spirit of the times are always at a disadvantage. I would guess that on the whole the effects are beneficial, though undoubtedly men who are ahead of their time get less than their due as a result. Indeed, for a long time this is what happened to Eli Robins and his colleagues.

Those who are out of sympathy with the aims and philosophy of the neo-Kraepelinian school can console themselves with two thoughts. Its present dominance will not last forever, or even for long if it commits itself to a crude belief in discrete disease entities and ignores the effects of social and psychological influences. And if their own research is really important, it will not languish forever in minor journals. Like the Abbé Mendel’s genetic researches, it will eventually be recognized. I hope, though, that it will be possible for everyone with an interest in clinical research to accept the need for operational definitions of diagnostic and other key technical terms without feeling that by doing so they are subscribing to an alien ideology. For my own part I regard the publication of the Feighner criteria as a major technical advance, but that does not mean that I regard myself as a “neo-Kraepelinian,” or subscribe to all of the nine tenets to which Blashfield refers.

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