Letters

Psychologists and Psychiatrists as Expert Witnesses

On behalf of the 90,000 members of the American Psychological Association (APA), we are writing with regard to the article "The expert witness in psychology and psychiatry" by David Faust and Jay Ziskin (1 July, p. 31). In particular, the APA is concerned that this article is organized and presented in a manner that leads to the overly narrow conclusion that the courts should consider excluding psychologists and psychiatrists as expert witnesses.

The difficulties inherent in providing clinical information for use in the courtroom are not new. In fact, they have been made apparent, in large part, as a result of the research-based, self-scrutiny that psychologists have characteristically applied to their clinical activities. The fact that this research indicates that clinical judgments are not as valuable for answering legal questions as we would like does not mean that they are of no value. To draw such an absolute conclusion is akin to "throwing the baby out with the bathwater."

Perhaps the most crucial issue creating difficulties when mental health professionals provide expert witness testimony (apparently overlooked by Faust and Ziskin) is the basic distinction between how clinical questions are answered and how legal questions are answered. Answers to clinical questions take into account as many of the complex factors as possible that have an impact on an individual's behavior and provide as complete a picture of that behavior as possible. Legal questions, on the other hand, must be answered in an absolute ("yes" or "no") fashion. The problem is created when one attempts to transform complex clinical answers into legal answers that require simple absolutes. The fact that this process may be difficult and problematic is not grounds for avoiding it altogether. In fact, the adversarial system is designed to do this very task. Furthermore, the "battle of experts" referred to by Faust and Ziskin is inherent in the adversarial process and can be found any time a legal question requires nonlegal information from experts.

The article covers much of the same ground as has appeared in previous reviews (*1*) and disregards a wide body of more recent research that demonstrates acceptably high reliability for many psychological and psychiatric diagnoses (*2*). Instead it relies heavily on research relevant to diagnostic classification and prediction of violence to demonstrate the difficulties inherent in making clinical judgments. Yet these specific activities are only two isolated components of a total body of knowledge that can be of value to judicial decision-making. They by no means comprise the broad range of complex judgments that clinicians make in daily practice or that they can offer to aid the court in understanding the evidence, determining a fact at issue, and ultimately making legal decisions.

Diagnostic classification alone is rarely, if ever, the basis on which legal determinations are made. Such legal questions as insanity, disability, and competency are based more on the judge's or jury's understanding of an individual's behavior and ability to function in specific situations than on what specific diagnosis is assigned to that individual. Legal determinations of insanity, for example, may be made in cases where a wide variety of diagnoses are found, including dissociative disorders, impulse control disorders, mood disorders, and schizophrenia.

With respect to the prediction of violent behavior, psychologists and psychiatrists alike have typically been reluctant to give predictive testimony. However, the U.S. Supreme Court, in the case of *Barefoot v. Estelle* (463 U.S. 880, 1983), endorsed the use of expert testimony in predicting future violence. The court recognized what it termed "shortcomings" of these predictions, but reasoned that the fact-finder and the adversary system would be competent to take account of its shortcomings.

Clearly, expert witness testimony should not be used to make assertions beyond the limits of the expert's knowledge and ability. But to argue for a cautious approach to the use of expert witness testimony can not be equated with arguing for the elimination of experts in the courtroom.

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1. J. D. Matarazzo, in *Handbook of Clinical Psychology*, B. B. Wolman, Ed. (McGraw-Hill, New York, 1965), pp. 403–450; in *Clinical Diagnosis of Mental Disorders: A Handbook*, B. B. Wolman, Ed. (Plenum, New York, 1978), pp. 47–96.
2. J. D. Matarazzo, *Clin. Psychol. Rev.* **3**, 103 (1983).

Response: Fowler and Matarazzo purport to write on behalf of the entire American Psychological Association membership, as if there were unanimity. However, they are not writing on behalf of the many psychologists like ourselves who have criticized the role of mental health experts in the courtroom and who also belong to the APA.

Fowler and Matarazzo suggest that our focus on diagnosis and prediction of violence was narrow and potentially misleading. However, the article was by necessity a synopsis, with limited examples, of a massive and wide-ranging body of literature. *Science* could hardly permit us to reprint our 2000-page text and its nearly 1400 citations that comprehensively document the limits of the expert witness in psychology and psychiatry (*1*). Many other clinical judgment tasks pertinent to courtroom activities, such as the capacity to detect simulated brain damage or psychosis, determine past mental states, identify the more suitable parent in custody disputes, or achieve valid conclusions by integrating complex clinical data also lack supportive scientific evidence or face considerable negative literature. Indeed, although Fowler and Matarazzo describe complex clinical judgment as an exception to the limitations we detail, there are few areas in the social sciences in which the research is so consistent in demonstrating otherwise. Many studies show that the integration of even a few variables may outstrip clinical judgment capacities or that exceedingly simple actuarial methods equal or exceed the accuracy of clinicians attempting these feats (*2*).

Fowler and Matarazzo state that we omitted more current research on such topics as diagnostic reliability and counter with Matarazzo's 1983 article. Twenty of the citations in our article, in fact, were as recent as or more recent than this Matarazzo citation. Further, Matarazzo's article was written when initial research, some of it promising, appeared on the third revision of the diagnostic manual. However, a number of later, more up-to-date studies produced negative findings and thus raised serious doubts about reliability (*3*). Nor do Fowler and Matarazzo mention that, subsequent to Matarazzo's review, the diagnostic manual has been revised once again, in the absence of formal reliability studies, leaving the profession to guess about the effects of these changes and the applicability of prior research.

In research on classification and in many other areas, there may be some positive literature, but when so much negative literature exists simultaneously there remains, at best, substantial doubt about the issues studied. If professionals cannot resolve their own controversies on the most basic issues and principles, how can the jury be expected to do so? Courts serve to settle legal, not professional, disputes.

We agree that expert testimony should not exceed knowledge or ability, but how can we determine what these abilities and limits might be? Unsubstantiated declarations are no substitute for empirical tests. Fowler and Matarazzo are aware of the current state of knowledge on psychological assessment, as their writings in scientific journals show.

Matarazzo has stated elsewhere that "clinical psychology [much less forensic psychology] is still an art based on some scientific background" (*4*, p. 20) and that "psychological assessment is currently almost exclusively . . . still-to-be-well-validated" (*4*, p. 20). He adds that computerized psychological test interpretations (which may be used as legal evidence) have not met "even the most primitive scientific tests of validation" (*4*). Fowler responds (*5*) that the "clinical report [which summarizes clinical judgments and opinions] . . . has itself rarely been subject to the scrutiny of validation studies. . . . Studies of clinician-generated 'interpretations' have yielded unimpressive results." Matarazzo responds that there is "no evidence" that clinical reports are any more valid than computerized interpretations (*6*). Matarazzo's statements logically imply that neither computerized interpretations or clinical interpretations have met even "primitive" tests of validation.

In their role as spokespersons for the APA, Fowler and Matarazzo imply that their articulated standard for courtroom testimony—that it should not exceed knowledge and ability—does not preclude a substantial role for the psychologist. Their journal statements, however, imply the opposite conclusion; but perhaps this inconsistency is emblematic of forensic psychology—professional interests and scientific data may create incompatible positions. For how can Fowler and Matarazzo's scientific assertions that psychological assessment is "almost exclusively . . . still-to-be-well-validated," has yielded "unimpressive" scientific results, and has not surpassed the most "primitive" tests of validation support their professional assertions that psychologists can aid in legal matters?

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3. H. Kutchins and S. A. Kirk, *Social Work Res. Abstr. 3* (1986).
4. J. D. Matarazzo, *Am. Psychol.* **41**, 14 (1986).
5. R. D. Fowler and J. N. Butcher, *ibid.*, p. 94.
6. J. D. Matarazzo, *ibid.*, p. 96.

IIASA's Credibility

David Dickson, in his article about new financing for the International Institute for Applied Systems Analysis (IIASA) (News & Comment, 15 July, p. 285), notes that IIASA needs to "develop its intellectual credibility." He does not sufficiently explain why. The trouble lies with the very notion of "systems analysis" in the sense adopted from the beginning by IIASA. This notion has little or nothing to do with real physical systems (such as the Chernobyl reactor mentioned by Dickson); it grows instead out of the grandiloquent and now largely discredited studies by Forrester and Meadows on "limits to growth" (*1*) (sometimes parodied as "models of doom").

As carried out by IIASA, this type of "systems analysis" consists of the construction of massive imaginary future "scenarios" with elaborate equations for quantitative "models" which combine to provide predictions or projections (gloomy or otherwise), but which cannot be verified by checking against objective facts. Instead, IIASA studies often proceed by combining in series a number of such unverified models, feeding the output of one such model as input into another equally unverified model. This procedure is illustrated in the IIASA study of world agriculture and in the massive IIASA "global systems analysis" of energy (*2*). Such studies as these are speculations without empirical check and so cannot count as science.

It is indeed the case that IIASA is supported by the Soviet Union. This may result in part from the somewhat naïve Soviet enthusiasm for what is called "cybernetics."

In view of all this, the U.S. support of IIASA has been mistaken from the beginning. At first, this support was provided by the government through the National Academy of Sciences (in the NAS council, I argued against this step). Subsequently, private U.S. support was provided through the American Academy of Arts and Sciences (I again argued in the council of that academy against this step). It is unfortunate that U.S. support for IIASA, to the tune of $450,000, is now to be provided by the National Science Foundation. The current efforts at IIASA may be "state of the art" (as suggested by Dickson), but the "art" in question involves no real element of science.

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1. D. L. Meadows *et al. The Limits to Growth* (Universe, New York, 1972).
2. Energy Systems Program Group, IIASA, *Energy in a Finite World*, W. Hafele, Program Leader (Ballinger, Cambridge, MA, 1981).

BEIR IV Report

A calculational error occurred in the estimates of the risk of lung cancer resulting from radon for smokers and nonsmokers in the recently published BEIR IV report (*1*). The modifying term for age at risk was inadvertently omitted in the program used to prepare table 2-4 in chapter 2 and tables VII-12 through VII-23 in appendix VII. When this term is included, the estimated risks for lifetime exposure decline. For example, the corrected estimate of lifetime risk at one working level month per year is about 20% smaller for smokers and about 25% smaller for nonsmokers. Therefore, the risk to exposed smokers relative to similarly exposed nonsmokers becomes slightly larger, 10.5 vis-à-vis 10.1. The results for males and females in the general population (without regard to smoking status) presented in chapter 2 of the report are not affected by this error.

Corrected tables for smokers and nonsmokers of each sex can be obtained from the Board on Radiation Effects Research at the address below. We thank Fanny K. Ennever of Case-Western Reserve University for bringing this error to our attention.

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1. *Health Risks of Radon and Other Internally Deposited Alpha Particle Emitters: BEIR IV* (National Academy Press, Washington, DC, 1988).

Erratum: In the Research News article by Richard A. Kerr "In search of elusive little comets" (10 June, p. 1403), the position held by John Craven of the University of Iowa was incorrectly given. He is a senior research physicist.