Bivens recognize that the scientific community has a role in determining where responsibilities are properly vested. We will no doubt need continual discussion to ensure that the proper lines are maintained. Because of the intensely personal nature of scientific research, and because students learn standards from the behavior of their preceptors and colleagues, just as children do from their parents and their other contacts, the discussion will always face an ancient and general problem: where should the law end and personal morality begin in setting standards of conduct?

In justifying their position, Mason and Bivens note that "scientific investigative panels" have judged misconduct to include a range of unacceptable behaviors beyond falsification and plagiarism. To my knowledge, the most prominent support for this view (and hence the main focus of my article) was the report of the Institute of Medicine (IOM). I would therefore emphasize that the many researchers with whom I have discussed the matter uniformly disagree with the IOM recommendations. Even though such issues as carelessness, bad judgment, and improper distribution of credit are perpetual problems in science, few scientists seem to believe it would be helpful for government to try to prevent them.

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Clinical and Actuarial Judgment

In their article, "Clinical versus actuarial judgment" (31 Mar., p. 1668), Robyn M. Dawes *et al.* address an important issue. But it is an issue that now extends well beyond psychiatric and clinical prediction. True, Meehl's landmark book (*1*) limited itself to clinical psychology, as does much of the article by Dawes *et al.*, but the question of whether to use the "head" (clinical intuition) versus the "formula" (actuarial or mechanical information combination)—to borrow Meehl's apt terms (*2*)—is equally relevant for medicine (*3*), engineering (*4*), auditing (*5*), management (*6*), polygraphy (*7*), and, as Newell and Simon (*8*) clearly show, for most decisions and choices made in ill-structured problem domains. Moreover, the dilemma they pose of using either the head *or* the formula is no longer the main focus of contemporary decision research. Rather, the focus has long ago shifted to evaluating the use of *both* modes of information combination in tandem.

This trend of combining judgmental with formal modes of information processing probably started in psychology with the suggestions of Edwards (*9*) and of Sawyer (*10*) that experts contribute to predictions by providing intuitions about appropriate judgmental quantities that are best aggregated mechanically. Such mechanical procedures are in constant need of judgmental monitoring (*11*). Dawes (*12*) has made important contributions to this literature, as have many others (*13*), including Meehl (*14*), whose observation, in this regard, is worth repeating (*14*, pp. 372–373).

> 95% of the ordinary decisions made by working practitioners . . . [in mental health settings] . . . are not comparable in richness and subtlety to that of a good psychoanalytic hour . . . [but] . . . when you check out at the supermarket, you don't eyeball the heap of purchases and say to the clerk, "well it looks to me as if it's about $17.00 worth; what do you think?" The clerk adds it up.

It seems, then, that Dawes as well as Meehl advocates the less divisive (than the title suggests) strategy of using the head *and* the formula, depending on whether the decision problem lends itself more readily to intuitive judgment or to mechanical combination. Faust, too, does not appear to have given up entirely on clinical intuition. Otherwise, why would he have provided a set of cognitive correctives in a recent article on human jugement (*15*)? The correctives were designed to help "clinicians to better serve their clients" (*15*, pp. 426–428).

These polemics aside, it is essential to note that the idea that began with the mechanical aggregation of judgmental inputs has been followed up by contemporary decision analysis, a technology that facilitates decisions that will outperform either a purely clinical or a purely actuarial mode. Decision analysis, a variant of Bayesian thinking, is a formal technique that incorporates Bayes' theorem, but adds three essential components (*16, 17*). Stated here as questions, these are (i) In my judgment, can this decision problem be decomposed into simpler segments? (ii) What are the consequences of alternative actions of the decision? and (iii) What are the uncertainties in the environment relevant to the actions and their consequences? By focusing on the resolution of these questions by means of a technique that uses both the head and the formula, contemporary decision analysis, which has been applied in a large variety of domains (*3, 16, 17*), avoids favoring either extreme of the clinical-actuarial dichotomy. It does so by blending formal logic with intuitive insight (*18*). This blend, it has been argued (*17*), yields better results than the use alone of either the head *or* the formula.

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Response: Kleinmuntz correctly states that the clinical-actuarial issue extends beyond the domain we covered; he has himself made distinguished contributions to this literature. His comments could, however, create erroneous impressions about research outcomes and our views in the domain on which we focused—the diagnosis and prediction of human conditions and behavior.

To restate the problem, if one assumes the option of using the clinical, actuarial, or clinical-actuarial approach (to which Kleinmuntz refers, respectively, as the head, the formula, or the two in combination), which judgment strategy leads to the most accurate diagnoses or predictions of human conditions and behavior? The literature shows, overwhelmingly, that the accuracy of the actuarial method equals or exceeds that of the clinical method. The limited research comparing the actuarial and clinical-actuarial approaches also favors the former strategy. Generalization or lack of generalization to other problem realms does not change the evidence in the domain of human outcomes. This large and consistent body of scientific evidence is so important precisely because the intuition that the research would or should turn out otherwise is so compelling.

If a judgmental task is not amendable to the actuarial approach, then there is no possible conflict between alternative approaches, for only the clinical approach remains. For the great majority of everyday decisions made in the clinic, however, actuarial methods are either available or could be constructed with relative ease. Individuals often overestimate the difficulty or expense involved in developing actuarial methods. Even so minimal an effort as collecting clinical staff opinions about predictive factors, pooling these ratings, perhaps with iterative feedback to convergence (Delphi method), and then compiling an unweighted, unvalidated linear composite may well equal or exceed the accuracy of those same clinicians.

The question of generalization to other fields and problem domains and the possibility of exceptions within clinical psychology and psychiatry raise complex issues that resist simple treatment [which is one reason the topic was covered at such length in Meehl's 1954 book (*1*)]. As our article indicated, we agree with Kleinmuntz that humans show certain unique capabilities, such as visual pattern analysis, and thus can provide potentially useful input for decision purposes. Nothing, however, prevents a clinician from recording perceptual impression, such as those gleaned in interviews, in a form conducive to actuarial analysis. The question, then, is how these or other data, once gathered, are best combined or integrated. Almost all of the available evidence suggests the same answer—the actuarial method—and none of the literature that Kleinmuntz cites provides a contrary research demonstration. However, in most of the problem domains Kleinmuntz mentions, such as medicine and engineering, research comparing the judgment methods is limited at best, and theorists are often reduced to educated guessing or forecasting.

We three authors in fact have somewhat different views, or forecasts, about possible exceptions and generalization to other problem domains, as detailed in our individual publications (*1, 2*). For example, one of us (P. E. Meehl) is the most sympathetic to the conjecture that *some* clinicians in *some* contexts can integrate *some* things in a (at present) "non-programmable" way. Meehl conjectures that some psychoanalytic inferences, especially those made from dreams and free associations in a "good" psychoanalytic hour, have sufficient probability to warrant analytic interpretation suggested by them. No actuarial or computer program exists for doing so. Meehl also shows the greatest leaning toward the conjecture that some clinicians in some circumstances can be sufficiently selective in countervailing actuarial conclusions that these departures pay off.

However, as Meehl pointed out in 1954 (*1*), and as all of us agree, even if such conjectures are accurate, that concession would have negligible impact on the main clinical-versus-statistical issue as we have formulated it. Complex psychoanalytic inferences made during psychoanalysis involve a mass of material collected in a unique manner by a specially trained clinician, and more than 99% of all clinical decisions are not of that kind. Kleinmuntz quotes Meehl as conceding more than is intended, for Meehl's quoted remark was to deny that from a premise about psychoanalytic inferences it is possible to conclude anything about the usual clinical process. Moreover, research shows that when clinicians countervail actuarial conclusions they err more often than not in doing so, for if their countervails were correct over half the time they would exceed the actuary, which they do not. This is a simple truth of algebra, not a theory of clinical cognition or a debatable thesis of epistemology.

In the areas in which our conjectures or forecasts diverge, none of us is confident that he is correct, for if there is anything the judgment research demonstrates it is the difficulty of prediction. All of us agree, however, and we suspect Kleinmuntz does as well, that questions about generalization, application, and optimal match between problem realm and judgment strategy should be less a matter of speculation and more the subject of continuing study.

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Erratum: In the News Briefing "Will John Deutch or Dr. X lead MIT?" (5 Jan., p. 25), the California Institute of Technology is incorrectly referred to as the "California Institute of Applied Technology."