

In an essay entitled Toward Social Reporting: Next Steps, published by the Russell Sage Foundation, Dudley Duncan outlined two approaches to the study of social change. One approach is to think through the question of what ought to be measured and then figure out how to measure it. The other is to measure something we know how to measure and then figure out what the measures mean. Broadly, and with some injustice, I would classify the paper by Powers, Cullen and Martin on drug abuse estimates as an instance of the first approach and the papers by Yin and by Hill and Newman as instances of the second approach.

Yin settles upon fire alarms as an indicator series on the basis of purely statistical criteria, that is to say, the consistency of definition of observations, consistency and promptness of reporting, coverage of the universe, etc. On such grounds he regards fire alarms as worthy of extended statistical analysis simply because fire alarms make good statistics, not because they have any settled meaning for the social statistician, as yet.

By contrast, the work of Powers, Cullen and Martin is clearly in response to a pre-established necessity to come up with decent data on drug abuse. They claim, justly I believe, that the statistics describe a massive and increasing drug abuse problem in New York City and the lower East Side, and that, for comparative purposes as between subareas of New York City, their data are probably quite adequate. However, they note many problems with the data, some of which we recognize in other contexts as well. For example, Powers, Cullen and Martin note that the data from the narcotics register rise as a consequence of improved reporting by agencies as well as the result of the increase in use. The confounding of trend data by such processes is something we are familiar with in crime statistics. Second, the duplication and subsequent unduplication of reporting for individuals has the effect of making the revised data look like wholly new series. Third, the authors note the difficulties with the statistical definition of addiction. Addiction has many dimensions--sociological, psychological, physiological, and pharmacological--which make it impossible to establish a single statistical classification scheme for addiction. Here, it seems to me, the investigators are imposing an unreasonable requirement on their own work. It is not clear the development of summary indices presupposes unidimensionality in the basic statistics. Fourth, the authors note the difficulty of knowing whether the absolute rates of addiction are correct, while acknowledging the usefulness of the data they present for comparing addiction rates for different areas. Finally, they suggest that the need to use a multi-year numerator poses a problem. This is not necessarily the case. In vital statistics we are familiar with the practice of preparing rates by consolidating numerator data for several years, especially in the case of rare events.

As a partial approach to the resolution of some of these problems, Powers, Cullen and Martin suggest in passing the possibility of using New York City Police Department figures on drug arrests. One must wonder whether this is really a viable alternative. In effect, it amounts to taking a measure of the treatment of a condition as the operational definition of the prevalence of the condition. Biderman has pointed out the extent to which conventional crime statistics are measures of the performance of the police rather than the prevalence of crime. There is an inflationary dynamic built into statistics of this character which impairs confidence in estimates of the prevalence of the condition. Just as a marketing director distinguishes between sales and the degree of market penetration, we must maintain the distinction between treatment and potential treatment, which here means prevalence.

These considerations suggest two general comments. One has to do with the problems associated with data production for an undefined audience in a field where the measures are slippery. The other has to do with the nature of the task in social indicators. In determining what is an adequate measure, the statistician must ask "What difference does it make?" That is, what would anyone do differently if the number given out were a rather than b. How good does a number have to be? There is, in general-purpose data programs, a perfectionist imperative which frequently results in the statistician's imposing higher standards on the data than are necessary for all but a very few uses. The result--and I have written my share of statistical reports this way--is to label the data "Use with caution," and in effect to say, here are the numbers but don't believe them. The real question is what does the policy maker need to know?

I think it is fair to say, given the disappointment of Powers and her colleagues with the results of what appear to have been heroic efforts, that drug abuse is an area in which there are no general-purpose data. This would seem to be one area where the most important number the policy maker needs to know is the telephone number of the analyst who prepared the estimate. He may be unable to use the numbers intelligently otherwise. While I emphatically dissent from Mr. Yin's suggestion that it is the function of the statistician to devise new policy options, I do believe that in the drug abuse area it is likely that the policy maker needs the opportunity to test the meanings he imputes to the statistics against the judgment of the statistician. As a statistician, one must be concerned with the policy maker's possible misapprehension of the data. This is different, however, from taking responsibility for the policy maker's misuse of the data in the sense which Mr. Yin discusses. I refer to Mr. Yin's concern about the statistician serving as an instigator of repression in the instance where the policy maker's options are limited, and the indicators, and

therefore the analyst who produced them, serving as "an unwitting partner of a fixed urban policy action."

These considerations lead me to a further suggestion about the relationship between social indicators and social reporting. In many of those interested in what is called the social indicators movement have thought of indicators development as an essential prologue in a process the outcome of which is social reporting on the state of the nation or the community with respect to some aspect of well-being. The problems in using highly imperfect data on highly important topics suggest grounds for selectively reversing the relationship here.

The efforts of Powers, Cullen and Martin suggest that in the area of narcotics addiction we are dealing with a subject where indicators for general use are not likely to be forthcoming soon. It is equally clear that this is an area where a limited range of propositions can be supported by the available data. What the statistician can do in this instance is to present those propositions as the chief outcome of his endeavor, supported, of course, by the estimates suitably qualified. That this exegesis requires care goes without saying. I think Hill and Newman provide us with a good view of some of the potential pitfalls, in the area they are concerned with.