

The Myth of Scientific Public Policy.

By Robert Formaini. New Brunswick, N. J.:
Transaction Books, 1990.

Robert Formaini's illuminating work throws into question a key doctrine of social planners not satisfied with the free market. A common argument for the market emphasizes consumer sovereignty: the goods and services produced in a free economy will be those demanded by consumers, since profit-making entrepreneurs have every incentive to produce what consumers wish. Those who will not or cannot do so find themselves rapidly replaced by those more alive to the market's signals.

Against this, many supporters of government planning claim that the free market does not stand alone in its ability to give rise to desirable programs. Quite the contrary, scientific measures of analysis permit objective scientists to determine which programs best promote social welfare. Among the most important of the methods that are alleged to achieve these wondrous results, risk evaluation and cost-benefit analysis rank foremost. Formaini exposes the pretensions of these pseudo-scientific techniques with withering criticism.

Risk evaluation can be used either by itself or as part of cost-benefit analysis. It attempts to answer questions such as: what are the chances there will be a meltdown at a nuclear plant? or, how likely is an epidemic of influenza? Unless problems of this sort can be solved, scientific analysis of public projects cannot succeed. If the designers of a nuclear plant do not know the likelihood of a meltdown, they will be unable to assess the costs of building the plant. Formaini raises a far-reaching difficulty for all such attempts: no consensus exists on the nature of probability.

To some, probability is an objective matter. The chance, e.g., of

drawing a particular card out of a well-shuffled deck is 1 out of 52, no more and no less. The figure has not been arrived at by empirical testing: instead, it follows from the mathematical theory of chances, whose axioms are *a priori*. Formaini, unsympathetic to this approach, raises two problems for it. First, it depends upon the existence of complete information: in the example just given, one must know that there are 52 equally likely possibilities. In cases that risk evaluators commonly deal with, however, full information cannot be obtained. Second, any actual results are consistent with the theory. If a very improbable event occurs, the theory has not ruled it out. Since the rules of probability are "true by definition," the theory cannot be falsified.

The first of Formaini's points is well put, but I am not quite sure that I grasp what he has in mind in the second objection. Why is it a difficulty in the theory that it cannot be falsified? Incidentally, it is not quite right to say that the theory is made true by definition. (How incidentally *can* something be made true by definition?—a famous question posed by Quine.) The theory is based on certain axioms, which, though knowable *a priori*, are not definitions: of course, Formaini is right that definitions are also needed. I may well have misunderstood Formaini's point: he might mean that the question of the theory's application to "real world" cases is not settled by the theory itself. If so, he is perfectly correct.¹

But these are mere quibbles. The view just discussed by no means stands alone in the field. The Bayesian approach, toward which Formaini is more sympathetic, regards probability as dependent on subjective estimates. It does not operate from an *a priori* basis but instead looks to the facts of a particular case. If a Bayesian claims that the chance of getting heads on the toss of a coin is 1-in-2, he will base his assertion on the evidence of past tosses of that coin. His estimates are always subject to revision as further information comes to light.

The dispute between the two views is not a theoretical issue with no practical relevance. As Formaini shows, the two variant approaches sometimes arrive at different estimates of probability for the same case. If so, the scientific pretensions of non-market decision makers already seem shaky. If no consensus exists on the way to estimate probability, how can it be claimed that there is a scientifically objective way of calculating benefits and costs?

The main thrust of Formaini's argument strikes to the essence. Furthermore, the theories must confront additional objections be-

¹The passage that I have difficulty interpreting is on page 22.

sides the ones our author has effectively urged against them. Ludwig von Mises contended that the calculus of probability, which he termed class probability, does not apply to individual events at all. To revert to our instance of the deck of cards, Mises would contend that one can properly speak only about the mathematical chances of classes of events. Nothing about an individual event, the drawing of a particular card, follows from class probability.

Even if one does not accept Mises's characteristically radical and incisive proposal, another severe problem faces the theory. This point is especially prominent in the work of a writer of quite another stripe than Mises: John Maynard Keynes. In the *Treatise on Probability*, Keynes shows that the application of the theory to single events depends on a controversial and seemingly arbitrary assumption, the principle of insufficient reason.

And what about the Bayesian approach? Here I think that more might have been made of the fact that Bayes's theorem requires that one assign a prior probability. The choice of this figure can radically affect one's results, but it can be picked only on an arbitrary basis.

Formaini also proposes an improvement in the way probability estimates are applied. He suggests that the analyst modify his estimate by the degree of confidence he has in his calculation. "A simple formula can be added to probability calculations to allow for uncertainty attaching either to the data or the theory through which the data is [sic] manipulated" (p. 21). If one uses the Bayesian approach, has not uncertainty in the data already been considered in the calculation? So far as uncertainty about the theory one uses is concerned, Formaini's suggestion threatens to produce a regress. How is one to estimate one's certainty? How certain, again, is this calculation? Must we discount yet again for uncertainty? This problem was raised long ago by David Hume.

Formaini goes on to suggest that if a good event is expected, one should discount the likelihood of the event's occurrence; if one predicts something bad, the likelihood should be increased. Once again, I fear that I have misunderstood. It is not apparent why certainty about one's calculation should vary directly with the badness of an event, and inversely with the goodness; and in any case one cannot be more certain than $p = 1$, which will result in the original sum calculated, not an increase. But more likely Formaini intends the discount for the nature of the predicted event as another step in the calculation, not as a consequence of the certainty estimate. If so, he gives no justification for this additional step.

An objection that might be raised to Formaini is that his case is in a sense too good. If probability calculations lack a firm basis, why does this count against non-market methods of decisions alone? Is it not necessary for entrepreneurs in the market to calculate risk? Will not precisely the same problems of estimation confront private decision makers? Although the book does not directly address this problem, Formaini has the resources adequately to deal with it.

The response in fact emerges in the third chapter. Before this, however, Formaini briefly sets forward the method of the Austrian School. His second chapter accomplishes a great deal, covering in a succinct and accurate way the foundation of the Austrian School; its conflict with the German Historical School; the Austrian deductive method; the use of *verstehen*; and the development of Austrian economics in the twentieth century. (Incidentally, it was not the German Historical School, as Formaini thinks, who proclaimed themselves "intellectual bodyguards of the House of Hohenzollern." Though its members would have enthusiastically assented, the statement comes from Emil DuBois-Reymond. Also, Hayek did not leave Austria owing to the Nazis (p. 27); he was named to a chair at the London School of Economics in 1931. In addition, Karl Popper does not think that the inductive methods of science can falsify a theory, nor does he claim that a well-corroborated theory is true [p. 33]. His best-known doctrine is the utter rejection of induction.)

The book's third chapter, "A Subjectivist Evaluation of Cost-Benefit Analysis," is the best in the book. In painstaking detail, Formaini shows the numerous problems that confront cost-benefit analysis in the vain efforts of its proponents to elevate it to the rank of objective science. For one thing, a social discount rate must be estimated. This rate determines how much future benefits are to be lowered when conducting a cost-benefit analysis. If, e.g., someone proposes that a subway be constructed that will be able to carry passengers for 50 years, one cannot simply add together the benefits from travel 50 years' worth of passengers will receive. Future benefits are not worth as much as present gains: but how much less? As Formaini shows, there is no good way of telling.

Further, cost-benefit analysis usually rests on the assumption that perfect competition is a welfare ideal. In the Austrian view, this assumption is without basis. Why is a particular market structure more desirable than whatever is actually to be found on the free market?

To this the neoclassicals have a ready answer. They claim that monopoly results in a welfare loss; further, any deviation from perfect competition imposes at least a degree of monopoly.

Formaini's response is forthright. The "welfare loss" in question depends on hypothetical preferences that are ascribed to people. The Austrians refuse to adopt this conception of preference, which they regard as arbitrary. The only acceptable conception of preference, as Austrians see matters, is demonstrated preference. If an actor does something, one can say that at the moment he preferred the choice he made to any alternative available to him. On this view of preference, the claim that a welfare loss is caused by monopoly cannot get off the ground. Neither can alleged benefits from public goods expenditures be demonstrated. Of course, paying taxes that are used for public projects does not demonstrate a preference for these projects, since these subventions are exacted by coercion. The powerful tool of demonstrated preference enables the Austrian approach to respond to the objection I raised earlier. There is no special free market way of calculating probability, and the establishment of a free economy does not resolve the difficulties of probability discussed earlier. But we can say that if people on the market engage in a project, they have demonstrated a willingness to assume whatever risks are involved in that project.

The problems inherent in cost-benefit analysis are many and various. The welfare criteria that cost-benefit analysis analysts use are either ineffective or questionable. The Pareto criterion, according to which a project can be undertaken if at least one person is made better off and no one worse off, rules out almost all state activity. Thus resort is frequently made to the Kaldor-Hicks principle, which allows changes if the gainers from a project are able to compensate the losers. The criterion does not mandate actual compensation: it thus sacrifices the welfare of the losers to the winners. Formaini notes that sometimes planners attempt to add up total benefits and total costs: a project will go forward if benefits exceed costs to a greater extent than for any alternative. This procedure is not a "watered-down" version of Kaldor-Hicks, as the author claims (p. 55), since application of the latter criterion need not maximize welfare.

In a final chapter, Formaini gives a detailed account of the swine-flu vaccination program, a major debacle of 1976. The constant errors of the bureaucrats responsible for this program cast a grim light of humor over the pretensions of cost-benefit analysis to be scientific. The government medical establishment conjured up a swine-flu epidemic out of a handful of flu cases. Their prevention measures for the non-existent epidemic cost hundreds of people their lives.

In a brief postscript, the author sums up the lessons of his outstanding study. Ethics provides a much more solid basis for decision

on whether public projects should be built than the supposed objective discipline of cost-benefit analysis. I wish that Formaini had explained in detail his reasons for thinking that *Brown v. Board of Education* was decided in a morally correct way (p. 95). Common sense is a better guide than dubious expert assessments.

I have sometimes, I fear, been unjust to Formaini in the preceding remarks. For reasons of temperament, I tend to emphasize points of disagreement. But my overall reaction to this excellent book is one of wholehearted assent.

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