

# Observations on Health Services Research and Health Policy Analysis

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**A**S A DIPLOMAT MIGHT PUT IT, BICE'S PAPER ON social science and health services research is an impressive tour d'horizon. From any standpoint this paper is an authentic tour de force.

Bice's paper is encyclopedic in scope, rich in texture, and always evocative of the concrete instance. It conveys a unique sympathy for and understanding of other social science disciplines as well as his own. The paper is itself a justification of Bice's plea for a unified social science. Indeed, the paper exemplifies what interdisciplinary research can be, when it is powered by a single well-equipped and curious mind.

Bice is sensitive to the problems facing workers in health services research today, and emphasizes both the concerns and the steps needed to make it useful for decision makers. The paper displays a level of effort and performance that I have come to expect of Bice, on the basis of both his own creative and self-critical writings and his service as staff director for the Institute of Medicine Committee on Health Services Research (1979).

With this much said—and I am not about to qualify a single word of these encomiums—I proceed to discuss selected issues of substance posed by Bice's paper. My commentary falls into four sections:

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1. Shared propositions: those that I agree with.
2. Unshared propositions that I differ with, which nevertheless lead to policies for health services research that I agree with.
3. Propositions that I disagree with.
4. Other topics: issues that I deem important and wish Bice had dealt with.

## Shared Propositions

This section deals with four propositions by Bice that I share.

### *Communication with Nonspecialists*

The importance of communication between researchers and the intelligent laity, including policy makers, cannot be overestimated. There is no good reason for expressing published research findings in a language other than standard English, with the exception of mathematical proofs. Most leading economists write clear English prose and some write it with elegance. I have observed that some employers hire economists to perform generalized tasks because they know how to read, write, and do arithmetic.

Communication between researchers and the rest of the world is a two-way street. Not only should the researcher disseminate his findings among potential users but the users must also convey their needs to the researcher, who will then convert these felt needs into questions that can be studied empirically, as Bice points out.

In communicating research findings to nonexperts it is worth recognizing the potential risks of misinterpretation and the importance of acting to reduce such risks by expressing caveats or qualifications clearly. Bice's own work with David Salkever on the effects of certificate-of-need programs is a good example of cautions clearly stated (Salkever and Bice, 1976). From my own knowledge of the health economics literature, I can recall instances in which caveats were not expressed with prudence. In the first phase of research on hospital use it was found that the age composition of the population was not a factor; but it turned out that this finding was an artifact

produced by the particular statistical method used. Opposed to the widespread belief that the death rate and income are inversely related was the finding that for adult males they are directly related; however, this relationship may not hold for all age classes. The finding in recent years that the use of physician services and income are negatively related for the young and the old, but positively related for working adults, may merely reflect the reduction in income due to the illness of breadwinners, and tell us nothing about our society's attitudes and behavior toward children and aged persons. Of course, not all risks of misinterpretation of research findings can be averted. Nevertheless, some precautions will be worthwhile.

### *Economists and Policy Analysis*

According to Bice, economists are comfortable in performing policy analysis and avoid becoming enmeshed in the fact-value distinction. I believe that economists feel comfortable in this endeavor because they enjoy comparative advantages in possessing a kit of tools, in being able to manipulate certain data, and in approaching issues from a marginal (or incremental) perspective. Economists do not spend time on the fact-value distinction, because as economists they have largely limited their concerns to matters of economic efficiency, that is, the best possible allocation of scarce resources that are capable of alternative uses among competing ends, under given institutional arrangements. They acknowledge their inability as scientists to make interpersonal comparisons, even as they retain the freedom to make value judgments as citizens or policy advocates. The opportunity to exercise value or political judgments in a separate arena permits considerable tolerance for diverse viewpoints on policy within the economics profession.

Contrary to Bice, I do not believe that economists, more than any other group, subscribe to the ideal of a rational social and political order. Rather, I believe, they are concerned with pursuing rational means toward given wants or ends, whatever these happen to be, and however they are arrived at. With few notable exceptions, like Veblen or Galbraith, economists are comfortable not to inquire into consumers' tastes and preferences, regarding them essentially as a black box.

Moreover, economists emphasize that rational behavior is required only at the margin and not throughout the economy.

However, the ends to be maximized are not solely or necessarily material ones. To maximize a person's utility function, as the jargon goes, is to arrive at his or her preferred combination of pecuniary and nonpecuniary returns from a purchase or one's occupation. The notion of a balanced bundle of pecuniary and nonpecuniary returns is an old one in the history of economic thought, and was clearly expressed more than 100 years ago. Almost from the founding of the discipline in 1776 (Smith, 1937), economists—whether they be practical men of affairs or cloistered scholars—have regarded as the purpose of economics the improvement of the lot of mankind, “free from the pains of poverty and the stagnating influence of excessive mechanical toil” (Marshall, 1936:40). Individual economists may perform different tasks within the discipline, but few who embark on a career in economics lack the desire to influence the shaping of policy in the real world.

### *Relations among the Social Sciences*

I support Bice's plea for greater understanding among the social science disciplines, even as I recognize that I am not nearly so adept a practitioner of a unified social science. His point is well taken that it is the problem at hand, not the discipline in which one was originally trained, that should dictate the choice of method for studying it and the literature to be explored. Furthermore, the better one's understanding of the assumptions that are conventionally adopted in disciplines other than one's own, the more likely one is to examine these assumptions and to employ them prudently.

My own four-year term as a member of the then new Health Services Research Study Section (to review grant applications for the National Institutes of Health) in the early 1960s served as a splendid postdoctoral fellowship, which extended my horizons beyond economics and politics to sociology, anthropology, epidemiology, biometrics, and preventive medicine. Conferences that focus on the discussion of prepared papers, like those sponsored by the Health Services Research Study Section in 1965 and 1966, serve useful

purposes in facilitating communication among the academic disciplines. Not nearly so often does a special committee on a particular public policy issue promote interdisciplinary research and mutual respect among the disciplines. Perhaps graduate education within the social science disciplines can cultivate a broader base, provided that training in depth is not sacrificed.

### *Peer Review*

I subscribe fully to Bice's remarks on the importance of peer review for monitoring the quality of research. It should go without saying, then, that members of study sections would have displayed competence in research by having completed a substantial body of published work. Funding agencies should require, rather than permit, that research reports be published. Unpublished studies would be treated as a mark of failure. I am inclined to invoke an additional, if informal sanction, namely, that unpublished studies are to be regarded as if they were in the process of publication, a process that is terminated after two or three years, when bibliographic references to them will cease.

### Unshared Propositions, but Shared Policies

About some matters Bice and I differ in approach but tend to reach the same conclusions on policy. A couple of examples will suffice.

### *Why Government Should Support Research*

Unlike Bice, I do not base the desirability of government support of health services research on government spending for health care. Even if all spending and provision of health services took place in the private sector, a strong case could be made for government support of research as a pure public good. By this term is meant an economic good whose enjoyment by individual A does not diminish its availability for enjoyment by individual B. Given, in addition, the substantial economies of scale that exist in the application of research

findings, the case for public support of research impresses me as incontestable.

Clearly this argument for government funding of research has nothing to do with the government's obvious interest in learning how its own service programs are being implemented, since I have posited the extreme—and unreal—case of total private spending for and provision of health care. To put the bulk of research funds into the hands of agencies with program responsibilities is to misapprehend the nature and potentialities of research, especially when it is conducted and viewed as a cumulative enterprise. I find some support for this thesis in the history of health services research, for the term itself was coined and the Health Services Research Study Section was organized in the years 1960–1962, several years before the enactment of Medicare and Medicaid.

Bice's recognition that government spending on services is a basis for its interest in funding research is perhaps a wise concession to reality. But this concession presents a weaker case for public funding of research than is warranted. More important, it may lead to distortions in research activity. It is even conceivable that concentrating responsibility and authority for health services research in program agencies is to insure that needed research is not carried out, if it is seen as potentially threatening to the program.

### *The Contribution of Health Services Research to Policy*

Still, what has been the contribution of research to the formulation of health policy? Can it be said that health services research is useful?

Bice cites first a number of articles in the health services research literature that have answered the second question in the negative. He then proceeds to document a sizable literature outside health services research that concludes that, under certain favorable conditions, research tends to be applied.

I have never been persuaded that decision makers prefer to adopt policies in ignorance. My experience supports the external literature as the more plausible. Of particular importance is the cumulative weight of a line of research, rather than the findings of individual

projects, as the Comptroller General of the United States has learned (Staats, 1980). Regardless of how planners and decision makers may have come to hold this view, it is evident that the "availability effect" (or supply exerting an influence on demand) is a dominant notion in hospital planning today. Is it not a fact that the same scholars who question the importance of reimbursement as a source of increase in hospital care expenditures when they are academics assign it prime importance when they become bureaucrats? Is it not true that the research finding of low hospital utilization under prepaid group practice has become a major underpinning of the health maintenance organization (HMO) movement? Let me introduce a personal note: the device adopted by the Congress for including the treatment of end-stage renal disease under Medicare is precisely the one developed by the Gottschalk Committee (1967) in its report to the Bureau of the Budget. Does it really matter whether a staff member of a congressional committee read the report or developed the same idea independently in the prevailing intellectual climate?

I should be inclined to argue that, even in the absence of the above examples, the content of public discussion about health care policy is influenced and changed by research findings. For, if nothing else happens, research findings permeate university courses in health economics, medical sociology, health policy, etc. All modern economists know that John Maynard Keynes concluded his influential book, *The General Theory of Employment, Interest and Money*, as follows:

Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist. . . . I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas. Not, indeed, immediately, but after a certain interval; for in the field of economic and political philosophy there are not many who are influenced by new theories after they are 25 or 30 years of age, so that the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest. But, soon or late, it is ideas, not vested interests, which are dangerous for good or evil. (Keynes, 1964:383-384)

It may be that a greater danger than neglect of research findings is arriving at a premature consensus over what those findings signify.

## Propositions That I Disagree With

Although I agree with Bice on most propositions, his paper contains a couple of propositions that I wish to challenge.

### *Does Research Follow a Consensus on Policy?*

Bice notes, without questioning, Odin Anderson's (1966) proposition that health services research follows a consensus on health policy. This may be true for the most part of research that requires large sums of money, as in the collection of data through field surveys.

My observation of research in health economics is that the example set by a leading academic is a dominant influence on the problems that are studied. I have a firm impression that Kenneth Arrow's article on uncertainty in medical care (Arrow, 1963) made it respectable to do research in health care financing, after a long period of neglect. Indeed, such an article also serves as a lightning rod for attracting comment and criticism, which is one way a young scholar can attract attention to his keenness and prowess of intellect.

Another important influence on the contents of research is the coming together of concepts or questions to be studied with data. Economists who may be willing to use almost any data set, in the early phase of research in a problem area, at some point will desist from further estimating until the requisite data become available. A good example of this phenomenon is the virtual moratorium on research in the shape of hospital cost functions in the late 1960s and early 1970s, while scholars awaited the availability of data on hospital case mix. Also instrumental in the resumption of research in this area was the breakthrough by Robert Evans (1971) in Canada in handling such large masses of data.

I should not underestimate the influence on research of the leadership of service programs. In prepaid group practice, the organizers of the Health Insurance Plan of Greater New York stressed research and publication from the outset, in contrast to the Kaiser-Permanente plan, which emphasized data collection and analysis for internal use by management. In the Medicare program the original leadership, I believe, attached greater importance to research than the subsequent



ones, by deliberately building redundancy into the data systems, funding relevant external research, and awarding fellowships to young scholars to do intramural research.

### *Who Is to Prepare the Research Agenda?*

Still, one may ask, what difference does it make whether research in a problem area follows a policy consensus or is independent of it? It seems to me that the notion of independence supports the importance of sustaining scholars in pursuing their own ideas for empirical research. I am unaware of evidence that the funding agencies are better able than the research community to discern the important emerging problems in health or health care. The funding agencies are more likely to be immersed in the immediate concerns of the government's budget and to jump from fad to fad. The enabling role of hard-money jobs in the universities in saying No to requests for proposals is beneficial to decision makers, the potential users of research. The decision makers also stand to gain from the research community's emphasis on the quality of research.

I do not believe that any group of professionals or leaders of a society have a superior claim to that of the bureaucracy to formulate a research agenda. In my opinion, the most plausible research agenda is the one that the author of that agenda actually pursues. Obviously there are then likely to be gaps in the aggregate of all such agendas, leaving unstudied some problems that call for study. Here I am inclined to invoke an appeal to interested scholars, trying to persuade them of the importance of particular problems and indicating the feasibility of studying them with some degree of success. The foremost contribution that any of us can make to health services research is to do it.

Speaking only for myself, I feel that I have all the access to policy makers that I can afford and that they, in turn, have all the advice they can stand. Under no circumstances could I justify a leadership group bestriding the channels of communication between the research community and the sources of funds. In asserting so strongly the autonomy of individual scholars in preparing agendas for research, I intend to go far beyond Bice's point that federal requests for proposals

cannot properly prescribe approaches and techniques. I agree with Bice. In addition, it seems to me that if the authors of the requests for proposals knew enough to prescribe approaches and techniques, they would already have done the study being proposed.

## Other Topics

There are several topics that I should have dealt with if I had been the author of Bice's paper.

### *Lack of Tradition of the Royal Commission*

One topic that I deem important, but it need not detain us, is that in the United States we lack the tradition, so pervasive in the British Commonwealth, of high-quality research done by or for a royal commission of inquiry, thereby earning reputation and sometimes renown. In this country scholars reserve their high-quality research for professional journals. Policy analysis for committees and commissions is, with a few exceptions, regarded as work of an inferior order.

I wish that it were possible to change this perception, because participation in the work of a committee affords an opportunity both to exert influence directly on the decision-making process and to gain respect for the application of high-quality research and prudent interpretation of findings to policy formulation. My experience encourages me to believe that both results are attainable.

### *Downward Trend in Access to Data*

Over the last twenty years I have become increasingly concerned over the decline in access to data, at least in the New York area, which I know best. Time was when multiple sources of information permitted empirical research on most practical issues in local health care policy. Unfortunately, emphasis on efficiency in data collection has led to a curtailment of access to data and to the exercise of quasi-monopoly control over such access. In consequence, certain issues are no longer studied or are analyzed internally by the possessors of the data, without replication by professional peers.

Gradually I have arrived at the conclusion that emphasis on minimum data sets and on avoiding duplication of effort in data collection can and is likely to preclude the collection of data that are germane to the particular question at hand. At best, the tendency is to try to fit the problem to the available data and to call them proxy measures. At worst, certain problems remain unstudied for lack of data.

It seems to me that it may be just as difficult to influence the research community with respect to data as it is to influence decision makers with respect to policy recommendations. The widespread recourse to proxy measures, which remain untested in relation to the behavior of the data representing the true variables, serves to reduce the demand for appropriate data. For example, in the large-scale household survey of expenditures for and utilization of health services it took the exercise of some personal influence to modify the measure of income, so that the downward effect on income of illness by bread-winners could be measured and allowed for.

### *Health Services Research vs. Health Policy Analysis*

The addition to Bice's list of topics I deem most important has occurred to me only in the past couple of years: the distinction between health services research and health policy analysis. As a matter of fact, this dichotomy appears also in the title of Bice's paper.

To distinguish between research and policy analysis is to raise two questions: How do the two activities differ? If they do differ, what are the implications?

To begin with, although policy analysis is always addressed to the future, it is based substantially on research, which examines past experience. It is not evident what alternatives we have to basing policy analysis on empirical research; certainly deductive reasoning or anecdotal evidence is not a worthy substitute. Even so, the difference between the two activities in their time orientation suggests the need for caution in applying research findings to policy analysis. It seems best to treat such findings as a partial contributor to the process of policy analysis, to be interpreted with prudence and with due regard

for uncertainty concerning the future state of the world. That is to say, even if we were able to understand and explain the past perfectly, the explanatory model might still not hold for the future.

Uncertainty concerning the future clearly implies its unpredictability. If a number of forecasts are made, some of them are bound to be mistaken. Small populations will display greater variation than large populations.

From past experience it is reasonable to expect that some of the effects of major programs will not have been anticipated. To some extent, perhaps, what appear to be unanticipated consequences of a program may really represent reservations concerning the program that went unexpressed, because on balance the program seemed worthy of support or because the concessions were seen as a necessary price for participation in the program by erstwhile opponents. After allowance for these qualifications, it still remains likely that major programs will have some unanticipated consequences, certainly in magnitude and perhaps even in direction.

Given the inherent unpredictability of the future state of the world and of a population's responses to change, a principal focus of policy analysis ought to be the development and dissemination of devices and arrangements that enable programs to operate with flexibility, a flexibility that does not come costless. What I am suggesting is that in order to design systems that operate with flexibility internally, policy analysts must acquire knowledge of institutions and programs that they do not usually attain. Moreover, in the past, system-wide flexibility has been facilitated by converting facilities from one use to another. This option is not readily available under conditions of system-wide curtailment or contraction.

Another difference between the two activities is that while research is often conducted on small-scale programs, the findings are applied to the design of programs of all sizes. Yet large programs can produce repercussions that small programs cannot produce. It follows that research on large programs will have greater relevance for policy analysis of large programs than research on small programs. Conducting research on large programs may require going outside this country, where political and social institutions differ from ours. Whether

the particular differences are so crucial as to render the research findings inapplicable to our own setting is a matter of judgment, guided by experience and predilection. Another promising approach is for research to take advantage of actual variation in program contents, some of which may represent failures or lags in program implementation. What is an embarrassment for administrators may be an opportunity for scholars. Again, it is necessary to go into the field and learn what is going on.

I take it for granted that the objective of research is to describe the situation before and after a program is instituted and to describe the program, as well as to try to determine the effects of that program and of individual components of that program. What values to put on the physical effects seems a straightforward procedure when the outcome measures carry price tags. In their absence, valuation of effects becomes difficult or may even be impossible. To be specific, I note the absence of consensus today among economists on how to value given changes in health status. The absence of consensus is strikingly true of the valuation of prolonging life expectancy, but also holds true for the valuation of reduction in pain or discomfort and postponement of grief. At the technical level, it has become necessary to retreat from cost-benefit analysis to cost-effectiveness analysis. At the policy analysis level, it is necessary to acknowledge that often research sheds no light on the setting of priorities for the allocation of resources. Even what appears to be the abstruse problem of selecting a discount rate, in order to render commensurate the streams of costs and benefits taking place over time, is not merely a technical datum. The level of the discount rate turns out to have implications for the size of the public sector in the economy and for the allocation of funds among programs and among population groups within programs.

Additional difficulties arise in the effort to take account of the distributional effects of programs. As citizens we hold different views on how egalitarian our society ought to be, and we may hold different views concerning redistribution by cash or by services. We may even be inconsistent in employing the several potential indicators of equality—access to services, use of services, use in relation to need, or health status. Even the individual scholar when he or she turns into

a policy analyst may be inconsistent, or may simply have a change of mind concerning the relative weight to attach to any one among many findings over a research career.

It seems fair to conclude that, as scientific undertakings, health services research and health policy analysis are different entities. Perhaps with some exaggeration of the formal distinction, I see research as describing the real world plus testing hypotheses about how a given state of affairs came to be. I see policy analysis as listing and appraising alternative future outcomes and placing bets on them. In effect, the policy analyst is declaring for which outcomes he or she is prepared to be accountable. Again, in research a finding of zero effect has the same standing as a finding of positive or negative effect. By contrast, in policy analysis the research finding of no effect may signify that uncertainty concerning the future remains intact. The decision maker's need for advice is undiminished.

Notwithstanding all these differences, the same individual can perform both research and policy analysis. Some persons are well equipped by training, experience, and temperament to do both. However, it is important for the individual carrying out the particular activity to recognize and acknowledge which hat he or she is wearing at that time. Since policy analysis entails the rendering of value judgments, such judgments should be made explicit.

The important role of value judgments in policy analysis leads me to urge pluralism in organizing and operating the policy analysis enterprise. Moreover, given the likelihood that mistakes will have been made in forecasting the future and that some effects will have been unanticipated, both the mistakes and the untoward effects will be detected more promptly and acknowledged more readily outside the agency with budgetary responsibility for the program than inside it. No credit accrues to the manager of a public program for confessing error; virtually the opposite holds true for the academic.

The case for diversity in the health policy analysis enterprise is reinforced by recognition of differences in tastes among populations and communities. To place health planning at the local level is not to sacrifice a degree of efficiency, as Bice says, but rather to recognize and accept the diverse wishes of the citizenry. In the absence of sizable geographic spillover, the national interest is the sum of local interests.

The federal budget is not an adequate reflection of that national interest.

Although I have gone to great lengths to emphasize the differences between health services research and health policy analysis, the two activities do share some common features. Both require sensitivity and access to the problems of consumers and providers of services. Both activities call for data that bear on the problem at hand. Both can profit from the distance that lends perspective in choosing problems for inquiry. Both can make use of hard-money jobs and an academic base that enables scholars to disregard some requests for proposals.

However, the distinctions between the two activities I have noted—in time orientation, uncertainty, scale of programs dealt with, and the intrusion of value judgments—call for close examination of the accepted view that health services research and health policy analysis are either the same activity or inseparable activities. Clearly, in my opinion, they are not. Past failure to note and observe this distinction has contributed much, I believe, to the perception that health services research has failed to deliver knowledge useful for solving problems of health and health care in our nation.

I am grateful to Tom Bice and to the organizers of the Conference on Social Science and Health for the opportunity to discuss a paper of such breadth and depth, and am pleased to have my extended comments published along with the paper. I have no doubt that Bice accepts my comments and criticisms in the spirit in which they are tendered. Indeed, it would not be astonishing if continuing reflection and further discussion served to narrow the apparent differences between us. In any case, his paper has already served the purpose of stimulating thought on how to promote health services research and health policy analysis in the future.

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