Book Reviews

Consumer Report

Politicians, Bureaucrats, and the Consultant. A Critique of Urban Problem Solving. GARRY D. BREWER. Basic Books, New York, 1973. xii, 292 pp., illus. $12.50.

New information systems proliferate faster than we can keep track of them. The futurists are here; technology assessment is established by mandate of Congress; management by objectives is enshrined in the Office of Management and Budget; research on social indicators grows apace; variants on program budgeting are adopted the world over, almost as fast as old ones are abandoned; and management information systems of all kinds breed faster than rabbits. Despite apparent differences, all these devices have certain attributes in common: they are established without a single successful demonstration, they are tried everywhere, and they do not work anywhere. They require theory that no one has and data no one can get. All claim to enhance societal learning, but none contain operative mechanisms for benefiting from their own mistakes.

What we need, some people are saying, is a method for assessing the impact of technology in the future. What can such a statement mean? Presumably it does not mean learning from experience, because the idea is to avoid that costly method. The only other mode of learning known to mankind is called theory. When applied to the purpose of social control, theory assumes a causal aspect: under specified conditions and assumptions, which must be explicated and defended, certain elements in various combinations and interconnections will, within a range of probability, produce the intended consequences. Put this way, the requirements for predicting either which technologies will become dominant or the multitudinous chain upon chain of consequences they may entail, all subject to varying degrees of improbability, are evidently enormous. If

the predictive variables are too few, the theoretical models are too simple; and if there are too many, it is extremely difficult to understand their interaction. Indeed, I doubt very much whether anyone today can "retrodict" a theory that would account for the victory of the piston engine over the steam engine or generate the innumerable consequences of mass automobile traffic in such a way that the variables involved could have been subject to governmental intervention at the time. Social science is in its early Ptolemaic period, if it has got that far, and nothing is achieved by assuming that our need to know will generate knowledge or that the will to believe is a substitute for the hard work of constructing and testing social theory.

Anyone in danger of being persuaded that there is a marvelous new information system available to solve his problems should read Garry D. Brewer's brilliant book, *Politicians, Bureaucrats, and the Consultant,* a thoughtful, balanced, incisive analysis of one such informational device from its origins to its failures in execution. And the reader should keep in mind that the efforts at computer simulation of urban policy problems in San Francisco and Pittsburgh, which the author describes with such flair and discernment, are, however complex they may appear, many orders of magnitude simpler than technology assessment or futurism or arrays of social indicators.

Brewer appears in this book as a social science detective. By interviewing the people involved in creating, maintaining, and attempting to use the computer models, and by examining these models themselves, insofar as their endemic lack of documentation permits, Brewer is able to show us both what went into these operations and what, if anything, came out of them. If he seems more like Victor Hugo's Javert than Georges Simenon's Inspector Maigret, it is not because he lacks compassion, a quality evident

throughout his work, but because all he has to report is misery.

A consultant made fun of a city councilman in Pittsburgh who, upon being told that a computer simulation was contemplated, asked if it was anything like artificial insemination. Actually, there is more truth than poetry in that notion. The process is artificial. The idea is to recreate on a computer something like the actual process of decisions in a particular sphere of activity. A simulation is not unlike an interlocking series of animated flow charts with each actor represented by motives that lead him to use various decision rules for propelling the subject matter to one place or another, where, in turn, it is picked up and acted upon again until some final resolution is achieved, at least in the model. That is where insemination comes in. The idea is that by varying the inputs of data or the decision rules used, the computer will simulate the consequences of making these changes. If (and it is a big "if") the model of the policy universe corresponds reasonably well to the world to which it is supposed to refer, real actors will be forewarned about the probable consequences of alternative courses of action and will be able to choose better among them. The design is grand but, as Brewer shows, the execution is awful. At a minimum, computer simulation requires theories about the underlying relationships in the policy area, clients who know what they want, and social scientists who know how to give it to them. None of these elemental conditions was met in either the San Francisco or the Pittsburgh venture.

Computer programming proceeds by debugging. Even simple programs do not work the first few times, and complicated ones require endless iterations. A large model, involving numerous participants, large numbers of decision rules, and possibly tens or even hundreds of thousands of data bits, requires many runs to see if the outputs are intelligible, if they are sensitive to small changes in critical variables, and if they comport with common sense. Original conceptions of theory and early collections of data may have to be compromised in order to get material in a form that will permit it to be run expeditiously on the computer. Hence it is essential that a careful log be kept of exactly what has been done, so that future modelers will be able to learn from past errors. Even Brewer, who might be excused if he

thought he had heard it all, was evidently taken aback in the following exchange with the man who was in charge of San Francisco's computer simulation (pp. 149–50):

A: There is no documentation for this program. In other words, if you wanted a fresh programmer to come in here, it would take him at least two man-months of hard work just to learn it.
Q: A good programmer?
A: An *excellent* programmer. One who is able to lay that flow chart kind of thing out. One who is really astute.
Q: You mean to say that there are no flow charts?
A: No flow charts, no detailed charts for a computer programmer.
Q: You mean you have just a listing and nothing else?
A: Yes. Furthermore, the whole thing is on cards. You know you have eight or ten boxes . . .
Q: Just for the model?
A: Just for the model.
Q: My God, what is that, something on the order of 20,000 instructions?
A: We never were able to get a precise count, but we figure that it was between 20 and 25,000 instructions. . . . [Name of programmer] is the only guy who knows anything about the program—the only one. He is the only man who *still* knows anything about the programming.
Q: What would happen if he got hit by a truck?
A: [If he] did in fact get hit by a truck, and I hope to God that he doesn't . . . somebody, sometime, will have to go through the agony and labor of reconstructing it.

Can any program with thousands of instructions, we may ask, be understood by anyone?

The setting for the two urban simulations under consideration was created in 1959 when Congress authorized the Housing and Home Finance Agency (HHFA) to make grants to local governments for preparing plans under the Community Renewal Program. The idea was to get away from "projectitis" in urban renewal and move toward comprehensive and coordinated housing policies in each city as a whole. City officials evidently needed to know more about the kind of housing stock they had, the people housed, the nature and extent of blighted areas, and the activity of the housing market so that they could, before making their decisions, determine what needed remedial action, analyze the alternatives, and select those measures that might have the good consequences they intended instead of the usual bad ones that are unintended.

San Francisco's Community Renewal Program (CRP) sprang to life in October 1962, when the HHFA approved the city planning department's application. The federal government put in about two-thirds of the million and the city the other one-third. Four months later a contract was signed between the department of city planning and the consulting firm of Arthur D. Little. Exactly how the adoption of computer simulation for the CRP came about is not quite clear, but the essence of the matter is conveyed in an interview Brewer had with the program manager on the city planning staff: "Then they [the Arthur D. Little firm] took over and under their house funds, or what not, they actually wrote us a prospectus which then became the . . . [pause] and this is ironic, after we got this thing, pretty much reflecting what we had been persuaded was what we wanted to do, then we threw it open for proposals. . . . We had about five or six, but we finally did choose Little" (p. 105). Similarly, after putting together a package of something over a million dollars with HHFA support, the department of city planning in Pittsburgh engaged the services of the Center for Regional Economic Studies at the University of Pittsburgh and through it a consulting firm called CONSAD.

According to the consultants for San Francisco, "Arthur D. Little, Inc., has shown modern computer technology to be an effective tool for finding practical solutions to city problems. . . . Simulation models provide a continuing method for finding answers and predicting results as recommendations are followed and programs for revitalization continue" (p. 114). According to the consultants for Pittsburgh's CRP, CONSAD would "help in developing for use a digital computer simulation model to test the economic, social and locational consequences of various hypotheses of new investment and urban change. The model describes the entire urban area of the City of Pittsburgh and forecasts the impact of proposed land-use policies" (p. 169). Although much was said about relating city activity to its fiscal capacity, to federal resources, and to the needs of the neighborhood, the models were essentially concerned with housing and land use. The incredible complexity of the models, in terms of the units of data and the size of the output, is matched by the extraordinary simplicity of their

causal mechanisms: for the most part the models are driven by such assumptions as that population will grow the way it has grown and people will move where things are better and real estate operators will try to maximize their return on investments.

No summary can do justice to the thoroughness and perspicacity of Brewer's multifaceted evaluation of these various models. But I shall try. What is the range of distortion between the models and the real-world systems to which they are supposed to refer? The models are inaccurate, unreliable, and unreal. The range of variation in results is so large, failure to predict critical variables like population so great, and the use of mechanistic projections so faulty, that the models cannot (or ought not) be used for policy purposes. Are the inputs and the outputs intelligible? When potential users asked for interpretation, they frequently got "mumbo-jumbo" instead. Often the output was inches thick and took two to three weeks to produce; the papers were covered with figures that appeared to lack meaning, yet could not be ignored entirely. Do the results comport with common sense? No, they don't; a potential user must be in a quandary when the outputs suggest that local preferences are just the opposite of what they have recently been and that people are moving to new locations distant from their homes when they have usually proceeded to adjoining neighborhoods. Have important variables been omitted in the interest of machine readability or ease of generalization? Mostly it is not possible to tell because the modelers did not ask this kind of question. In the San Francisco case, however, where deficiencies of data were made up by creating artificial residential areas called "fracts," the fog of misinterpretation was enormous. Do the models have a static bias? Yes, they do, because the most difficult aspect of social theorizing is accounting for the conditions under which change will take place, and these were not built into the model. Could components of the models be altered without unusual costs? Sometimes they could and sometimes not. Were essential elements of the analytical question omitted? Brewer says that this query is "not applicable," because there was, in fact, no sensitivity analysis. Can the models predict either future time series or those in the past from which their original data were taken? It turns out

that the models were generally made by using single-time points; not only could they not predict the future in sufficient detail to assist policy makers, but they were usually unable to explain the past very well. The future might well lie ahead of these modelers but the past, so to speak, was hardly behind them.

"Sadly," Brewer concludes to no one's surprise, ". . . San Francisco does not have an operating computer simulation model that can be reliably or routinely used for renewal policy-making. All claims to the contrary, the model is still nowhere near completion and has been set aside by responsible civic officials" (p. 114). The same is true for Pittsburgh, except that political circumstances led to the demise of the model's sponsors, so that there was no client to demand an end product that would not be forthcoming. Something of the pathos in the situation emerges from an account by a politician in Pittsburgh: He would call up to ask, say, how many vacant lots that might be suitable for housing existed in the 15th ward, and be told he would get the answer in an hour. A few days later he would call back, again without satisfactory results. "All they had," he told Brewer, "was a very elaborate reason why they couldn't get it. From the computer I got one of two things, either nothing and an excuse, or an answer that turned out later to be wrong" (p. 203).

Why did these efforts to improve municipal policy making through use of advanced techniques fail so abysmally? Some possible explanations may be broadly classified as political. Not all city officials were wildly enthusiastic about these efforts; their refusal to provide data, their unwillingness to supply support when needed, was certainly not helpful. The fact that the city financial contribution was largely illusory, representing a form of soft payment in kind but no outlay of hard cash, also meant that the cities risked little in contracting for these ventures. The cash nexus is not merely vulgar; it signifies a mutuality of interest that was evidently absent in these cases. Lacking a manifest stake in the activities, city officials were easily drawn into gaining rewards from their latent functions. They used what a consultant called the "Pinball Machine Syndrome"—whizzing colored lights— to advertise themselves as in the avant-garde of municipal reform. Computer simulation also serves the bureaucratic

function of keeping one's staff occupied and onlookers bedazzled, and can be invoked either as a rationalization for decisions already made or as an excuse for indecision and delay.

Political factors shade over into professional ones. No one knows exactly what a good simulation looks like and, as Brewer's book shows, it takes a long time and a lot of hard work to find out. Neither professional associations nor professional standards exist to provide guidance. The salesmen know what their product is supposed to do, but usually very little about how it will get done. Consultants know that they would like to try, but not whether they can do what is required. If the particular model in question has to be revised or extended, which is nearly always the case, the consulting firm can always promise to do the revising or extending for a fee, and city officials can always make another application to spend other people's money. When the final deadline approaches, the city will be given a product, though what it should be called is another matter: "It is my considered judgment," Brewer quotes an Arthur D. Little executive as saying, "that the entire future of ADL in urban planning depends upon delivering a workable CRP model to the City. How we define the expression 'workable' is something that must be thrashed out by you and the project team" (p. 150).

The purpose of analysis is not merely to find an answer to a preexisting question but to find a question that can be associated with an answer. The clients, as a consultant put it, "had no clear idea of how they wanted to use this thing at all" (p. 115); and how could they have when, as another consultant put it, "Let us be honest, we really didn't even know what the hell we were going to do" (p. 116)? Asking questions like "What are the city's goals?" produces an answer like "faith, hope, and charity" or its equivalent, "the best housing at the lowest cost for all of our citizens." No wonder, then, that each participant accuses the other of not giving the required instructions or failing to follow them. Eventually one of the modelers realized that "everybody was doing the project because he thought that somebody else wanted it done that way. I don't know of anyone who was doing the project because he wanted to do it this way. . . . It's hard to find out who wanted what. I don't know, maybe that's the problem" (p. 165).

In the end it is important to recognize that no one understood how the housing market operated. Existing theory was woefully insufficient for the purpose. The essential purpose of the models was (or should have been) to create the needed theory, but the kind of people hired to do the modeling were not experts on housing, and without knowing what they had to find out in the end it was not possible for them to do a good job in the beginning. Working under a deadline, without adequate support or instruction, some low-level operations researcher or computer technician inevitably makes fundamental choices on the basis of the only criterion he knows—running data in and out of machines.

The lesson to be learned from these unfortunate experiences is not that computer simulation cannot work but that it is not yet useful for policy purposes. Today no government official should expect to make practical use of computer simulation. It should be considered an experiment conducted in the hope of creating knowledge for the future, and local governments should be reimbursed for its costs. In time, advances in theory, in data collection, or in human cognitive abilities may overcome present incapacities. Ultimately new information systems, magnitudes more complex than the one we have been discussing, may prove efficacious. Maybe.

Still, the need is pressing, and nothing anyone says will stop people from trying an available product; so a few rough rules may be offered to guide government officials contemplating the installation of information systems. First, the rule of skepticism: no one knows how to do it. As Brewer's account suggests, the people most deceived are not necessarily the clients but may well be the consultants. Their capacity for self-deception, for becoming convinced by listening to their own testimony, should never be underestimated. Thus it may be less important to discover whether they are telling the truth than whether the truth they think they are telling is true. Unless the idea is to subsidize employment of social scientists, the burden of proof should be on the proposer. Second, the rule of delay: if it works at all, it won't work soon. Be prepared to give it years. Third, the rule of complexity: nothing complicated works. When a new information system contains more variables than, shall we say, the average age of the

officials who are to use it, or more data bits than anyone can count in a year, the chances of failure are very high. Fourth, the rule of thumb: if the data are thicker than your thumb (skeptics—see rule 1—may say "pinky") they are not likely to be comprehensible to anyone. The fifth rule is to be like a child. Ask many questions; be literal in appraising answers. Unless you understand precisely who will use each data bit, how often, at what cost, relevant to which decisions they are empowered to make, don't proceed. Sixth is the rule of length and width, or how to determine whether you will be all right in the end by visualizing the sequence of steps in the beginning and middle. Potential users of information should be able to envisage the length of the data flow over time, that is, who will pass what on to whom. If there are more than three or four links in the chain it is likely to become attenuated; data will be lost, diverted, or misinterpreted. The width of the chain is also important. If the data go to more than one level in the organization, the chances that they will be equally appropriate for all are exceedingly slim. The longer the sequence of steps, the wider the band of clientele, the less likely the information is to be of use. Seventh, the rule of anticipated anguish (sometimes known as Murphy's Law): most of the things that can go wrong will. Prepare for the worst. If you do not have substantial reserves of money, men, and time to help repair breakdowns, do not start. Eighth, the rule of the known evil. People are used to working with and getting around what they have, they can estimate the "fudge factor" in it, they know whom to trust and what to ignore. They will have to reestimate all these relationships under a new information system, without reasonable assurance they will know more at the end than they did at the beginning.

Ninth comes the most subtle rule of all, the rule of the mounting mirage. Everybody could use better information. No one is doing as well as he could do if only he knew better. The possible benefits of better information, therefore, are readily apparent in the present. The costs lie in the future. But because the costs arrive before the benefits, the mirage mounts, as it were, to encompass an even finer future that will compensate for the increasingly miserable present. Once this relation-

ship is understood, however, it becomes possible to discount the difficulties by stating the tenth and final rule: Hypothetical benefits should outweigh estimated costs by at least ten to one before everyone concerned starts seeing things.

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Virology and Cancer

The Molecular Biology of Tumor Viruses. JOHN TOOZE, Ed. Cold Spring Harbor Laboratory, Cold Spring Harbor, N.Y., 1973. xxii, 744 pp., illus. $16. Cold Spring Harbor Monograph Series.

Peyton Rous once told me that when in 1911 he discovered that a chicken sarcoma was caused by a virus (a discovery for which he received a Nobel Prize in 1966) an eminent pathologist stated that Rous sarcoma could not be a cancer since Rous had discovered its cause—and it was well known that the cause of cancers was unknown. That kind of reasoning did prevail a long time in cancer research. Slowly, however, there emerged a large body of regularities which forced even the metaphysicians to admit that cancers, like all physical phenomena, have causes and that some cancers are caused by viruses. More recently, the balance has swung, and a substantial body of oncologists are betting on viruses as *the* cause of all cancers— not, of course, by the rather trivial path of infection, as for measles, polio, or the common cold, but through more subtle relations between the cellular genes and viral genes obscurely hidden within the cells.

The present book, therefore, comes at a most appropriate time. It provides, in a detailed if not always delectable form, the essential background both on cells as responders to tumor viruses and on the viruses that have been incriminated as causes of cancer. It presents the various current theories and their justifications in an impartial although not detached way.

This book is clearly not meant for readers interested in an overview of the field. Its 13 chapters are decorated with sets of references ranging from 100 to 400 a chapter. Written mostly in 1969 and 1970 on the basis of two tumor virus workshops held at the Cold

Spring Harbor biological laboratory, it has been brought up to date to late 1972 and occasionally even later under the editorship of John Tooze. Twenty-two contributors are listed, interestingly, without attribution of specific sections, evidently because the rewriting was done by one or two people. This procedure is validated, in the opinion of this reviewer, by a homogeneity and excellence of style such as could hardly have been expected from 22 scientists.

Yet, the book, as stated above, is not a book for the biomedical public in general but for specialists, more specifically, for the young scientists ready to enter the exciting field of tumor virology as well as for all cancer workers wanting to be up to date in this forefront area of cancer research.

In the tradition of previous Cold Spring Harbor monographs—*The Bacteriophage Lambda* and *The Lactose Operon*—the present book is evidently meant to be useful. Much more than the two other monographs, it is effectively organized for use and study. It opens with a historical survey, already dense with current ideas, followed by two chapters on mammalian cells in culture and on cellular surfaces. Then it deals with the DNA-containing tumor viruses, adeno-, herpes, and "papova" viruses (this reviewer seems to be the last virologist left who refuses to use silly acronyms as names of viral groups), thoroughly exploring the phenomenon of cellular "transformation" to a malignancy-like form.

The last four chapters, on RNA tumor viruses, are of course the most intriguing, since it is viruses of this group (which includes Rous's original isolate) that are looked upon by some virologists as possible causes of human cancers as they are of cancers of fowl, mice, and other mammals.

The reader should be aware of a major source of the excitement that lies underneath the dry surface. The tumor viruses have not much RNA or DNA—maybe 5, maybe 10 or 15 genes. Any one of these genes may be "it": the gene that makes for cancer. The excitement of the tumor virus workers—the sense of zeroing in on one of the greatest and nastiest secrets of nature—projects itself only occasionally out of this book's factual presentation of the experimental landscape.

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