AN OPINION ABOUT THE FUTURE OF FOREST RECREATION RESEARCH

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ABSTRACT. A discussion of the research environment, with emphasis on the quality of future research. Some current research attitudes are criticized and a suggestion is given for increasing the value of research.

IN THE LAST several years, the concept of a *system* has received great attention in many different areas of research. Analytical enterprises falling under such headings as operations research, decision theory, cybernetics, dynamic programming, and information theory (all of which are heavily interrelated) depend upon a system within which to work. Thus, when considering problems of long-range planning for forest recreation, it is quite reasonable to approach them by researching the system in which they occur.

It is unfortunate, however, that the research and administrative agencies that devise and implement decision strategies also form a very large system. It is this point I wish to discuss first. In a recently published speech (Science 172:491-494; 1971) Prof. Richard Bellman of the University of Southern California, a pioneer in dynamic programming and recipient of the first Norbert Wiener Prize in Applied Mathematics, observed that:

> . . . we have begun to understand our society is a contrast of interconnecting, interacting large systems, and that so many of the difficulties that we see today are the difficulties, not of inherent theory, good theory, bad theory, not of conspiracy, but just the difficulties due to large systems. I think it's beginning to be realized that our systems are falling apart. We don't know how to administer them. We don't know how to control them. And it isn't at all obvious that we can control a large system in such a way as one remains stable. It may very well be that there is a critical mass—that when a system gets too large, it just gets automatically unstable. The problems then we see in our medical systems, in our educational systems, in our legal systems, in our transportation systems, in our garbage collection systems, all the systems you can probably think of, these are problems of instability.

If Bellman's conjecture about a "critical mass" is correct, it may very well be that the forest recreation system (a very large system) does not have a stable control mechanism. For such a system with unstable control I think the concept of "long-range planning" is at best a fantasy, and probably just meaningless.

Suppose, however, that the Forest Recreation (FR) system *does* have a stable control mechanism; and suppose further that the FR system and the nature of its controls are fully understood. The long-range planning and control of the FR system might still be jeopardized. The threat, in this case, would come from the stability problems inherent in the system that promotes research in FR, formulates planning for FR, and administers Control of FR. Call this the RPC system.

For an analogy, consider an automobile

as a system. As we all know, it is a reasonably small system and, under normal conditions, has a stable control mechanism. Now consider an automobile being operated by a committee of 15 people. Let different people control the accelerator, brakes, and steering wheel. Let some be in charge of looking out the windows and others of keeping an eye on the fuel gage and speedometer. Finally, let the rest of the unassigned committee members circulate around and help out wherever they see fit.

It should be clear in what sense the future of this car is in jeopardy. In reality, the situation would probably be worse because additional internal problems would arise such as a dispute over what constitutes safe driving, a contest between the gas-pedal man and the brake-pedal man to see whose function is more influential, or a front window viewer who just doesn't know the significance of ice on the road.

THE FR AND RPC SYSTEMS

It seems to me that, of the two systems—FR and RPC—the more sensitive and influential with respect to FR planning is *not* FR but rather RPC. Stated another way, my contention is that facts about the RPC system are more significant in the researching, planning, and controlling of the FR system than are facts about the FR system itself. There are even some aspects of RPCing FR which are completely independent of factual input from the FR system.

First, there is always the kind of situation in which a well-researched and well-formulated recommendation for planning or control is not implemented. Budgetary or manpower restrictions, previously adopted policy commitments, and even personal philosophies can easily cause a timely suggestion or plan of action to go unheeded.

Second, the selection and presentation of appropriate research is very susceptible to influence by the ideas and opinions currently embraced by members of the RPC system. A researcher, for example, may be unwilling to accept as valid the test results that falsify his conjecture, or may be too eager to accept as significant other results that support it. A planner, too, may be reticent to give adequate consideration to researched recommendations that, in his opinion, point in the wrong direction. As in any scientific enterprise, the results of an observation depend directly on the current theory, which means that it is very easy to see what you want or expect to see.

Finally, the evaluation as to whether or not a particular proposition or problem is researchable depends heavily on facts about the RPC system. More concretely, those areas of investigation that can qualify as targets for research are more likely to be found in the FR system than in the RPC system (which I have argued is the more significant of the two). It would be interesting to see more research on such questions as; "What is the actual working structure of the RPC system?", "What are the actual criteria used by its decision-makers?", and "Is the current hierarchical structure of the RPC system the one most compatible with the system's assigned functions?". I think questions like these will have to be dealt with if future planning is to be very effective.

In one sense, of course, there always has been and always will be long-range planning in the FR system. The sense I refer to is the stipulative or prescriptive sense of planning, such as planning a vacation or setting up a schedule for car payments. Such planning, however, merely predetermines certain aspects of the future and cannot, by itself, respond to the effects of any intermediate events. Notice also that under this interpretation almost any decision with long-range effects can be considered as "long-range planning."

At the other extreme, we could think of long-range planning as the formulation of optimal strategies giving rise to control that is sensitive and responsive to intermediate events. In this sense, long-range planning will probably never happen in the FR system unless drastically simplifying views of its structure can be found. The status of planning will always lie somewhere between the two extremes. To the extent that planning is going to be responsive, research will be needed to yield foresight and planners will have to ask well formulated questions. To the extent that planning is going to be prescriptive, planners will use research to vindicate their actions and researchers will produce hindsight.

A MODEST PROPOSAL

Somewhere along the line, the idea arose that for one's work to be "scientific," it had to be precise, cautious, and empirically well grounded. Apart from being false, this view is all too easily taken to mean "if it's wrong, it isn't scientific" (or words to that effect). One side effect of holding such a view is the production of "not-wrong" research, which I call "safe research."

Safe research is almost always publishable (as a research note if nothing else), and thus is of value to the researcher himself if to no one else. Safe research most easily arises by researching tractable questions rather than questions that need answers. In the event that no tractable questions are available, the researcher may well make up some, or worse, may simply produce the research results and then work backward to find the question that "was of interest."

If this last statement seems a bit harsh, I would point out that in a statistical analysis, for example, any dickering with the confidence levels after the fact may amount to exactly what I've described. In any case, in long-range planning and control, research that is safe in the short run is most probably *not* safe in the long run.

Scientists, however, are people. They realize that their survival as scientists depends not only on their scientific skills but also on their ability to serve and preserve the hand that feeds them. In the opinion of J. R. Pierce (Bell Telephone laboratories, Murray Hill, N. J.) which appeared as a recent editorial in Science magazine (April 1971):

> In the end, most scientists will do whatever there is money for doing. Scientists know, or should know, which socially and economically useful goals are within reach and which have a good chance of accomplishment through promising research. Yet, in their personal and collective actions, scientists often seem more concerned with the total number of dollars, with the public image of science, and with the cry for certain specific results than with the sensible selection and vigorous pursuit of fruitful areas of research and application. It will be a sad thing for scientists if they fail to choose wisely and act energetically toward valuable and attainable goals—for, if they do not choose what they shall do, others will choose for them.

The initiative for change, then, is to be placed most squarely upon the shoulders of the scientist. But what is a reasonable first step for him to take?

My recommendation for improving the situation in forest recreation requires individual participation on the part of researchers; and moreover, since good research cannot be legislated or capsulized into a research cookbook, each researcher's participation will have to be unilateral. He will have to stand alone. I ask that he make a concerted attempt to produce *fully interpreted* research results.

This involves suggesting applications and speculating on outcomes. It involves saying things that might be wrong rather than safe. If he uses factor analysis or least-squares curve-fitting (or both), he might spell out what he feels the factor loadings signify in his case (if anything), or in what way he would recommend using his regression equation.

Instead of politely backing off from the question of application, the researcher should be aggressive and should be willing to go out on a limb to a certain extent. He might also include an estimate as to when his data will no longer be accurate ("Is it good forever?"; "Is it good for 3 or 4 more years?"; "Is it already inaccurate at the time of publication?").

Please notice that I am *not* asking the researcher to be correct in all his speculation. I am simply asking him to shoulder his fair share of the risk involved in the application of his own research. It is hardly fair to ask a planner to place confidence in someone's research if the researcher himself isn't willing to do so. Even though some good research will not have immediate or imminent application, I think that a researcher should consider himself co-responsible for any application (or *lack* of application) of his work.

Such an attitude would lead to the appearance of more publications in which the investigator could conclude by saying "more applications in this area are possible" instead of the usual "more research in this area is necessary."