ECONOMIC RESEARCH PROGRAMS FOR COMMUNITY DEVELOPMENT -- WHY DO IT?

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> We can no longer afford to approach the longer-range future haphazardly. As the pace of change accelerates, the process of change becomes more complex. Yet, at the same time an extraordinary array of tools and techniques has been developed by which it becomes increasingly possible to project future trends -- and thus to make the kind of informed choices which are necessary if we are to establish mastery over the process of change.
>
> --President Nixon announcing formation of the National Goals Research Staff.

My task is to discuss the research implications of the framework for Community Development. I will discuss research -- about some of the ground rules for research on development problems; about some of the difficulties researchers face today; and list some criteria for research that I think might improve the end product.

The strategies for community development that underlie most of our activities in this area tend to be partial strategies and to be reflected mostly in individual programs. These are generally directed at specific, narrow problems -- low income, housing, chronic unemployment, transportation, availability and cost of electricity to name a few. Such programs are numerous and various, designed for different problems over the last 40 plus years.

There has been no authority to fully consider the interdependencies among problems and among programs. Therefore, there is not a general strategy for development, as such. The programs have various central purposes and they have a range of developmental impacts. They share a common characteristic of dependance on economic trends for the main thrust of development, with attempts to guide or change trends through various kinds and amounts intervention. The discipline of the budget works to hold the intervention as small as possible. In this framework, the problem is to develop as much "muscle" as possible given the discipline of the budget. One issue is whether or not the priority of "development" justifies larger expenditures and higher risk of violating the "minimum" rule, and sub-issues involve priorities among places and among needs in each place. Theoretically, at least, research should shed light on these crucial questions.

While the intervention has been incremental, the forces of change have been massive, even overwhelming for many communities. The problems resulting have been, and are, critical in every human dimension. They are often reflected in low incomes, inadequate services and an unsatisfactory quality of life. These characteristics can be seen to some extent in almost every community, but they are also very heavily concentrated in some communities. We have concentrated our research to a great extent on these very real human problems. The research has tended to be descriptive and has not adequately considered the interaction among problems. Thus, it has most of the conceptual faults of the programs themselves.

THE STATE OF RESEARCH

There is, in the Capitol and around the country, more interest in rural development and economic development research than at any time in my experience. We have better trained researchers today than ever before. Funds available across the Nation for economic development research have increased sharply with further increases expected.

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But how well are we really doing? What is the state of research in general and development research in particular these days? Are things as rosy as we might have some reason to expect?

As always, all returns are not in. But evidence is mounting and mounting rapidly that research in general and social science research in particular faces serious problems. From the discussions I hear and papers I read, the problems are upon us now.

The 1970 report of the National Goals Research Staff, for example, focuses on eight emerging national issues. Fourth among these, following population, the environment, and education is the question of science. The discussion concerns basic natural science, but most observers agree that it applies with equal force to social science research in general. The proposition is that from World War II until the mid-1960's it was generally agreed that science should grow according to its own internal logic as dictated by the structure of the evolving knowledge and the criteria and judgment of the scientific community. Today, they say, the relationship of the scientific establishment to its funding is being reversed. In addition to skepticism among the general public concerning the capacity of science to accomplish objectives, there is a real and growing concern that the knowledge developed will be used for ends they do not approve. I suggest that there is general awareness among researchers that the climate is changing. I would like to identify some of the changes I see.

Researchers, economists among them are as mystical as ever with inputs and outputs and models and magic in between, but the confidence and awe the public had as late as 1969 that we really could put a man on the moon because researchers said we could has vanished. Today, the statement that we can put a man somewhere in 10 years is met with a question – Why do it?

With that question, researchers and the rest of the world tend to part company. Too often the "why do it" question is considered outside our job description. From the research point of view, it is obvious that the job needs doing. We are not proposing to break the bank with the project, and we intend to be quite reasonable about the resources employed. Therefore, it is a good thing to do and a shocking and discouraging thing that serious questions would be raised about not doing it.

As researchers, we are inclined to say to ourselves and each other that the skepticism about research is part of the popular and general discontent arising from a troubled moment in history. But that is a superficial view. The skepticism is real and it is deep. Research, development, progress, and growth are part of the change processes we have been caught up in and which have been characteristic of our society in the third quarter of the 20th century. Cataloging the changes, measuring the rates and searching for the causes is a popular activity among both casual observers and serious scholars alike. The national debate over the causes and effects of change and the best prescriptions for the problems accompanying growth and change, as well as many long standing and persistent problems that "have always been with us," involves national priorities and goals. It is very serious business indeed and a proper matter of concern for researchers.

While all researchers must be concerned about the issues of that debate, I do not see economists as the central target. I argue that the skepticism we face arises from the question of whether or not the things we do are worth the cost. Consider the question of whether or not research has solved the farm problem; raised farm income; or caused rural development. If you are a researcher who has much occasion to design and justify economic research programs on rural problems, I expect you face these questions regularly. The public assumes that research on farm income and farm policy is designed to increase farm income and improve farm policy – and that community development research should lead to community development. Here, I suggest, is a credibility gap that is largely our fault.

It is our fault because we are not communicating well with the public, either in terms of what research should be undertaken or in describing what can be done and what should be expected. I think a little diagram used by social psychologists to describe one kind of information flow is very useful in describing the problem:[1]

The Johari window is essentially an information processing model. The four-celled figure is designed to reflect the interaction of two sources of information–in this case something called the "research institution" and something else called the "public." The content of the model is pieces of information available for use in establishing relationships between the institution and the public. The squared field represents a kind of interaction

[1] *The Johari Window: A graphic model of Interpersonal and Team processes* as used by Jay Hall and Martha Williams in their "Personal Relations Survey," Teleometrics International, Conroe, Texas. The concept was originated by Drs. Joseph Luft and Harry Ingham for programs in group dynamics training – Joe and Harry – thus the name "Johari" window.

space. Each of the four regions represents a particular combination of relevant information with special significance for the quality of the relationship.

I am thinking of this model from the point of view of the "research institution" in the broadest sense–the universities, the institutes, the USDA, the foundations, etc. The argument can be sharpened as the model focuses on more specific targets, and as it focuses on different targets such as "research" so that the unit of observation is a body of research information, goals, processes, and results. But for this discussion I am thinking in extremely broad terms of the operational Institute.

The "Arena" is the sector where both the Institution and the Public know what's going on – the "Facade" is an area of activities where the Institution knows, but the Public does not. The "Blind Spot" includes information about the Institution that the Public knows but the Institution does not – and the "Unknown" quadrant includes those things concerning the Institution that neither the Institution nor the Public knows.

All four of these quadrants are well known to us, and clearly the more pieces of information that fit into the "Arena" area, the better the communication is – and remember this is basically a model about communication.

The "Facade" is an important area. It includes most basic research because of the complexity of the inquiry, but it also includes a lot of research that could and should be understood by the public. The "Blind Spot" is also recognizable. It includes a lot of elegant research that leads to trivial answers and all those conclusions based on ceteris paribus and perfect competition assumptions (among others) that researchers make that the public either intuitively or by experience knows do not fit.

The operating assumption is that the larger the "Arena" quadrant and the smaller the other quadrants, the better. Furthermore, I assume that by certain processes the lines that divide the quadrants can be changed. The "Facade" quadrant can be reduced by information; by education; by public relations; and by other activities designed to expose what is behind the wall – the "Facade."

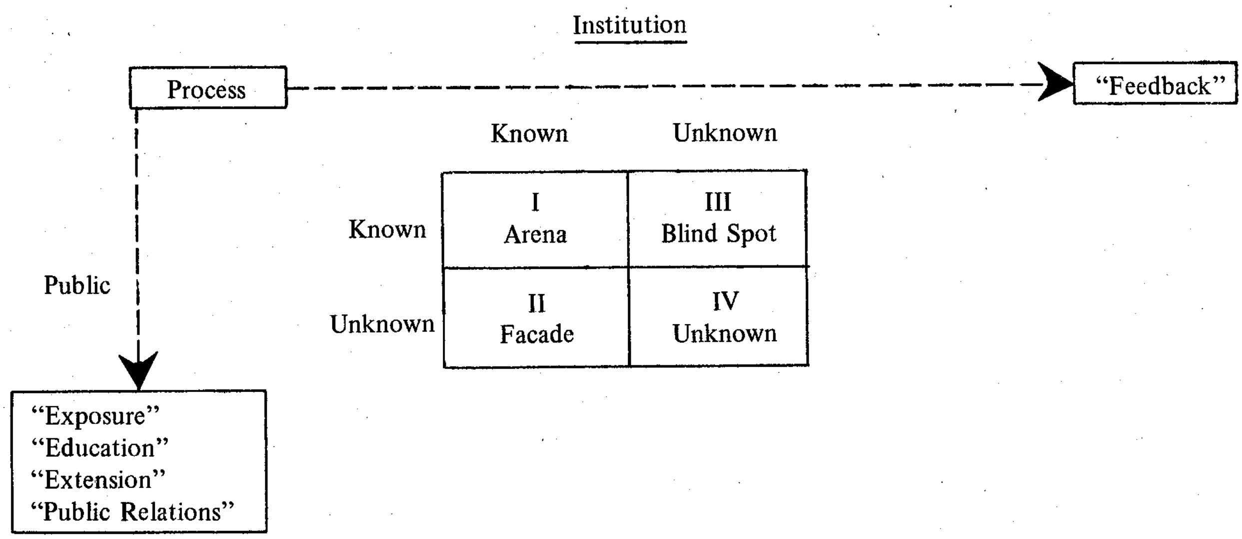
The "Blind Spot" can be reduced by observation and by feedback.

I believe the most interesting aspect of this little model is the proposition that the institution is not really very good at discriminating between the "Facade" and the "Blind Spot." As a result, we undertake information and education efforts when we should be thinking about feedback.

Because of the difficulty we have telling the "Blind Spot" areas from the "Facade" areas, we tend to believe the "Facade" area to be larger than it is and the "Blind Spot" smaller. The result is that we paint ourselves into corners.

From my observation, we do this in at least four ways:

1. We design and redesign research of all kinds in elegant and abstract terms at the expense of a lot of burning local and national issues.
2. We describe human and community characteristics and problems such as income, taxes, and housing, demographic trends and highway expenditures, but do very little in terms of workable strategies to lead to better development – or development at all.
3. We avoid fundamental causal relationships because they are messy – and we stop with our input-output coefficients and shift-share analyses long before they provide real evidence useful for policy or administrative decisions.



4. We are satisfied with vague conclusions -- generalized data across areas using averages of old observations.

We make these mistakes when we are operating in the "Blind Spot" but acting as if more education and information would move us into the "Arena," when in fact only a proper mixture of feedback and exposure will do the job.

The question of what research can and cannot do is probably our biggest "Blind Spot." Researchers and decisionmakers know very well, for example, that at least two important conditions must be met before research can solve any problem: the issues must not involve conflict and the conclusion must be acted upon. This implies that the system is willing to ask hard questions and act upon hard answers.

Obviously, research cannot solve a real conflict. It should not be expected to. It can show where conflicts do not exist and reduce conflict from imaginary to only real issues. But the obvious potential for even increasing conflict as research illuminates issues is real. At least such battles are fought for the "right" reasons.

But the fact of this limitation of research is a kind of unspoken wisdom. As a result, the problem solving capabilities of research have been oversold in many cases.

PPBS is an example of improper billing for social research. Many thought PPBS could solve problems, make decisions, and ensure good government. Naturally it could not, and a popular game nowadays in and out of Washington is to hunt down those who oversold PPBS the most. In my view, everyone oversold it in the late 1960's and about that many are underselling it now. PPBS and social research can clearly add a lot of information to the system -- and it can lead to better decisions, if properly presented and properly used. The responsibility for its use must always hang on the administrator, for whom it should mean better decisions, but not necessarily easier ones.

Information about what research can do and what it cannot do falls both in the "Facade" and in the "Blind Spot" quadrants. It is easy to mistake the "Blind Spot" for the "Facade" and to instruct people about what research can do without getting enough feedback on things research. Institutions are not doing very well.

We rationalize our behavior in at least the following ways:

1. Social problems are complex and extremely difficult to unravel in cause and effect terms. They involve human values which change and they involve conflict and other messy things. So, we say, we need our "Facade."
2. Because they involve conflict and uncertainty, this kind of research risks the wrath of the public and more importantly, administrators and legislators. Again, we use the "Facade."
3. Rural development research, as is the case with policy research in general, is often concerned with intervention in governmental decisions somehow. The questions of where and how and how much are very forbidding ones. Revenue sharing vs. central federal programs is a real issue. Researchers can say much about anticipated outcomes. But how and where to focus on the system is difficult to know and evaluate, and another high risk operation. Here we plead to be either in the "Facade" or "Unknown" quadrants.

How do we get the feedback we need to (a) tell us when we really are in the "Blind Spot" quadrant and (b) move the lines so that more things really fall in the "Arena" quadrant and fewer in both the "Facade" and the "Blind Spot" quadrants?

I suggest the first step is application on the part of research directors of some tough tests as to whether or not research projects ultimately lead to what Jim Hildreth[2] calls "well being." Hildreth has characterized the ideal system of publicly supported research as comprised of a chain of boxes containing things researchers do. The first box contains "ideas and systems of thought" and the last "well-being of people." In between are boxes labeled "definitions," "analysis," "conclusion," "policy dialogue," "decision," and "action." Hildreth starts with the proposition that publicly supported research should benefit the public. Therefore, efforts must somehow affect the last box and presumably, pass through most of the chain. But too much research starts and ends in box 1 and 2 or perhaps box 3 -- a great deal of it concentrating entirely on the system of thought (box 1) and more still on problem, analysis, and empirical research (box 2) with much time and effort spent in policy dialogue (box 3) which can be endless. Hildreth correctly points out that the pay-off from these activities is private until you begin affecting box 6 -- well-being of people. He postulates four "Hildreth Dicta," the first three of which consist of knowing which box you are in and getting on from the one to the next. The fourth is to maximize the ratio of public output to private output. This is strong medicine and ought to be taken very seriously. I agree with it wholeheartedly.

[2] R. J. Hildreth, The Farm Foundation, "So What" (unpublished).

I think at least part of the remedy involves several hard tests of reality to be applied to potential research projects if we are to provide real guidance to the development efforts. The first is perspective. We are too prone to examine a problem and conclude that the only solution is massive inputs of outside money. This may in fact be the only answer. But that answer has been given too often. There is not now, and is not likely to be, that much federal money forthcoming. Perhaps there cannot be that much federal money. Thus, that answer is in many instances no answer at all. What is the next best solution, and what are its pros and cons? Usually we do not say, often because the question is not asked.

The second point is that most answers are partial answers. Housing is a partial answer. So is education because education without a job is surely a problem. So is growth in jobs because all the jobs may go to nonresidents, and so is growth of local jobs if nothing is done about local services. The projects are partial in order to make them manageable but they may also be trivial if no one "puts it all together" and makes it available to those who must make decisions and who can implement a broad strategy.

On the question of perspective, a rule of common sense is called for. The partial views that have been all too common are almost always too narrow – but we cannot examine the whole world in each project. A middle ground with a broader view, but still manageable project system is called for.

The question of project priority is always difficult -- perhaps a common sense test again is the best answer.

In addition to test of relevance, perspective, and priority, there are other tests of project effectiveness, timeliness, completion time, coordination with others and a long list of good things that make good projects good. But a final test I want to mention is for critical mass.

As I see more research and gain more experience in developing research, I am more and more impressed that some projects develop as much more than the sum of several individual efforts and others fail to get over the relevance, timeliness and usefulness threshold because they lack critical mass. They lack resources to tackle enough meaningful questions in a short enough time period to allow real and helpful conclusions. The working parts lack the capacity to test themselves and each other. They lack the ability to try out ideas and assumptions on real people in real communities. They lack the critical mass of people and money that can make the product consistent, useful and complete.

I suggest further, that we do not know how to test for this critical mass at a time in the development of the project when adjustments can be made.

This is a threatening concept, to a small extent, because even if we knew how to apply the test, we do not yet have the means for a solution. That is because Directors, including myself, frequently are not willing to put enough chips on one project (given all the risks that entails) and figure out how to coordinate and run the efforts of several researchers so that they truly focus on an interlocking set of relationships at the same place and at the same time. This is tricky business. It is said to infringe on the initiative and even the rights of researchers. It involves tough problems of professional recognition and research. But I wonder if we can any longer afford the luxury of those research terms.

In a kind of summing up, I am arguing that we have not communicated well those things that we best do. But the greater need of designers of economic research is to carefully allocate our scarce research resources among problems and projects with the greatest probability of improving the well-being of people. I am suggesting that some basic tests applied to new projects plus a willingness to design according to the scope and perspective of the problem set will help make research more relevant, and that relevant research is a scarce and singularly beneficial commodity at this point in the Nation's history.