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Social Inventions for Solving Human Problems

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This is a time for rethinking sociology. In President Reagan's initial budget proposal for 1982, we were told, in effect, that what we (and other social scientists) do in research is of little relevance in solving national problems. On past performance, we do not warrant that judgment, but let us not expend our energies in defending past accomplishments. We must do better in the future to demonstate the practical relevance of sociology.

We can meet that challenge if we reorient the way we do sociology. I suggest that we conceptualize this focus in terms of the discovery, description, and analysis of *social inventions for solving human problems*.

Let me start with a definition. A social invention can be

—a new element in organizational structure or interorganizational relations,
—new sets of procedures for shaping human interactions and activities and the relations of humans to the natural and social environment,
—a new policy in action (that is, not just on paper), or
—a new role or a new set of roles.

We can leave it to the historians to determine whether the social invention we study is *new* in the sense that nothing quite like it has ever been done before in the history of mankind. For sociologists, the important point is that the ideas underlying the invention are new to the people involved in developing and applying them. Even if they have consciously copied from elsewhere, at least they had to adapt the copy to their own social, economic, and cultural environment.

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Before going farther, let me distinguish between *invention* and *intervention,* two words that sound similar but have different meanings.[1]

Whatever else it may be, an *intervention* is something brought into an organization or community from the outside. An *invention* is a new creation which may and often does emerge in a community or organization, without any direct outside influence. While an *intervention* may indeed involve the introduction from outside of what I would call a *social invention,* I am here primarily focusing on inventions more or less autonomously created within the organization or community in which they are utilized. Quite apart from concerns over terminological exactitude, I emphasize the autonomous creation of social invention to suggest that human beings have enormous resources of creativity that permit them to devise their own social inventions, without waiting for an outsider to intervene and invent what the community or organization needs.

To illustrate the potential utility of this line of research, let me focus in some detail on the study of social inventions in two fields of human affairs quite separate from each other: worker industrial production cooperatives in Spain and agricultural research and development organizations in Latin America. In both of these studies I have done field work myself, but the more systematic studies have been carried out by younger colleagues (Ana Gutiérrez-Johnson and Lynn Gostyla) who have also made major contributions to the analysis.

THE STIMULUS OF MONDRAGÓN

I first began to focus my ideas in this direction when I presented a paper (Johnson and Whyte, 1977) on the Mondragón system of worker production cooperatives at the 1976 meeting of the American Sociological Association.[2] I was excited by what we were learning regarding a system of worker production cooperatives in the Basque country of Spain that had been started by five men in 1956 and two decades later had expanded to 65 cooperative production firms with 14,665 worker-members. By the end of 1980, in spite of a serious slowdown in the Spanish economy, the system had grown to include 87 worker cooperatives with over 18,000 members.

Since the conventional wisdom held that worker cooperatives were a form of industrial organization that had little promise of success (Blumberg, 1968:3-4), it seemed important to let my colleagues know about Mondragón.

When later published, this article did indeed attract a good deal of attention, but I was taken aback by the reception the paper received from one of the discussants. The critic denied that the case had any general significance. In the first place, he attributed the success of the Mondragón system to the Basque culture, whose peculiar features could not be duplicated anywhere else in the world. In the second place, he cited the contribution of an extraordinary individual, José María Arizmendi, who was indeed the founder of the system and the principal person who guided its development until his death in 1977.

Let me deal first with the cultural determinism argument. To be sure, there is a cultural base to the Mondragón system, but that base accounts only for the Basque inclination toward cooperative forms of organization. Culture explains neither the structural form of the Mondragón system nor its extraordinary growth.

Basques themselves are conscious of this cultural base; they speak of "our associative tendencies." Indeed, one author (Johnson, 1982) claims that there are in the Basque provinces many more worker production cooperatives outside of the Mondragón system than within. Therein lies the clue to the limitations of the cultural explanation, for we find that these other cooperatives have not joined together to constitute integrated and expanding organizational systems. Rather, they conform to the traditional Basque pattern: small, closed social systems with strong in-group solidarity. Mondragón departs markedly from this traditional culture pattern. It is an open system, constantly expanding in individual members and in the number of firms it links together. In Mondragón internal cohesion is balanced by strong linkages of the firms with the communities out of which they have arisen. No cultural explanation can account for the basic differences between the Mondragón system and the traditional Basque cooperatives.

Let us now consider the limitations of the great man theory of the history of Mondragón. Father José María Arizmendi was indeed an extraordinary human being, and I counted it a great privilege to have several conversations with him. However, let us shift our focus away from the personality and character of the founder and concentrate upon the design of the organizational structure, on interorganizational relations, and on sets of procedures for shaping interactions, activities, and relations of humans to the natural and social environment. If we look for innovations in social policy and in the creation of new roles, we can visualize Don José María and his associates as the creators of a series of social inventions. While it may take an extraordinary individual to create an important social invention, it should be possible for mere mortals to identify that social invention, describe it, and analyze it in ways that will permit others to adapt it to their own interests and needs.

I shall illustrate by describing two sets of social inventions contributing to the growth of individual units and the development of the total system.

1. The legal and financial structure of the Mondragón firm solves problems which have led to the disintegration of many worker cooperatives all around the world. When a worker cooperative is built on the basis of member stock ownership, it can go out of existence either because the firm goes bankrupt or because it is highly successful. In the latter case, as the firm becomes more prosperous and expands, what we call "collective selfishness" takes over. The original members realize it is to their financial advantage not to dilute their equity by sharing ownership with newly hired labor. When worker-owners retire and want to sell, their stock has so increased in value as to be beyond the reach of nonowning workers, and so inevitably that stock shifts into the hands of private investors.

In Mondragón, the financial instrument is *debt* rather than *equity*. Each member's initial contribution is conceived of as a loan, to be deposited in the worker's account with the firm. Furthermore, members' shares in the annual profits or surplus of the firm are not distributed in cash but are deposited to member accounts, where they are used by the firm to finance its future growth. Each member account (initial contribution plus profit sharing plus interest) constitutes a fund available in full to the worker upon retirement and which can be withdrawn, with certain deductions, if the member leaves before retirement age.

Many worker cooperatives have failed because, in good times, they have distributed profits in cash to members and have not retained in the firm enough money for reinvestment and as a reserve fund against future losses. The Mondragón legal and financial inventions provide strong protection against such disasters.

We estimated in 1976 that roughly half of the extraordinary growth of the Mondragón system was accounted for by the reinvestment of 70% of the profits each year in the member accounts—beyond the 15–20% always retained in a reserve fund.

2. The other half of the system's growth was provided by loans from **Caja Laboral Popular,** a cooperative bank founded in 1959, three years after the launching of the first worker production cooperative. A cooperative bank or credit union was certainly not a Mondragón invention. However, up to that time in Spain as well as elsewhere, credit unions had been utilized for individual savings and for loans to finance the purchase of consumer goods. While the Caja does have individual savers who deposit money and take out personal loans, its primary purpose has been to finance the creation and expansion of industrial cooperatives, consumer cooperatives, housing cooperatives, construction cooperatives, and education cooperatives.

The Caja has experienced extraordinary growth. By the end of 1980 it had 300,000 individual depositor-members and capital close to one billion dollars. The Caja has also invented programs not ordinarily associated with banking institutions. It has an entrepreneurial department with a staff of over 100 to provide organizational skills and general technical assistance in working with cooperative organizations in the process of formation or expansion.

The Caja has also proven to be a key factor in linking together into a single system so many industrial production cooperatives. Besides providing technical assistance to the group seeking to create a cooperative firm, the Caja lends 60% of the initial capital needed and supplies additional credit to cover the losses of the new firm for the first two years of its existence. As a condition for receiving this assistance, the initial members of the firm establish a constitution and bylaws compatible with the framework established by José María Arizmendi and the cofounders of the first firm. Thus, while each firm is completely autonomous in its internal operations, the common organizational framework and financing mechanisms make the difference between isolated firms and an organizational system. Furthermore, besides providing management consulting services, the Caja helps individual firms to band together to share costs of services such as personnel, legal, purchasing, and marketing.

INVENTIONS IN AGRICULTURAL RESEARCH AND DEVELOPMENT[3]

In recent years I have been working closely at Cornell with an interdisciplinary group of professors ranging across the plant, animal, and soil sciences and including economists, political scientists, and anthropologists. What brought us together was the common recognition that, except where they have access to irrigation, small farmers in developing countries have so far received little benefit from the great scientific advances of the "green revolution." In working together, we were moving toward a common diagnosis of the problem of extending the benefits of science and technology to these limited resource farmers and were also designing a new model for agricultural research and development. While I have profited greatly by information and ideas brought to me by colleagues with experience in Asia and Africa, I shall illustrate this process of diagnosis and reformulation in terms of my own field experience in Latin America.

Before we can identify a particular social invention in agricultural research and development we have to visualize the traditional system for carrying out these activities. According to the research literature and my own field work in Latin America, the traditional R & D system had the following characteristics:

1. Agricultural research was concentrated in experiment stations.

2. The links between agricultural research and extension have been weak. Researchers tend to look down on extension agents, and the agents often complain that researchers are too interested in scientific purity to give attention to practical applications. This is part of a broader problem of lack of coordination among government and private agencies. Small farmers have often found that the recommended inputs to improve their farms are not available in local markets. If they can get the credit they need to follow extension recommendations, the money generally arrives much later than the optimal time for its use.

3. The activities of extension agents and the flow of credit have tended to be concentrated especially upon the more affluent farmers.

4. Extension agents have generally followed a communications strategy, disregarding the social organizations of rural communities. That is, they have

customarily dealt with farmers on a one-on-one basis or through public announcements and demonstration projects.

5. Social scientists, studying the diffusion of innovation, have defined the problem as *resistance to change*. That is, they have viewed peasants or small farmers as being locked into their traditional culture and therefore inclined to reject improvements that would raise their standard of living. In this framework, the problem is how to overcome resistance to change by these irrational or nonrational small farmers.

If that is the general pattern you perceive, what does it suggest for the next steps in research? I see my own exploration and analysis proceeding through the following four stages.

1. Discovery of the Fallibility of Agricultural Professionals. Some years ago in Peru I began to hear of cases in which small farmers had followed the recommendations of agricultural professionals and had suffered disastrous results. This stirred my interest, and I was then able to document similar events in Africa, Asia, and elsewhere in Latin America. Now I asked myself, why was it that such blunders of agricultural professionals had been news to me?

The conventional research strategy assumed that the recommendation offered had been practically and economically sound—which is the same thing as assuming the infallibility of the professional. Therefore, if the small farmer rejected the innovation, he could not do so on rational economic grounds. This led naturally to what I have been calling "the myth of the passive peasant." The causes for "resistance to change" cannot be located in the impracticality of the innovations recommended but must rather be found in the culturally determined psychological makeup of the small farmer.

Now I was also learning what factors could lead the agricultural professional into serious errors. In the first place, years ago professionals seriously underestimated the effects of the enormous variability in soil, water, and general climatic conditions that affected farmers from one region to another and even in different parts of a small geographical area. If the professional had not tested his recommendation in the same area where it was to be applied, the chances of being wrong were very high indeed.

2. On the Value of Peasant Ideas and Information. Now I asked whether small farmers might have valuable information and ideas that the professional lacked. I found the value of peasant information and ideas demonstrated most dramatically in the famed Puebla project in Mexico, the International Maize and Wheat Center (CIMMYT: 1975). Located in one state of Mexico, the project's purpose was to demonstrate how small farmers could increase their yields of corn.

Puebla was primarily a demonstration project rather than a research project. The designers of the project thought they already knew how to help the small farmers grow more corn, and the research was designed primarily to measure the effectiveness of this *technology transfer* project. The project involved methods of growing corn but also assistance in securing credit for the farmers, who would need additional funds to buy the inputs necessary.

Over a period of several years, the project enrolled increasing numbers of farmers and demonstrated approximately a 30% increase in corn yields—significant but hardly impressive compared with results then being achieved with high yielding varieties of rice and wheat. Furthermore, the number of adopters of the Puebla project technology, as measured by the number of farmers receiving credit through the program, leveled off at a point where only a quarter of the corn farmers in the program were participating. This led the Mexican professionals implementing the program to wonder why adoption was not continuing to grow. Rather than falling back on the traditional explanations of peasant traditionalism and resistance to change, they went out into the field to observe and study the nonadopters.

The revelation that was key to my reformulation was put to me in this way by Mauro Gomez, Puebla Project General Coordinator (1970–1973):

> In Mexico we have been mentally deformed by our professional education. Without realizing what was happening to us, in the classroom and in the laboratories we were learning that scientists knew all that had so far been learned about agriculture and that the small farmers did not know anything. Finally we had to realize that there was much we could learn from the small farmers.

To oversimplify a complex set of results, the Mexican field workers discovered that the basic reason some of these farmers were not adopting the Puebla recommendations was because they were making twice as much money following their own methods. The Puebla program was geared to the monocultural production of corn—that is, planting corn in rows with no other crops between the rows. The most successful nonadopters planted beans between the rows of corn. They were making much more intensive use of their small plots of land and also making more efficient utilization of fertilizer, which now served two crops instead of one.

When the Mexicans made this discovery, they naturally asked themselves, "Why is it that we have been telling them they should not plant anything between the rows?" Not finding any rational explanation, they recognized the force of tradition: "That is not the way they raise corn in Iowa." In other words, their recommendations had been based implicitly upon the model of farming in the

Midwest corn belt. The monocultural strategy made sense in Iowa where affluent farmers plant large acreages of corn and use tractors, which require space between the corn rows for most operations.

Promoters of the Puebla program realized that, where land and capital were in short supply and labor was relatively cheap and abundant, tractors were not practical. However, instead of recognizing that the rationale for leaving empty space between rows of corn depended upon the use of the tractor, they simply proceeded on the implicit assumption that, since the Iowa corn farmers were highly efficient, their methods must be applied in Puebla. In this case, we must ask, who is tradition bound, the agricultural professional or the peasant farmer?

The traditionalism of the agriculture professionals is strikingly illustrated in the basic Puebla report of CIMMYT, the International Center for Maize and Wheat Improvement, which was responsible for the project. The report has a schizophrenic character. The authors faithfully describe the slowing down of the adoption process and the discovery that farmers intercropping beans and corn were making up to twice as much money off of their crops as those who faithfully followed the Puebla recommendations. Furthermore, they describe how, three years after the project's beginning, project officials endorsed intercropping and stimulated the growth of organizations of corn and bean farmers. At the end of their report, however, they arrive at this extraordinary conclusion:

> Clearly, the job of adjusting and delivering adequate technology, as well as that of inducing farmers to use the recommended technology is very difficult and it is far from being accomplished in the Puebla area. (CIMMYT, 1975)

Of course, it is difficult to persuade small farmers to adopt a new agricultural technology when it promises to cut their crop income in half. How could a group of highly competent agricultural professionals come to a conclusion that bore no relation to the facts they themselves had documented? To answer that question, we must distinguish between accidental and systemic errors. An accidental error is one which may be committed by anyone at any time; a systemic error is caused by the *conceptual scheme* the researchers bring to bear upon the problem. The designers of the Puebla Project began with a conceptual scheme that visualized the development process as a problem in the *transfer of technology* in ways designed to overcome the *resistance to change* on the part of *passive peasants*. Their own findings were clearly incompatible with such a scheme, but, as historians and sociologists of science have noted, the discovery of discrepant facts may not be sufficient to overthrow an established theory—or, I would add, a more modest construct such as a conceptual scheme. Especially in the early stages of the development of a science, those attached to the established paradigm tend to ignore the facts that don't fit or to make patchwork adjustments in the theory. A popular theory is discarded only after it is confronted with overwhelming contrary evidence *and* the emergence of a new theory which fits both the old facts and the new facts that have now become available. The writers of the Puebla Project report stayed with their defective conceptual scheme because they had nothing better to put in its place. It remained for those who followed their pioneering work to move toward the new paradigm.

These reflections prompted me to abandon any thought of further studies of peasant resistance to change. I did not go to the other extreme to assume that small farmers are always eager to embrace change but simply assumed that, in general, they are no more resistant to change than are the professionals in their own field of activity. Therefore, when small farmers decline to adopt a proposed innovation, we can generally assume that either (a) they have enough sense to know it would not work, or (b) they lack the money to buy the necessary inputs or cannot get those inputs in local markets.

3. The Active Participation of Farmers in Agricultural R & D. Such analyses led me to the conclusion that effective R & D systems in developing countries must involve the active participation of small farmers in the R & D process. Cases along this line are beginning to appear all over the world, but here I shall concentrate on my own studies, first in Guatemala and then in Honduras (Gostyla and Whyte, 1980; Whyte, 1981).

The new program began in Guatemala with the creation of ICTA, the Institute of Agricultural Science and Technology, out of what had been a sprawling agricultural bureaucracy. ICTA was designed not only to do more and better research but especially to do research in ways that would benefit small farmers.

The designers of ICTA recognized that, if their program was to meet its new responsibilities, more of its research must be done on the fields of small farmers. While abandoning total reliance on the artificial setting of the experiment station was a necessary condition for aiding small farmers, a mere change in location of research was not sufficient to reach the new objectives.

As various observers have noted, the conventionally minded plant scientist, when ordered to do his experiments on the fields of small farmers, will try insofar as possible to approximate the control over the experimental process which he enjoyed on the experiment station. If the new location was to provide information beyond the response of crops to different soil and climate conditions, then ICTA needed to devise a set of social inventions through which small farmers could play active roles. Furthermore, in order to provide a realistic base for these field experiments, Peter Hildebrand and his Guatemalan associates had to develop social inventions to provide them with baseline information on the nature of the indigenous farming system practiced in the area studied.

The Socio-Economic unit, organized by Hildebrand, began field work by delimiting an area of study within which the small farmers were practicing basically similar farming systems—that is, planting the same crops, using roughly the same inputs and systems of cultivation. In such an area, any improvement developed on one farm would have wide applicability throughout the area. Note that such studies are based upon the assumption that the baseline for introducing changes is *not zero;* the baseline must be a *systematic knowledge of the indigenous farming system.*

The invention of the methodology for discovering and analyzing an indigenous farming system went through three stages of development. The first stage involved intensive field studies in which the social scientists interviewed many farmers, observed their farming practices, and also worked with 25 farm families to develop daily records of labor utilization, activities carried on, inputs used, and money expended. This methodology produced for the first time in Guatemala a systematic description of an indigenous farming system. Though important for research purposes, this methodology was too costly for general utilization throughout large areas of Guatemala. It required the services of eight to ten professionals working in the field and in their offices throughout an agricultural year and then going back to the same area a second year to check on the yields the farmers had achieved and the quantities and prices of the crops they marketed.

On the basis of what they had learned in the first stage, the Socio-Economic unit devised a shortcut field survey methodology. In this stage the social scientists spent about two weeks in the field interviewing a sample of small farmers. When they found that the data produced by the shortcut methodology were close enough to those yielded by the more intensive first stage studies to be used for planning purposes, the researchers had solved the cost-effectiveness problem.

The second stage left a major problem still unresolved: data gathered by anthropologists, economists, and sociologists had low credibility with plant scientists, who dominated ICTA. In the third stage, the Socio-Economic unit overcame this problem by working out an arrangement with the plant scientists to carry out the area surveys jointly.

For each day in the field, the members of this joint team went out in pairs, a social scientist with a plant scientist, and each evening the team got together to discuss results and to raise questions for further checking. Each day also the composition of the pairs was changed so that each social scientist gained experience with each plant scientist, and vice versa. This strategy gave each team member a broad range of interdisciplinary and interpersonal experience. As the new methodology came into widespread use, we found plant scientists increasingly basing their experimental strategies upon information provided by the field farming system surveys.

On the basis of solid information regarding the indigenous farming system practiced in the area of intervention, the Socio-Economic unit now devised another set of social inventions designed to involve small farmers as active participants in the experimental process. The design of the program was based on the assumption that the innovations to be tested out needed to be fitted into the indigenous farming system and must at first involve only minor modifications of that system, which would be within the financial capacity of the small farmers.

The experimental process was carried out in two stages. In the first stage, professionals of ICTA were in control, but they planned the experiments in active consultation with small farmers and rejected any change which the farmers considered impractical. In the second stage, the farmers themselves assumed control, with ICTA professionals standing by as observers and consultants.

ICTA had solved the problem of involving small farmers actively in the experimental process, but this was done at first only on a very small scale at a few locations. For the methodology to be applied widely within ICTA itself, leaders of the organization had to invent solutions to a number of problems involving administrative decentralization and interdisciplinary collaboration. Let me pass over these now to point to a major area of still unresolved organizational problems in Guatemalan agricultural R & D.

We found there a striking contrast between ICTA, an innovative research organization, and DIGESA (General Agricultural Services Administration), an extension organization still following the conventional pattern of organization and practice imported from the United States years earlier. The emphasis was upon supervised credit. The extension agent worked out for the small farmer the production plan to be used as a basis for securing credit from the agricultural bank and then closely monitored his farming activities. With this strategy, the agent could not work with more than 40 to 50 farmers in any given year. Reaching any large percentage of small farmers through such a model would be prohibitively expensive.

This was much more than simply a problem of numbers. The DIGESA model was still based implicitly on the myth of the passive peasant: the notion that he could not change unless guided and controlled by professionals. The ICTA model was based on an assumption of reciprocity and mutuality: professionals and small farmers had much to learn from each other. When we finished our field work, Guatemala was still struggling to overcome the structural and social theory problems blocking the effective integration of research with extension.

In neighboring Honduras we discovered social inventions which appeared to be enabling the Ministry of Natural Resources to overcome the organizational problems we had observed in Guatemala. In the first place, the directors of research and of extension occupied offices across the hall from each other and worked closely in planning joint programs. Extension agents were beginning to participate actively in farming system surveys. Some of them acknowledged that they were embarrassed initially with this new approach because they had been telling the farmers what to do for several years, and now they were asked to make a new beginning by finding out first what the farmers were actually doing and why. Still, this interview and observation approach was providing the foundation for a new and more collaborative relationship between extension and small farmers.

Increasingly, extension agents were getting away from a one-on-one strategy, encouraging the formation of groups of farmers so that extension could work with the group and through its informal leaders. This group-based strategy was facilitated enormously in Honduras by the existence of large and powerful peasant movements. In each village the peasant organization aims to establish a cooperative for buying inputs, selling crops, and planning and financing production. As these base-level cooperatives have gained strength, they have been joined together to form regional cooperatives, including eight to ten local units.

At this point, a creative official of the Food and Agricultural Organization, Rolando Vellani, began working with the cooperative movement. After extensive interviews with farmer leaders, he helped each regional cooperative work out a program of weekly meetings with the regional heads of all of the government agencies related to agriculture. Vellani's group helped the regional leaders plan the crops to be planted and determine the inputs and equipment they would need to carry out the plan. Then, as the officers of the regional cooperative met with regional government officials, they presented a production plan complete with a collective loan application to the agricultural bank.

These organizational innovations involved in the building of local and regional cooperatives and in linking them with government agricultural agencies have made possible the achievement of extraordinary economies of scale, especially in the provision of agricultural credit. For example, in Mexico agricultural scientist Antonio Turrent told me in 1977, "Even ten years after the beginning of the Puebla Project, it is impossible to get credit to the Puebla farmers less than a month after they can most efficiently utilize it for buying fertilizer and other inputs."

When a government credit agency has to deal with hundreds of small farmers or even with small groups of farmers, such bureaucratic delays are commonly observed all over the developing world. In Honduras, the head of the regional office of the agricultural bank meets weekly with leaders of the regional cooperative and is thoroughly familiar with the process of production planning and the estimation of financial needs. With this background of knowledge, he needs little additional time to study the loan application and, with a single stroke of the pen, can release funds for hundreds of farm families. The leaders of the regional cooperative then assume responsibility for distribution of the funds to the base organizations. The costs of administering these loans from regional cooperative to farm families are carried by the regional cooperative itself, as the loan includes 1 or 2% for travel and office expenses for cooperative officials.

When we were visiting Honduras in February of 1980, Vellani described the recent emergence of a third level of the peasant cooperative organization:

a national federation of regional cooperatives. The national federation already included six regional cooperatives, and others were in the process of formation. Again Vellani and his group had facilitated the establishment of linkages with government, this time at the national level. There is now in practice in the capital city a monthly meeting at which the operating heads of all agriculture-related government agencies sit down for a day-long meeting with the leaders of the national federation to discuss and plan agricultural programs and policies. This national meeting provides a linkage at the top level between farmers and government while building on the local and regional cooperatives.

IMPLICATIONS FOR FUTURE RESEARCH AND PRACTICE

The analysis of social inventions in these cases raises as many questions as it answers, but that in itself is an indication of a fruitful research strategy. For example, I find it striking to discover important social inventions developing under such inhospitable environments as those imposed by the repressive dictatorships of Franco in Spain and the military juntas which have governed Guatemala since the CIA engineered the overthrow of a democratically elected government in 1954. These cases suggest that the resourcefulness and creativity needed to produce significant social inventions may be found even under governments that impose severe limitations on human freedom. This is not to suggest, however, that social inventions at the local level or in one part of a government agency will be sufficient to transform a repressive dictatorship into a progressive democracy. This analysis simply points to the need for further study and reflection regarding the relations between small-scale changes, built on participatory strategies, and the issues of reform or revolution at the national level.

IMPLICATIONS FOR RESEARCH METHODS AND SOCIOLOGICAL THEORY

The study of social inventions involves more than a shift away from more traditional topics. It also involves major changes in research methods and theory development. Let us explore these implications, starting by contrast with what I take to be the standard model of social research taught to our graduate students.

According to that standard model, the researcher goes through the following steps.[4] He reviews the literature and consults with his colleagues regarding the problem he would like to study. Then he selects hypotheses that he wants to test, arming himself with a combination of reasonably well-supported hypotheses that he can reinforce, hypotheses that involve conflicting evidence from past research, and perhaps a novel hypothesis or two that he can think up himself. With this theoretical armament in place, he picks out a target population for study—and with this research style, the "target population" is well named. He then moves in to persuade the gatekeepers controlling access to this target population that, if they let him do the study, somehow the information he gathers will be useful to them as well as to him. Having done the study, if he isn't too busy writing his scientific papers and proposals for new research, he may return to the gatekeepers with what he has learned.

Where did this research style come from? I suspect that sociologists have been unconsciously following a physics model. In physics, the phenomena under study are fixed, at least in the sense that, though they are in constant movement, they follow a reasonably standard orbit. The physicist is experimenting on the basis of a highly developed and coherent body of theory. And finally, since the phenomena are under the control of the investigator, he does not require their active participation in the experiment.

This model is much less appropriate in sociology or organizational behavior. The phenomena we study are in movement, and new combinations are constantly emerging. Our theory base is much less firm, and our links from data to theory are often exceedingly shaky. Furthermore, we are dealing with active human beings, who can contribute to our study if we allow them to participate. Under these conditions, before adopting the standard model, we should at least ask ourselves: Do we really know the territory we are investigating? Or are we just mechanically applying a given research instrument?

In the research strategy required for the study of social inventions, you do not start out with a preestablished research design. Of course, you don't start out with a blank mind either. You consult the research literature, but you refuse to be bound by it. In the first place you assume that the published literature is likely to be a decade behind the most interesting things happening in the field. Furthermore, while the literature may illuminate a problem, it may also impose intellectual blinders that guide you along traditional pathways. In many cases it is less important to gather new data than to develop a new way of organizing and interpreting data. For example, few problems in sociology have received more research attention than the *diffusion of innovation,* yet, as I have pointed out, researchers on changes in agriculture in developing countries have generally followed a conceptual scheme based on a misdiagnosis of the problem and have therefore provided findings that are worse than useless.

Before preparing your research design, you go out into the field. Through interviewing and observation, you develop a rough map of the social, economic, and technological territory. You gain a preliminary idea of social processes—of the interactions and activities in which people are engaged—in order to diagnose and solve their problems.

After some period of immersion in the field, fortified by reading about it, you discover a general pattern along the following lines. The people you are studying have their own conventional definitions of the problems they are facing.

Conventional solutions are proposed and sometimes tried out. More often than not, the conventional solutions don't work, and the problems remain—or else the conventional solution solves one problem but creates other equally intractable problems.

You also discover that people have characteristic ways of adjusting to the recognition that their standard model is not working well. They lapse into *normative* thinking. They do not question the model itself but instead assume that it would work well if only they could recruit better people for leadership positions, provide better training for supervisors, and develop better means of monitoring activities, and punishing people who are not doing what they are supposed to do. Over the years, from field studies of piece rate systems in industry (Whyte, 1955) to studies of agricultural research organizations, I have found this a common reaction to the perceived deficiencies in the workings of the standard model.

Assuming that, in order to arrive at scientific generalizations, sociologists must gather systematic data on a number of cases having important characteristics in common, we have too often been trapped into studying the *standard model* in all our various fields of research. Thus we can demonstrate ad nauseam how the standard models are not working the way their proponents claim they should. If we confine our studies to cases where the principal actors are defining their problems in similar ways and attempting similar solutions, with similar results, we can only speculate as to what would happen if the actors defined the problems differently and took different lines of action.

Instead, let us assume that our preliminary diagnosis is accurate and then look for situations where actors are defining the situation differently and devising different solutions—in other words, where they are trying out social inventions.

Having discovered a social invention, you then move in to observe, interview, and gather documentary material so that you will eventually be able to provide a systematic description of that invention. You then seek to evaluate the invention or set of inventions.[5] This is not simply a matter of judging the degree of success or failure. If the invention appears to work, this judgment does not tell us *why* or *how* it works. If we are to be able to describe a social invention in a way that makes it potentially useful in other situations, we must grasp the social principles underlying its effectiveness in the case under study.

As I have argued in the Mondragón case, evaluation necessarily involves abstracting social inventions from the social and cultural context in which we find them. Unless we do this, we are vulnerable to two types of errors: We attribute success to an invention, whereas in fact the favorable outcome depended primarily on the cultural context; or we abandon as a failure a potentially sound invention which was implemented in an incompatible context. The first error is less serious for, in that case, as the social invention is tried out in a new context, the invention-context relations will become more evident and the invention can then be reevaluated. On the other hand, if a potentially sound social invention is undermined by an unfavorable context, that invention may be discredited and lost forever.

An example of the second type of potential error is a social invention of AID sociologist James Greene, designed to serve two purposes: to help rural communities in Peru to provide public works for themselves without undue burdens on the national government and to strengthen local governments through their involvement in building, financing, administering and maintaining such projects. An important and novel feature of FOROCO (the Revolving Community Loan Fund) was a procedure designed to prevent the diversion of public funds to private purposes, a practice which had discredited many of such cash-aided self-help projects in other parts of the world. The branch bank could make disbursements to suppliers of materials only when they presented duplicate copies of disbursement certificates the bank had received from its regional office. Furthermore, the local mayor had to accompany the supplier to the bank, certify to the receipt of the materials, and present the authorizing ordinance passed by the local government, together with a repayment plan based on receipts from a special tax levied by the local government. The duplicate copy of the disbursement certificate came to the local government and then to the suppliers through Cooperación Popular, the government agency which provided technical assistance to the community in drafting the plan and to the local government in drafting the necessary ordinances and tax levies.

FOROCO got under way impressively with villagers volunteering their labor and committing themselves to repay loans for materials and equipment through the local government, which thereby gained significant taxing power for the first time in Peruvian history. But then the program bogged down, with increasing defaults on the loans, and AID subsequently wrote off the Revolving Community Loan Fund as a failure.

What happened? At this time, for one of the very few times in Peruvian history (1963–1968) the President was faced with an opposition majority in Congress. Viewing with alarm the popularity of the new program, the opposition leaders pushed through a *grants* program, promising to give villagers the same public works without any payment obligation. Faced with the choice between a gift and a loan, villagers naturally opted for the gift, and those who were already committed to loans did not see why they should have to pay for what other villagers elsewhere were getting for nothing.

In such a case, the sociologist can perform an important service by showing that a social invention, abandoned as a failure, may actually be successful if planted in a different political context.

This research strategy has important implications for the choice and timing of research methods. The questionnaire or survey has long been the favorite method of sociologists, but you do not go out looking for social inventions by doing a survey. When it comes to evaluating success or failure, however, it is important to know the opinions and attitudes of the people who are affected by a social invention, and here the survey can be an indispensible instrument.

This research strategy also has important implications for the relations between the researcher and the people he/she studies. It is folly to treat those who have created an important social invention as passive subjects of research. We need to learn from them the personal experiences and thought processes that led them to create the social invention as well as their theories of why the invention works or why it fails to work as well as they think it should. That is not to say that we function simply as reporters, passing on the wisdom of the social inventors. We must look for the underlying principles of social dynamics, whose discovery will enable us to describe and analyze a social invention in such a way that other human beings in other situations may be able to adapt and utilize it.

In seeking to apply the results of our research, we should think in terms of social inventions rather than in terms of attitudes, beliefs, and values. For example, when I was beginning a study of a remarkable shift from conflict to cooperation in the Inland Steel Container Company (Whyte, 1951), one day I confessed over the telephone to a vice-president of the parent corporation that I was having difficulty in figuring out the change. He commented, "Yes, that did puzzle me for a long time, but I have finally figured it out."

In eager anticipation, I asked for the answer. His reply: "They learned to trust each other."

For a moment I sat there speechless, waiting for something else to come out of the receiver, but that was all there was. I drew a deep breath and said, "Well, I guess you're right, but *how* did they learn to trust each other?"

The vice-president laughed and said, "You're the sociologist. It is up to you to figure that out." That is the challenge to sociologists.

I am not denying the importance of subjective mental phenomena, but, we are rarely able to change behavior simply by telling people that they should change their attitudes. Attitudes, beliefs, and values do indeed change as people, in grappling with persistent social problems, devise creative ways of restructuring their activities and interactions and their relations to the physical and social environment. In studying the social inventions that enable people to bring about such changes, we can build a more useful applied sociology. And, as we study the implementation of social inventions in new socioeconomic and technological contexts we will also contribute to the building of sociological theory.

Sociologists and other social scientists have begun to realize the significance of social inventions developed in Mondragón and in innovative agricultural research and development programs. Practitioners and professors are beginning to use such inventions in project planning and in rethinking social theory. I do not claim to be the discoverer of any of these social inventions. My aim has been to conceptualize them in such a way as to stimulate new strategies for research and action.

May I suggest that the research strategy presented here is itself a social invention?[6] Lest this seem too grandiose a claim, let me qualify it in two ways.

In the first place, I do not claim to be the originator of the concept of social invention. It appeared in the sociological literature of the 1920s and 1930s, and, in all probability, its origin could be traced back further decades or centuries.[7] Until now, however, the concept has been peripheral to sociology, having no recognized place in the mainstream of sociological theory and research methods. I am suggesting that we bring the concept into the mainstream as a key organizing principle both for theory and practice.

In the second place, to state that something is an invention tells us nothing at all about its potential value. Very few of the mechanical inventions receiving patents ever get developed to the point of widespread utilization. The value of this social invention will not be determined by what I say about it. You, my colleagues, will make that judgment. If you don't use the invention, this address will rest quietly in the files of the *American Sociological Review*. If you do use it—and improve upon it—then this Presidential Address will prove to have more than ceremonial value.

NOTES

1. In fact, several colleagues wrote to congratulate me for selecting "Social *Intervention*" as the theme of the 1981 ASA meeting.
2. The Mondragón study was part of a research program supported by the Center for Work and Mental Health (NIMH–R01 29259).
3. Much of the research reported here was supported by a Cooperative Agreement between AID and the Rural Development Committee of Cornell University.
4. In describing this model, I use the masculine pronoun because I assume that men have been primarily responsible for creating the model.
5. In this paper, I have not dealt with the problem of evaluation of Mondragón or of the Guatemalan or Honduran programs. Evaluation research has become a well-established field of sociology on which I have nothing to add beyond noting that it is a good idea first to make sure that you have focused on something worth evaluating.
6. I am indebted to Renee Fox for this point.
7. I am indebted to Warren Dunham for reminding me that William F. Ogburn used the concept in the 1920s. I am also indebted to Sheldon Stryker for pointing out that Stuart Chapin also used the concept (see References).

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