development. While we have focused here upon gambling in its narrowest sense, it is the diffused attitude toward wealth and its attainment—in the case of the Philippines, carried here but not fully borrowed—that is of paramount interest. Investigation of *hua ch-iao* gambling inevitably leads to consideration of the role of the Chinese in the economy of the nation. The study of gambling may fall within the narrow purview of folklore, but a projection of its findings are relevant to contemporary economic anthropology.

The Case for Unorganized Social Research

RUBEN SANTOS-CUYUGAN
University of the Philippines

One of the most interesting developments in recent years in the field of social science is the increasing popularity of organized social research. By this is meant research which is undertaken either by individual social scientists or by teams at the request and with the support of an organization established for the purpose of undertaking or supporting such research.

The Columbia Survey[1] undertaken by Paul Lazarsfeld shows the wide variety of academic institutions granting doctoral degrees that have proliferated out of this enthusiasm in the United States. Many of these institutions are fairly autonomous units within the college or university to which they are attached. The majority are "special purpose" units, which confine their attention to specific topics such as delinquency, mass communication, or community development. Others do not restrict themselves to any one topic. In all of them the picture that emerges is one of tremendous activity. In the United States, support of such activity through Federal funds more than doubled between 1953 and 1958 going from twenty to forty-eight million dollars.[2] The situation in other countries, although less spectacular parallels the trend in the United States. Empirical social research, says Lazarsfeld, is "one of the outstanding features of the twentieth century."[3]

The opportunities for social science research in any one country are obvious. There are many imponderables in the situation, however, which need pointing out, even as we happily start to stake out our claims to the rich lode of government funds, foreign aid, foundation monies, and other largesse which make the disinterested researcher so scarce a commodity. One of the purposes of this brief paper is to make a plea to the social scientist to preserve and cultivate his capacity for candid, critical, appraisal of self, and of the forces that direct his biases. Such an act often spells the difference between work which is suspect, and useful science. This is to raise the issue once more as to the amount

[1] Paul Lazarsfeld, "Observations of Empirical Social Research in the United States, "Information, (Dec., 1961) International Social Science Council, with the aid of UNESCO, pp. 3-46.

[2] International Survey of Programmes of Social Development, N. Y.: United Nations, 1959, p. 128.

[3] Lazarsfeld, op. cit.

of freedom the scientist should state as the condition for the expenditure of his talent and his energies. Organized social research—research done for the group, whether for government, industry, etc.—confronts the social scientist with pressures in the selection of topics for investigation, special emphases, the use of models for analysis, perception of aspects of observational datum, which consciously or unconsciously color his work. One might expect this to be more true in the United States where social research is more organized. What mitigates this danger in the U. S., however, is the sheer diversity of programs and the institutional organization in terms of financing, administration, coordination, and facilities. It is also true that there are programs of research and teaching, many associated with smaller colleges or universities, that are less organized and hence, in our sense, less subject to the danger of bias and regimentation.

In the Philippines, where the social sciences are only now becoming empirical and where research can only be done in relatively affluent and better-staffed units such as the larger universities and government agencies, the countervailing factor of independent research done for its own sake is evident. Because of rather urgent pressures to move immediately into organized, sponsored research, increasing support for such research is therefore both a blessing and a risk.

Lazarsfeld has pointed out one unfortunate consequence of organized social research. Citing an observation of James Coleman, he says: "a large number of useful social inventions have never been made because there are not institutions which are interested in encouraging them."[4] We may add, using a term introduced by Merton, "serendipity" has often turned out useful social inventions or ideas from research which were not sponsored nor particularly encouraged by any institution. The problem becomes even graver in a country such as the Philippines, where in recent years what has received greatest encouragement and support, sometimes exclusively, by foreign or local councils, have been researches in the *application* of the social sciences, to such fields as public administration, public health, aggricultural extension and education, etc. Despite the tendency to regard the distinction between applied and basic research as lacking merit, still there are many necessary investigations, for example research into the value-system of Philippine culture, or into the social stratification of the entire society, which may not seriously be considered by foundation hierarchies or government councils for support because they do not have a surface bearing on concrete problems. The problem can be stated in another way. As the applied fields in social science receive more support, they begin to unearth novel problems, for which they turn to "pure science" for the necessary insight before they can go further. This is particularly a problem in the Philippines, where what is being applied represents work *not* done on the same culture or society. Having been left out in the scramble for outside funds, the basic social science researcher oftentimes finds himself unable to help the practitioners.

Because basic research is wanting, a number of consequences follows. Among these, I wish to emphasize the following:

1. The tendency is to follow beaten paths in research, to "apply existing routines."

2. Similarly, it is likely that existing models derived from American social

[4] Lazarsfeld, op. cit., p. 28.

science will be used, rather than begin work on conceptual schemes as such for later use.

3. Because of the bias implied in (2), alternative analyses are likely to be missed. As an example, we note the tendency to consider behavior which is contrary to the accepted moral standards of Western society to be pathological deviations or aberrations possibly stemming from a wilfulness which is at best a "primitive impulse." Thus the opportunity is lost to relate them to what is possibly basic personality structure, role structure, or to basic factors regulating the system of interpersonal relations in Philippine society.

These are some of the consequences, not all.

It seems, then, that if we are to go further in the understanding of the problems that face us in applied social research such as those in community development, public administration, etc., we will also have to face up to the need for research which may not be popular, research which may even lie off the beaten paths. Most certainly this will be research where group or institutional biases will not serve us well. On the contrary, it will require the same open-mindedness which the founders of American social science themselves enjoyed.

Only then can we begin to attempt solutions to problems to which earlier research has led us. These problems might include the following: Can we hope to achieve personality and community (culture) change by a process of backing into, rather than pushing forward, from underneath, as McClelland[5] pessimistically regards present efforts at community development? at what point does personal interest (ingroupness) get transformed into a concern for the community? what kind of role should a change-agent play in the community? what personality type should he be?

To my mind, these questions may not adequately and fully be answered by current organized research programs found mostly in academic institutions in or around the metropolitan Manila area. They need to be investigated by individual scholars or by "less-organized" groups working independently in academic institutions throughout the Philippines. Such endeavor can produce many new leads, assuming, of course, that the research person has adequate training. In this situation scholars can avoid the many pressures and biases that beset their more affluent colleagues in the larger centers of research. Research, as always, is useful only to the degree that it is scientific, dispassionate, and untrammeled.

[5] David C. McClelland, "Community Development and the Nature of Human Motivation: Some Implications of Recent Research," (Background Paper; Conference on Community Development Center for International Studies), 1957.