

Researching the Culture in Agriculture: Social Research for International

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If I were teaching a graduate introduction to anthropological theory, development, ethnography, political anthropology, or methodology, this would be a prominent text. It is one of the very few books that apply anthropological methods to anthropology itself, and it does so in a context of the first importance. The ultimate subject matter is agriculture, especially in economically poor countries and especially the problem of improving its productivity while maintaining sustainability. The ultimate methodological problem is the nature of science. The more immediate subject matter, where both of these ultimate subject matters come together, is the role of anthropology as science in the organizations and programs of the Consultative Group on International Agricultural Research (CGIAR). The place of anthropology in the CGIAR is representative of the place of anthropology in international development activity as a whole, and it is not altogether a cheerful story.

There are 22 essays grouped into three “Parts.” They originated in a conference of the CGIAR social scientists in 2002, concerned with declining support for their activities. The chapters in the first two parts are by CGIAR scientists. The third are by related “outsiders.”

CGIAR has developed as the apex coordinating body for the green revolution. The developments leading to it began in Mexico in 1943, when the Rockefeller Foundation and the government of Mexico established a cooperative research program on the land grant model to improve the yields of wheat and maize. The first High Yielding Varieties (HYV) wheats were released in 1961. By 1965, they were the most important wheats in Mexico. Although the Rockefeller Foundation officially closed the original program in 1962, it kept on certain of the senior scientists at the request of the Mexican government. Meanwhile, in 1960, the Rockefeller and Ford foundations jointly

established the International Rice Research Institute (IRRI) in the Philippines, and in 1963 the Mexican program was brought under a new agreement as the International Maize and Wheat Improvement Center (El Centro Internacional de Mejoramiento de Maíz y Trigo, or CIMMYT) along the same lines as IRRI. CIMMYT now has 17 branches in other countries around the world. In 1967 the International Center for Tropical Agriculture (CIAT) was founded on the same pattern in Cali, Colombia, and the International Institute for Tropical Agriculture (IITA) in Nigeria (Chandler 1982, 155-156).

In 1971, after the regional centers had been established, the Ford and Rockefeller Foundations, World Bank, Food and Agriculture Organization of the United Nations, the United Nations Development Organization, and several governmental bodies agreed to establish the CGIAR to coordinate funding for them. This now brings together many additional private foundations as well as agriculture departments of sixty governments. It supports fourteen major research agencies dedicated to increasing food and other crop production around the world in addition to the original four. CGIAR's mission was to develop and spread new versions of the original green revolution technologies. The scientific and managerial leadership of CGIAR and its consistent organizations consistently rested with what the contributors describe as "biophysical scientists," mainly plant biologists and breeders. The HYV varieties were designed to give increased yields by being responsive to increased inputs: water, fertilizer, and chemical insecticides. In this context, the role of the social scientists was mainly focused on designing programs to assure adoption. Few questioned whether they actually *should* be adopted, or asked when and where they should, so there was little interest in social science as a source of feedback or guidance.

In 2000, however, the mission changed. A major policy statement titled *A Food Secure World for All: Towards a New vision and strategy for the CGIAR* made poverty reduction and sustainability integral major goals and mandated the increased use of social scientific knowledge to bring this about. The problem for the volume is that this has not happened. The number of social scientists has actually been reduced, to point where in several areas the contributors argue they are below "critical mass."

Part I provides an overview. Chapter one, by Michael Cernea, details what he describes as “the uphill battle for social research in CGIAR.” Cernea served in CGIAR from 1998 to 2003 as a member of several high level science bodies, including the Technical Advisory Council. Before that, however, he had been able to observe the development of CGIAR from the vantage point of his long-term position in the World Bank, beginning in the mid-1970s. He was the first anthropologist on the Bank’s permanent staff and his role was to advise them on how to integrate others like him, which he did. Thus, the Bank started with no anthropological knowledge at all, recognized a need, and has been steadily building up. In the CGIAR, by contrast, beginning in 1974 the Rockefeller Foundation provided a cohort of high quality social science researchers with the specific purpose of “jump-starting” the social science aspect of their activities, the “Rocky docs” (p. 12). They clearly demonstrated their value and many stayed on after the Rockefeller program ended, but now they are leaving and the CGIAR leadership is not maintaining their positions. Cernea details the employment trends in CGIAR overall and in the constituent units.

A central question in the volume is what “social science” is, that economics is not. Cernea attempts to address this question in the beginning of his essay, but is too diplomatic. This is where he introduces the title theme the “concept” of “Culture in Agri-Culture.” His argument is that the “fundamental building blocks” of agriculture are cultural, and need to be understood as such:

Anybody who does not recognize this, and does not understand their actual weight in real-life processes, misperceives reality. Any centre manager or scientist in an international research centre who, either in public documents or in his or her own mind, downgrades or leaves out from research agendas the study of these fundamental social-cultural components, ends up with an incomplete grasp on reality and undermines his individual, and his institutions performance. (p. 7).

Although this is certainly right, it is not likely to be definitive for readers who do not already understand what the “building blocks” are. In effect, this is what the essays are trying to describe.

The second chapter, by Kassam, provides more information on the internal history of the CGIAR. Kassam's analysis suggests that the history needs to be divided into two main periods: before Cernea and since. Before Cernea, including the period of the Rocky docs, social scientists did not have autonomous, high-level representation in the organization and therefore also did not have control of the kinds of problems they would work on. They worked on what seemed to be important to the biophysical scientists, which was mainly farmer participation (mainly meaning acceptance) and gender. Since Cernea these are still the dominant topics, but they have been placed in a framework reflecting the new concern with poverty alleviation and sustainability.

Chapter 3, by Eva Rathgeber, reports the results of an internal survey of the CGIAR social scientists. Essentially, there is little sense of commitment to them. Most are on three year contracts or less, with the average contract period for women being less than for men. While 17% of the CGIAR social scientists aspired in the long run to work at the level of management, only 3% expected to do it within CGIAR (p. 72).

In short, the picture that emerges from the initial essays is that the CGIAR social scientists have been saddled from the outset with a short-term and socially naïve research agenda. This does not make sense under the new mandate, but precisely because they have done what was required in the past, there is little basis on which to demonstrate what the alternative could be for the future.

Chapters 4 through 15, Part II, detail these relationships across the CGIAR agencies. Each is written by one or a few senior social scientists within the agency they describe. Part III is thematic, addressing issues that run through all or most of the separate agencies.

Since space does not permit a coherent synopsis chapter by chapter, I will concentrate on some of the more important recurrent themes. These fall under five major heads: the ethnography of the CGIAR itself; what is meant by "social science;" "methods" and "frameworks"; theory; and the relation to academics.

Considered as ethnography of knowledge, the methods in the different chapters and situations differ greatly, but the overall portrait is consistent. Some accounts are by single authors, some are joint efforts, some have an overall coordinator but separate parts by different individuals, and a few summarize surveys. The situations ranged from those

in which social science has been crippled by design (IRRI and CYMMIT), through at least two notable instance in which individuals overcame the initial mission statements and incorporated social science into a more effective conception of the organization the (International Potato Center (CIP) and the system-wide Programme on Collective Action and Property Rights (CAPRI) described by Ruth Meinzen-Dick in Chapter 14), through a couple of cases in which social science had been recognized as fundamental from the outset (CIAT and International Water Management Institute (IWRI)). These variations, however, do not mean that the general conclusion is only partly right. They rather mean that the causal dynamics are not fully spelled out.

There is a continuing debate in development circles over whether development assistance should be primarily “technical” or primarily “institutional.” We see the same debate here. Some of the missions of the constituent organizations much more readily lend themselves to the technical perspective, others to the institutional. Yet the relationship is not automatic. A major theme in almost all the chapters is the evolution of different ways in which the shallowness of the more technical and paternalistic frameworks were exposed and more interactive approaches have been developed which were, in the nature of the case, also more institutional.

The conflict between economists and “social scientists” could be much better developed. Several writers mention that economists usually use surveys or “structured questionnaires.” Chapter 8, describing CIP, notes that it began under a CGIAR mandate that in effect said economists would be central and “social researchers” (who are almost all anthropologists) would be marginal. This reflected two “theoretical notions” that were widely accepted at the time: “modernization theory” and the view that farmers were essentially passive and traditional, while the CGIAR centers were the main sources of innovation. Anthropologists and other “social scientists,” by contrast, consistently argued for paying more attention to, and according more respect for, local knowledge. The initial CIP program did not work. It was then redesigned in a collaboration by the director, a biologist, and Robert Rhoades, an anthropologist who had formerly served in the Peace Corps. Modernization theory was replaced by the recognition that agriculture was inherently innovative and dynamic; the “CGIAR to farmer” orientation was replaced

by “farmer to farmer.” The CIP is considered one of the clearest social science successes.

The most extensive description of the difference is in Part III, Chapter 17, Social Research and Researchers in CGIAR: an Underused Potential, by Robert Chambers. This focuses on two important gaps: the gap between what social science in the CGIAR presently is and what it could be, and the gap between what CGIAR’s newly expanded mission and what it is actually doing. Chambers contrasts “social scientists”, which in this case includes economists, with “social researchers” which includes non-economists. He associates the former with “pipeline” research and the latter with “learning-process” research, which is the same thing that other contributors call “participatory” research:

The contrast is far from absolute, there are many exceptions, and much depends on individual personality and predispositions. To a degree, however, the approaches, methods and training of social research fit and are more comfortable with open-ended learning processes, and the approaches, methods and training of economics and the physical and biological sciences are more comfortable with the discipline of pipelines. This means that social researchers are generally better placed to catalyse and support the reorientation of CGIAR research to deal with the complexity, diversity and dynamism of the realities toward which it is pointed by its new mandate. (p. 363)

The description of “methods” is also sketchy. Many are named, but none are described in detail or reviewed critically. Some can be regarded as common and others relatively original to CGIAR and development work.

Methods that appear to be common include ethnographic methods and diagnostic surveys of farmers’ knowledge as an alternative to the assumptions of modernization theory; triad testing/free grouping/consensus analysis; case study; mapping; cross sectional household surveys; contingent valuation and conjoint analysis; and policy research. They are commonly invoked only by name, as though they are highly standardized and of undoubted analytic value, which has the unfortunate effect of precluding self-critical methodological discussion.

The methods that appear to originate in the CGIAR and related development activity are rapid appraisal of farmer knowledge systems; farm systems research; participatory approaches for problem diagnosis and constraint analysis (Ch. 9); and finally recognition that participatory development was a conceptual alternative to farming systems research (Ch. 11). Here, too, lack of self-critical reflection seems to get in the way of a really serious delimitation of what can and cannot be known. Rapid appraisal is not a social science method at all; it is a way of introducing a bare minimum of social science observation into the perspectives of engineers and the like. Farming systems research was much talked about in the 1970s and 80s. Essentially, it was the pretence that a concept like “American wheat farming” or “Chinese wet rice farming” could be reduced to definite relations among definite crops, inputs, and outputs of some kind. Many abstract diagrams were produced, but proponents could not agree on how to relate them to any definite data. The reason is that farm systems, in the sense of *systems of farming*, are actually not sets of objective relationships among objective inputs but human strategies that take shape around the farmer’s purposes. They cannot be described without focusing on those purposes and the ways they are carried out, which is precisely why the shift to participatory development provides an alternative to the farming systems perspective. It recognizes the strategies by incorporating the strategists, and once you have this foundation, the details of the process readily fall into place case by case.

Compared to the amount of space devoted to methods, almost no space is devoted to “theory.” The most frequently mentioned theorist is Robert Merton, to whom the volume is dedicated as an exercise in the sociology of knowledge. Harold Conklin is mentioned once in connection with taking indigenous ideas seriously, and Elinor Ostrom mentions Garret Hardin’s “tragedy of the commons” as a theory that requires “some serious rethinking (p. 330)” based on recognizing the different kinds of rules that can be used to transform a Hardin-like “commons” into a well-managed common property resource.

This failure to identify relevant theory is important. Merton, Conklin, and Hardin are determinists. Merton’s conception of theory was of a piece with Parsons and the other positivistic grand theorists of the 1960s and 70s, only he was sensitive enough to observation to state the case more tentatively. Conklin’s assumptions were the same,

although his topic was radically different and far more definite. Theory is supposed to say what “causes” individual behavior, and the assumption in this is that this cause cannot be the choices and purposes of the individuals themselves—it must be something outside, above, and most likely unknown to them. Theory of this sort has, throughout history, been used to justify authoritarianism, both in scholarship and in politics. Of course this is not to say that all those who have argued for such theory have been conscious proponents of authoritarian government, or even recognized the association. But ideas have implications whether they are recognized or not, and one of the implications of theory that presumes that behavior is ordered only by imposition from above is that it is logically unable to explain why such imposition so consistently does not work, or to frame any alternative to it other than disorder.

The alternative to determinism is represented by modern skepticism and pragmatism—Kant, Holmes, Dewey, Mead, and so on. This has been obscured in sociology and anthropology but is dominant in the broader stream of social thought that includes legal philosophy and the working theories of democratic government. Its central methodological concept was articulated clearly by Rudolph von Jhering, a giant of modern legal theory. In *Law as a Means to an End*, Jhering argued that in all living beings there is “no volition, or, which is the same thing, *no action, without purpose*” (1913:2, emphasis his). The experimental test he offered for the presence of purpose is whether we can substitute the idea of “in order to” for the word “because” in statements about their actions. If we can, then we are assuming that purpose is the cause. It is of course stupid to tell a judge or a lawyer that purpose is “subjective” and cannot be described; they do it all the time.

The difference between volitional and deterministic theory was well understood by the founders of the first Anthropology programs in Berlin, Cambridge, and Columbia, and dominant opinion was clearly on the side of recognizing purpose and rejecting determinism. The conflict was obscured only with the general ascendancy of positivism after World War II, when an important theme in the rhetoric of the Harvard Department of Social Relations and the Unity of Science Movement at the University of Chicago was the claim that positivism actually agreed with pragmatism and, thereby, superceded it. It cannot be so, for reasons that the social scientists in CGIAR have rediscovered.

Finally, the sense of isolation of the issues and debates in CGIAR is not merely a difference between “practical” anthropology and “academic” anthropology. As Robert Rhoades argues in Chapter 20, the same separation exists between anthropologists and biophysical sciences on university campuses. After leaving the CIP, Rhoades went to the University of Georgia, a land grant university. His duties included working with the agricultural faculty in designing research projects, and he found himself in substantially the same kind of position as in the CGIAR:

The problems of achieving a robust interdisciplinarity are largely social, not narrowly scientific. How problems are defined, who controls the process, which rewards are given to whom are not scientific or even negotiated 'processes'. The problems grow from ingrained biases in favour of those who address short-term objectives and technical and economic impacts, which are measurable, giving rise to expressed displeasure and impatience with social science research that foregrounds culture and society and gives human meaning to agriculture. Social science is channeled largely into a service field working on problems identified by biological and technical agriculture, not by the ultimate clients of that effort. (p. 418)

Thus the institutional problem is at once far more general than simply something about the historical culture of CGIAR, and also more specific.

There is no reason to expect anthropological discussions in agencies like the CGIAR to reflect such academic controversies as the substantivist-formalist debate, the alliance-descent debate, the debates over ethnohistory and componential analysis, or the flap over postmodernism. These are short-term squabbles framed within deterministic theory and as such have little to offer. But on a more comprehensive scale, dealing with the basic choice between one type of theory or the other, the analyses in the CGIAR have much to gain from academic theory and academic theory has even more to gain from articulation with the development problems. In this respect, the disarticulation has left both realms of discourse far behind where they would have been if they had been interconnected.