

KNOWLEDGE-BUILDING FOR RURAL DEVELOPMENT:  
SOCIAL SCIENCE AND THE COOPERATIVE AGREEMENTS

A Review and Analysis of Nine Cooperative Agreements  
Funded by the Office of Multisectoral Development, USAID

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PREFACE

COOPERATIVE AGREEMENTS

In March of 1982, the Harvard Institute for International Development began a project funded by the Office of Multisectoral Development at AID to: (1) help identify a future-oriented agenda for social science research on rural development; (2) review the research output produced by nine cooperative agreements with universities and consulting firms in terms of their contributions to social science knowledge about rural development; and (3) make recommendations about the cooperative agreement as a mechanism for uniting basic and applied research with technical assistance to AID field missions. This report responds to the second and third of these tasks.<sup>1</sup>

The predecessor to the Office of Multisectoral Development, the Office of Rural Development, was created in 1974.<sup>2</sup> From the beginning, its principal activities were to assist missions to obtain consulting services and to promote research efforts aimed at generating new knowledge. In 1976, the office asked AID missions to give them advice on the kinds of rural development-oriented consulting and

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<sup>1</sup>The findings of the first activity is presented in Cohen, Grindle, Thomas, and Walker (1983).

<sup>2</sup>For a review of the Office of Rural Development and its activities, including the cooperative agreements described here, see Montgomery, Carroll, and Robinson (1981).

research programs they would like to have provided. The Office of Rural Development, with the concurrence of the interagency Working Group on the Rural Poor, concluded that research and advice should focus attention on off-farm employment, farm strategies, credit, management information systems, and participation. The rural development office then began to identify universities and consulting firms with strong competence in agricultural economics and rural development. Discussions were held with them to explore ways in which they might cooperate to provide research and consulting services on these and other broadly defined topics. Finally, project papers were drafted for particular projects and contractors were selected to carry them out.

The cooperative agreement mechanism was adopted by the Office of Rural Development as an appropriate model for these projects. Pioneered by the Agricultural Office in its work on sectoral planning topics, the cooperative agreement was thought to be more flexible than a contract and allowed the contractor a voice in determining what research was needed. It was also more field-oriented than 211d grants.<sup>3</sup> The basic cooperative agreement model that the Office of

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<sup>3</sup>The cooperative agreement mechanism was not without its critics. Montgomery, Carroll, and Robinson (1981:13-14) state the following:

"...the General Counsel objected that too general a scope of work could not be enforced by law...Some of the bureaus objected to the size of the agreements, especially the second generation ones, which were

Rural Development formulated sought to: (1) develop state-of-the-art papers synthesizing existing knowledge, practice, and methodology in the assigned area and to develop hypotheses that might be tested in the field; (2) conduct long-term applied research based on sustained field work; (3) perform short-term consulting services for field missions as a kind of "reality testing" as well as a contribution to AID's operations; (4) develop networks of professionals in each subject matter area, in the U.S. and abroad, so that there would be communication among specialists as a by-product of the cooperative

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expected, with mission add-ons for field services, to reach as much as seven million dollars. RAD's response was that the critical mass attained by these cooperating institutions would add to their versatility because more specialists would be involved than in a smaller contract. Furthermore, the cooperative agreements would encourage new, interdisciplinary ventures because of their sheer size: single departments would not be able to perform all of the specified functions. The expectation was that these agreements would eventually produce highly visible centers of excellence in fields that were important to RAD's operations.

Others objected that the cooperative agreements were a costly way of producing technical assistance, that the Indefinite Quantity Contract (IQC) would achieve the same result faster and more cheaply, and that an excessive amount was going to research and university overhead. RAD's counter-argument was that there was insufficient capacity in the areas where cooperative agreements were involved to rely on existing organizations through an IQC, that research was necessary to develop this capacity, and that university overhead was less than that of other potential contractors. Finally, another group of critics objected that the agreements were more beneficial to the universities than to the Agency. The universities, with rare unanimity, dissented from that judgment, arguing that pressures on them to provide services to the missions created a powerful corrective to their faculties' proclivity toward theory and library research."

agreement; and (5) disseminate project-generated information directly to academics and field practitioners.

Five cooperative agreements were instituted in the first years: one each with Cornell University, Ohio State University, and Practical Concepts, Inc., and two with Michigan State University. As progress was made with the initial cooperative agreements, more were entered into. Later cooperative agreements were subject to competitive bidding, which increased the need to design the project paper carefully and be more specific about what the objectives of the activity were to be. Currently, the Office of Multisectoral Development oversees a large portfolio of activities based on the cooperative agreement model.

This paper reviews nine of these cooperative agreements.

These are:

1. Rural Development Participation  
Cornell University
2. Organization and Administration of Integrated Rural Development  
Development Alternatives, Inc. and Research Triangle Institute
3. Alternative Rural Development Strategies  
Michigan State University
4. Rural Non-Farm Employment  
Michigan State University
5. Performance Management  
National Association of Schools of Public Affairs and Administration and Development Project Management Center, USDA
6. Rural Financial Markets  
Ohio State University

7. Managing Decentralization  
University of California, Berkeley
8. Land Tenure  
University of Wisconsin, Madison
9. Regional Planning and Area Development  
University of Wisconsin, Madison

General findings about these cooperative agreements and recommendations for improving the knowledge-building potential of future ones are set forth in Section I. The recommendations are presented on pages 36 through 55 of Section I. Reviews of the research output of individual cooperative agreements are presented separately in Section II. Finally, Section III contains comparative analyses of important issues that have implications for all the agreements reviewed.

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## SECTION I

### SUMMARY REVIEW AND RECOMMENDATIONS

#### A. Overview

The central questions addressed in this report are: (1) has the cooperative agreement mechanism contributed to the building of knowledge about rural development? and, if so, (2) how can future cooperative agreements be designed to maximize knowledge-building contributions while providing applied consulting service for AID regional bureaus and missions?

Answers to these questions are based on an extensive review of the major publications of nine cooperative agreements. These publications therefore serve as our data base, rather than information collected through systematic interviews with project directors, authors, or AID project officers and similar systematic analysis of project files. We also do not consider specific mission-related services provided through the cooperative agreements. This approach is justified on the ground that, in the end, a review of research findings must be intersubjective and hence confined to the published record. Although we have held numerous discussions with cooperators and AID personnel, it should be noted that the findings and recommendations of the report do not necessarily address the fact that the research intentions of project participants were at times frustrated by administrative and logistical constraints.

We assess the cooperative agreement research output in terms of how well it meets canons of social science research, how much it has influenced the wider academic community, and how well it addresses the needs of field practitioners. Our major conclusions are that the cooperative agreements reviewed have made significant contributions to professional knowledge about rural development and that they can be implemented in ways that allow researchers to generate findings that are both academically respectable and useful to field professionals. We also conclude, however, that their potential for knowledge-building has not been fully achieved. Finally, we offer specific recommendations and guidelines for increasing the knowledge-building potential of future cooperative agreements.

The contributions of the cooperative agreements to knowledge-building are evident when they are viewed historically. Professional knowledge about how to reach the poor rural majority was limited when the congressional mandate on rural poverty and development was issued in 1972. To be sure, substantial work had been done on agricultural and rural development prior to that time, but it was still insufficient to provide the kind of focused guidelines needed by governments and donors to formulate strategies and operationalize them in policies, programs, and projects. A major objective of the cooperative agreement program of the Office of Multisectoral Development was to fill that gap.

Thus, the cooperative agreement was quite different from the indefinite quantity contract (IQC) or the special purpose contract



(SPC). Specifically, the IQC focused on troubleshooting, task forces, and rapid problem-solving while the SPC focused on field-testing, problem definition, problem-solving, and task forces. In contrast, the cooperative agreement centered on concept development and knowledge-building while performing all the functions normally carried out through SPCs.<sup>4</sup>

The investment by USAID in the nine cooperative agreements reviewed has resulted in: (1) the expansion of general knowledge about rural development policies, programs, and projects; (2) the generation of focused and analytically useful case studies on rural development interventions; (3) the consolidation of existing literature on particular rural development topics; (4) the sharpening of analytic concepts and middle range conceptual frameworks used in analysis of rural development processes and interventions; (5) the elaboration of sets of working hypotheses that can be used to guide further research; and (6) the establishment of a small but important set of empirical generalizations that can be drawn upon to formulate rural development policies or to guide the design and implementation of rural development projects.

In addition, investment in these cooperative agreements has contributed to: (1) the involvement of well-established academic experts in regional bureau and mission activities; (2) the building of

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<sup>4</sup>Based on a typology in Montgomery, Carroll, and Robinson (1981:56-59).

increased professional capacity in universities and consulting firms; (3) the training of graduate students with special skills in the substantive areas of the cooperative agreements; (4) the formation of professional networks among academics and field professionals for the exchange of ideas and information; and (5) the publishing of a large number of well-regarded books, monographs, and journal articles.

On the other hand, the review also suggests that knowledge-building efforts closely coupled with applied consulting activities: (1) are not likely to generate major new theoretical models or challenge existing ones; (2) tend not to be guided by explicit conceptual frameworks well-grounded on established macro-theories; (3) require more time than cooperative agreements typically allow to make major breakthroughs in knowledge; (4) tend to follow less rigorous methodological canons than standard academic research exercises; (5) are not well linked to each other or conscious of each other's findings; (6) require the integration of their research products with those of other rural development-oriented cooperative agreements; and (7) face difficult format problems in publishing findings for different audiences.

Substantial gaps still exist in the knowledge base about rural development. Moreover, rapid rural change is creating new conditions that have yet to be addressed.<sup>5</sup> Further, more detailed knowledge is

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<sup>5</sup> A related task of this project is to assist AID in identifying such gaps and areas. In this regard see: Cohen, Grindle, Thomas, and Walker (1983); and Grindle and Cohen (1983).

needed in a number of areas where the cooperative agreements have operated. Most important, there is need for promoting linkages between the work products of the cooperative agreements and the rapid expansion of knowledge about rural development being generated by those academics and professionals around the world who have not been involved in the AID-funded research.

Drawing on the individual research reviews in Section II of this report, the remainder of Section I will elaborate our findings in detail and conclude with specific recommendations for: (1) making cooperative agreements more rigorous; (2) making cooperative agreements contribute to building expertise; (3) making cooperative agreements more program relevant; and (4) making cooperative agreements more cooperative. This section will also briefly summarize the recommendations of Section III on how to: (1) improve research methodology; (2) increase research utility; and (3) promote research contributions to knowledge about households and women.

## **B. Knowledge-Building and Cooperative Agreements**

### **1. Cumulative vs. Dialectical Knowledge-Building**

Important contributions to the social sciences are generally made through a process of cumulative knowledge-building.<sup>6</sup> In such a

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<sup>6</sup> Knowledge-building is defined as contributions to the understanding of empirical reality, particularly as they explore relationships among observed phenomena.

process, scholarship adds incrementally to a body of knowledge by clarifying theories and concepts, further specifying hypotheses and the conditions under which they apply, field testing these hypotheses, and generating empirical generalizations about widely observed phenomena. As with all incremental processes, cumulative knowledge-building can lead to important alterations in accepted explanations of empirical reality, but the process by which this occurs is gradual and usually the result of a large number of scholars engaged in research on similar topics.

Less frequently, important contributions to the social sciences are made through a dialectical process in which widely held explanations are challenged by new interpretations.<sup>7</sup> This dialectical process generally requires researchers to be particularly concerned with issues of theory and to be very explicit about the identity and nature of major cause and effect relationships. As a result, broad and theoretically self-conscious alternatives to prevailing explanations of observed phenomena are put forth, rather than additions to or

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<sup>7</sup>These processes of knowledge-building correspond to the well-known perspectives presented by Kuhn (1962). In practice, the distinction between cumulative and dialectical processes is often difficult to establish, especially given the tendency of many scholars to set up straw men (often caricatures of dominant explanations or out-of-date characterizations of the state-of-the-art) in order to increase the visibility and appearance of originality of their own work. They may appear to be engaged in a dialectical process while in fact adding only incrementally, if at all, to a currently prevailing explanation. Moreover, there are generally a variety of widely accepted and overlapping prevailing explanations in circulation at any given time.

elaboration of them.<sup>8</sup> In general, individual scholars are closely identified with the generation and "popularizing" of these alternative explanations. With time, of course, many of these alternative explanations become widely accepted and may even become dominant within a given field of scholarship.

## 2. Cooperative Agreements as Cumulative Knowledge-Building

The nine cooperative agreements reviewed in this document have contributed to social science research on rural development almost exclusively through a cumulative process of knowledge-building. In general, then, the research undertaken through the cooperative agreement mechanism has not established major new understandings of the process of rural development in terms of its causes and implications. Because their contributions have been cumulative, it is difficult to specify what "new" knowledge has been generated through the nine projects; the source of incremental knowledge is by definition rooted in prior research and established modes of analysis.

That their contributions are not conclusively original does not imply that the contributions of the cooperative agreements have been insubstantial. On the contrary, our broad review of the research output indicates that they have generated important insights about rural development. In particular, the nine cooperative agreements

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<sup>8</sup> A good example of this is the way in which dependency theory challenged a dominant set of assumptions and theories about development in the 1960s and 1970s; by the 1980s, it had become a major school of thought in the development field.

reviewed here have been very successful in: (1) assembling and assessing broad literatures on particular topics in state-of-the-art papers; (2) compiling a number of case studies and applied consulting reports that contribute to existing data bases; (3) formulating or expanding middle range conceptual frameworks for analyzing particular aspects of rural development; (4) developing clear and definitive conceptualizations of ideas that have great potential to improve the performance of rural development projects; (5) generating a rich set of testable hypotheses about rural development projects and programs; (6) establishing a series of working hypotheses that are sufficiently reliable to serve as the basis of action by practitioners; (7) drawing together multidisciplinary groups of skilled professionals to undertake research on common themes; (8) training of promising graduate students in the substantive areas of cooperative agreements; (9) building a large network of scholars and practitioners with a broad appreciation of the third world; and (10) publishing a wide range of books, monographs, and articles that are widely used by academics, applied professionals, and students in both donor and developing countries.

### 3. Assessing the Knowledge-Building Contributions of Cooperative Agreements

To explore these and other contributions, we assess the knowledge-building impact of the nine cooperative agreements on social science knowledge from three perspectives: (1) canons of research inquiry; (2) criteria for academic contributions; and (3) tests of research relevance. The first perspective judges the research product

against the standards generally accepted in the social sciences as central ingredients of high quality research--theory, conceptualization, hypothesis formation, hypothesis testing, methodology, and empirical generalization.<sup>9</sup> The second perspective considers the contributions to knowledge-building through influence on the academic community through publications, training, and networking. Finally, the third perspective assesses the relevance of the knowledge offered to the programmatic needs of those involved in applied policy making or problem solving. The evaluation from all three perspectives is purposely kept brief and general. Inevitably, therefore, it does not fully reflect the richness of perspectives, analysis, and detail found within each project. Much more detailed reviews of each of the cooperative agreements are presented in Section II of this report. In these individual reports are to be found significant exceptions to some of the broad generalizations that follow.

### C. How Well Does the Research Meet Canons of Social Science Inquiry?

#### 1. Building Theory

Theory: A systematic and interrelated set of principals and relationships proposed to explain observed phenomena at a high level of generality or abstraction.

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<sup>9</sup>In the pages to follow, each of these "ingredients" is explicitly defined. The definitions used are not the only usages of the terms, but represent their rigorous use in social science research. Our reference at the general level is Kaplan (1964).

The nine cooperative agreements have not contributed substantially to the formation of general theories of rural development, although the work they have accomplished is clearly of relevance to those more broadly concerned with theory building. At the broadest level, a theory of rural development would explicitly specify a broad set of processes and relationships to explain the dynamics and nature of change in rural areas. Such undertakings are noticeably absent in the research documents reviewed.

Instead, the cooperative agreements have pursued research on the basis of a number of basic assumptions that largely preclude significant contributions to general theory building about the process of rural change. In large part, this is a result of the pragmatic and applied emphasis in the field of rural development itself. Rural development specialists tend to begin with a specific problem that needs solution rather than with a more abstract interest in understanding change. Moreover, rural development specialists tend to share a common and implicit set of assumptions about rural development that is reflected in the cooperative agreement research output. While detailed lists of assumptions for each cooperative agreement are set forth in Section II, their overall thrust can be described as follows:

- a. Rural development means increasing production and productivity among rural inhabitants to increase their welfare and income while reducing levels of inequity among them. It is a broad process involving economic, technological, and social transformation.
- b. Rural development can be achieved through a gradual process of change within a given politico-economic structure; radical changes in society are not an essential precondition to rural development.



- c. Rural development can occur in a reasonably short period of time if the right mixture of technology and capital can be applied by governments committed to development and willing to make necessary institutional changes needed to achieve it.
- d. At the heart of rural development is the promotion of small farm strategies that emphasize output per land unit through the use of labor-intensive husbandry practices backed by research, extension, and marketing improvements.
- e. Rural development is best pursued at the program or project level; if the program or project is well designed and implemented according to the tenets of good management<sup>10</sup>, rural development will be accomplished.
- f. Public and private organizations engaged in rural development activities (and the individuals who work within these organizations) can be made to operate more effectively through training, education, reorganization, or reorientation.
- g. Beneficial technological change can be identified and introduced into rural communities.
- h. Participation by beneficiaries in rural development interventions increases the probability that they will be successful.
- i. The rural household is the basic unit of production and consumption in rural areas.
- j. Rural inhabitants are interested in rural development and the programs and projects that purport to promote it.

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<sup>10</sup> An exception to this is the research on rural financial markets at Ohio State University that has taken a broader policy focus. So, too, is Michigan State's off-farm employment study.

Taken together, these assumptions militate against asking the basic questions: (1) what causes rural areas to change? and (2) what are the implications of these changes for rural inhabitants?<sup>11</sup> The attention of researchers is instead focused on immediate policy issues or on shorter-term programs or projects and not on broader historical and dynamic processes of change that affect rural inhabitants. From the perspective of field practitioners, this may be advantageous, for the focus on the concrete interventions encourages situation-specific research on issues of interest to practitioners. Moreover, the assumptions underlying the research efforts are clearly related to the cumulative nature of knowledge-building apparent in the cooperative agreements. Nevertheless, to the extent that the underlying assumptions are mistaken or overly generalized, the validity of resulting policy, program, and project relevant research is questionable.

The cooperative agreements reflect a more general "theoretical crisis" in rural development that is evident in: (1) the lack of overarching, broadly shared theories of rural change; and (2) the inconsistency among a wide range of ideological views, disciplinary perspectives, and middle range conceptual frameworks. Greater concern for theory would be a fundamental contribution to the general field of rural development for it would help: (1) establish a set of over-

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<sup>11</sup>In contrast, major contributions to theory about the cause and nature of rural change can be identified in the literature on dependency, world systems, peasant "rationality," revolution and peasant movements, and communications.

arching causal relationships from which a variety of subordinate hypotheses could be derived; (2) focus attention on important concepts that need to be defined and refined; (3) establish relationships among a wide variety of components of successful rural development projects; and (4) encourage both researchers and practitioners to be more self-conscious about the benefits of the interventions they propose.<sup>12</sup>

## 2. Elaborating Concepts

**Conceptualization:** The definition and operationalization of ideas and relationships implied in theories and hypotheses.

The nine cooperative agreements under review here have made important contributions to the conceptual clarity and operationalization of ideas and relationships that are frequently held to contribute positively to rural development projects and programs. Conceptualizations of this nature have made it possible to identify and measure these factors in practice. The most impressive example of conceptual work that has resulted from these cooperative agreements is that of participation in rural development projects developed by Cornell researchers. The project on managing decentralization at Berkeley also produced a clear and researchable concept of decentralization. So, too, did Ohio State through its efforts to clarify notions of

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<sup>12</sup>Our understanding of the cooperative agreement philosophy suggests that this approach may not be appropriate for addressing such theoretical needs. Nevertheless, given AID's progress in expanding the knowledge base about rural development, it seems imperative that sustained attention be given to the "theoretical crisis" in rural development.

borrowing and lending costs, and the Land Tenure Center through its elaboration of the concepts of community and common property rights. In these cases, the research output presents concepts that have multiple but clearly defined dimensions from which it is possible to derive testable hypotheses.

Work at Michigan State University on farming systems research, while less definitive than that on participation at Cornell and decentralization at Berkeley, has made an initial attempt at specifying this concept more concretely. In other cases, as with the concepts of "learning process" and "bureaucratic reorientation" introduced by the NASPAA project, much conceptual work needs to be done before it can be argued that a real contribution to social science knowledge-building has occurred. In general, focused and self-conscious attention to conceptual definition and clarity is basic to cumulative knowledge-building processes. As such, those cooperative agreements that have paid significant attention to it have been the most successful in contributing to social science knowledge about rural development.

### 3. Formulating Hypotheses

**Hypothesis:** The statement of testable cause and effect relationships, often (but not necessarily) derived from more abstract theory.

The nine cooperative agreements have not been notably self-conscious about generating hypotheses to guide research undertakings. In particular, there is a tendency to fail to distinguish between hypotheses and normative judgments or advocacy statements. In

addition, many statements that can be identified as hypotheses for social science and rural development are actually the conclusions derived from research rather than statements of cause and effect relationships posited to guide research.

Nevertheless, a series of important hypotheses can be extracted from the research output that, as hypotheses, are of major importance to the study of rural development in the social sciences. These are presented in some detail in each of the nine individual reviews in Section II. Among the most important for social science knowledge-building are those that specify conditions under which particular outcomes are likely to be accomplished. To the extent that individual projects have focused on issues of this nature and developed specific hypotheses about cause and effect relationships, they can be credited with important contributions to cumulative knowledge-building about rural development.

The output of the research effort is also rich in suggesting working hypotheses for additional research. In many cases, such as the work done by Cornell, Development Alternatives, Michigan State (off-farm employment), Ohio State, and Berkeley, the working hypotheses are sufficiently established to form the basis of guidelines that can be adopted by field practitioners with a reasonable amount of confidence. The establishment and bringing together in coherent form of these working hypotheses constitutes a valuable contribution of the research; rigorous testing and the development of second-order hypotheses should now be the focus of concerted research.

#### 4. Testing Hypotheses

**Hypothesis Testing:** The formalized scrutiny of the validity of testable cause and effect relationships through the collection, ordering, and analysis of data.

The nine cooperative agreements reviewed here have tested a large number of hypotheses through the collection and analysis of various forms of data. In general, as with hypothesis formulation, testing of hypotheses has not been sufficiently self-conscious to lend a high degree of validity to the research findings. More specifically, there is a frequently observed failure to establish a distinction between a hypothesis to be tested through a concrete strategy of data collection (a methodology) and an empirical generalization resulting from the analysis of data. The general pattern in reports is to state a proposition and to assemble data in support of that proposition. This has increased the tendency in the research output to cloud the distinction between hypothesis and empirical generalization.

There are, however, a number of good examples of self-conscious hypothesis testing to be found in the research output of these cooperative agreements. The work at Ohio State University on differences in transactions costs across various groups of borrowers and lenders in rural areas is a notable example. Similarly, a number of studies produced by the project on rural development participation at Cornell have paid close attention to the importance of data collection and analysis to explore explicit cause and effect relationships. Hence, it is possible for AID to insist on the more self-

conscious statement and testing of hypotheses on future cooperative agreements.

### 5. Developing Methodologies

**Methodology:** An explicit strategy for the collection, ordering, and analysis of data.

Research undertakings of the nine cooperative agreements have been variable in their attention to methodological issues, including concern for the appropriateness of specific methodological tools to particular kinds of data. There are some excellent examples of fine social science research to be found within the research output. A number of reports are methodologically self-conscious and sophisticated in the collection, analysis, and interpretation of data. These are discussed in the individual reports and in an analysis of how to improve the methodological rigor of cooperative agreements found in Section III-A.

There are also a series of methodological problems that characterize a significant number of research reports. These problems also characterize much research in the social sciences more generally and similarly result in conclusions whose validity often cannot be sustained. Among the more significant problems are the following four:

- a. Researchers frequently fail to distinguish between empirical analysis and normative judgments. This is particularly true in research that takes a strong advocacy position.
- b. Researchers often adopt particular methodologies (a case study, for example), with insufficient discussion of why it is appropriate to the research questions or hypotheses. Thus, they fail to justify their choice of methodology.

- c. Researchers fail to justify adequately the specific selection of cases to be examined. It is important to the validity of the findings to establish if the cases are considered to be representative of a broader population of cases (and if so why), if they are the only cases available, or if they are "test cases" of important relationships.
- d. Standards of evidence and inference for particular data sets are typically left undiscussed in the research output. In particular, if the data are poor or incomplete (as is frequently the case with research in the third world), the knowledge-building enterprise would be well-served by a discussion of their limitations and the extent to which inferences can be drawn from them.

#### 6. Establishing Empirical Generalizations

**Empirical Generalization:** An empirically tested hypothesis that is found to hold true under specified conditions in a non-trivial number of cases.

There are a wide variety of important potential empirical generalizations in the research output of the various cooperative agreements. However, because of the very broad range of factors that impinge on the process of rural development and because of the high degree of variability of social, economic, political, and historic conditions in the third world, it is difficult to use any hypothesis testing exercise to establish an important empirical statement of wide generality. Moreover, because of the frequent lack of attention to explicit hypothesis formation and to methodological issues, the results of the research are difficult to characterize as clear empirical generalizations. Thus, working hypotheses are more likely to be the outcome of any specific study rather than important empirical generalizations.



The nine reviews in Section II and the review of policy implications in Section III-B outline some of the better established "working hypotheses" to emerge from the cooperative agreement exercise. At a minimum, these suggest that the research model underlying the cooperative agreement approach has the potential to move toward more broadly useful and reliable empirical generalizations.

D. How Has the Research Influenced the Academic Community?

1. Attracting Recognized Experts

The cooperative agreements reviewed here have been carried out at some of the most respected universities in the country. The only exception is the one on Organization and Administration of Integrated Rural Development executed by one of the more academically-oriented consulting firms working in the area of rural development. Perhaps more important, the cooperative agreements have involved some of the most well-established academic experts in the field of rural development. They have also attracted promising graduate students, individuals who will one day have reputations of their own.

A review of the bibliographies for each of the nine cooperative agreements confirms that many of the most innovative and thoughtful experts in rural development were attracted to work under the quid pro quo conditions of the cooperative agreement mechanism. Clearly, doing applied field work on the types of projects reviewed was not seen as a barrier to academic advancement. In addition, the bibliographies suggest that the cooperative agreement mechanism has

served to promote the involvement of a wider range of social scientists than is usually the case. In an area generally dominated by agricultural or development economists, the cooperative agreement mechanism has promoted the involvement of geographers, anthropologists, rural sociologists, political scientists, and specialists in organization and administration. It should be noted here that the cooperative agreements also contributed to broadening the notion of rural development, particularly through the choice of cooperative agreement topics, but also through the selection of cooperators and social scientists to work on them.

## 2. Establishing Research-Consulting Credibility

Cooperative agreements have involved experts in short- and long-term research work. Academics generally assume that short-term consulting on data gathering, project design, training, diagnostic problem solving, and evaluation is unlikely to contribute to the knowledge-building output of each project. Indeed, the principle publications of the more productive cooperative agreements appear to rely heavily on the output of the long-term mission work products. For example, Development Alternative's own long-term projects in North Shaba and Arusha play a bigger role in sustaining points than many of their short-term assignments under the cooperative agreement. Cornell's sustained work effort in Botswana, Sri Lanka, and Jamaica had considerable impact on its knowledge-building activities, while its short-term work in such places as Dominica did not.

While short-term work is an essential component of the cooperative agreement model, ways need to be found to ensure that it relates to the knowledge-building efforts of the project. Otherwise valuable "academic opportunity costs" will be incurred. Ideally, activities should be systematically chosen to provide valuable cases for generating the kind of data that contribute to the cooperative agreement's central knowledge-building concerns. An important issue to consider, therefore, is how AID can generate applied consulting requests by missions in countries with development patterns closely related to particular cooperative agreement topics. This problem needs to be addressed if the knowledge-building output of cooperative agreements is to be enhanced and summary reports are to have greater academic acceptance.

A similar "academic opportunity cost" is incurred because many cooperative agreement contracts do not allow enough time for the final consolidation of knowledge learned. The typical cooperative agreement: (1) begins with a state-of-the-art paper while mission activities are being developed; (2) surges forward in the middle and final years with the publication of field-generated studies; and (3) ends without completing careful synthesis work leading to a revised state-of-the-art paper or guidelines for field practitioners. This suggests that the broader a cooperative agreement's mandate and/or the more productive its applied field work, the more the need to ensure an extra year of funding for the core staff to do synthesis work unburdened by mission consulting obligations.

### 3. Publishing Research Findings

Publication and dissemination is central to knowledge-building. It is also important to university and expert incentives, a fact critical to AID's ability to attract highly qualified specialists. In this regard, while some cooperative agreement projects have been more prolific than others, nearly all have disseminated their work products widely. This has largely been done through in-house publication series with distinctive covers and formats. In our opinion these have been made widely available to outside experts, both in the United States and abroad. As evidence of this, we note that these publications are increasingly being cited in the general literature.

Some of these products have been submitted to professionally refereed journals and subsequently published. Ohio State's work on rural credit and MSU's on off-farm employment in particular have been notably successful in this regard. Two books were generated through the Berkeley project on decentralization and important results of the Cornell project will also be published in book form soon. These publications are a testimony to the academic credibility of some cooperative agreement products and a recognition of their contribution to knowledge-building. It can be anticipated that over the next few years, the number of professionally refereed publications flowing from AID's investment in cooperative agreements will substantially increase. Illustrative of this are Milton Esman and Norman Uphoff's forthcoming book on local organizations, David Korten and Rudi Klaus'

soon-to-be published collection of papers on local empowerment and self-help, and George Honadle and Jerry VanSant's nearly completed study on integrated rural development. Also illustrative is the publication of Development Alternative's "Beyond the Rhetoric of Rural Development Participation" in the respected journal, World Development, just as this review was being completed.

The bibliographies at the end of each of the nine reviews in Section II in no way illustrate the extent of publication the cooperative agreements have generated. Two examples demonstrating this point can be given. First, Cornell's team that worked for the Yemen mission on local development associations published three major reports, 15 focused studies, and several academic papers and journal articles that substantially consolidated and advanced existent knowledge about rural development in that little-studied country. Similarly, Berkeley's field work in Kenya allowed David Leonard not only to prepare papers for the AID mission but also enabled him to work on problems of budget and finance that led to the publication of the first professionally refereed articles on the use of microcomputers in development ministries. Other examples could be given. Indeed, if the full spin-off of the nine cooperative agreements could be documented five years from now, the fuller implications of AID's investment in knowledge-building would be evident.

#### 4. Legitimizing New Topics of Research

Several research products of the cooperative agreements have contributed effectively to the acceptance or legitimacy accorded to

important topics. For instance, participation has become an accepted component of a wide variety of rural development projects promoted through AID, an outcome that can in large part be credited to the work at Cornell. Farming systems research has become more credible and acceptable among agricultural researchers in part because of the work done by Michigan State University. Ohio State's work on rural financial markets has helped legitimize market-oriented credit policies, and Michigan State's research on off-farm employment has stimulated increased attention to the importance of small-scale enterprise in rural areas. In other cases, even when ideas are still vague or poorly tested, they have captured the attention and imagination of practitioners and researchers. The work by David Korten and others on "learning process" and "bureaucratic reorientation" through the NASPAA-DPMC agreement is a good example of this. It has generated much lively discussion and considerable interest in further research.

##### 5. Promoting Involved Social Science

Development practitioners often charge that academics are not constructive and that they specialize in using the "bright light of hindsight" to point out what went wrong and why. In our opinion, the tone and content of many of the publications reviewed in Section II is different from that found in many ex post studies. This suggests that the cooperative agreement mechanism has allowed academics to shift from a perspective of "negative social science" to one of "existential social science." This is particularly the case with younger researchers who have not worked with donor agencies before or who have not

been responsible for implementing activities. Involving the major producers of development literature in designing and implementing donor interventions, has major implications for issues of program relevance. Specifically, it increases the sensitivity of academics to the constraints of reality, teaches specialists about the problems faced by those trying to make rural development interventions work, and promotes an experience of praxis which can increase the utility and value of their future writing to development professionals. While not all academics are likely to be so affected, it is clear that a number of the authors of the research reviewed were conscious of the need to relate their research and writing to applied field problems.

#### 6. Training New Specialists

A difficult-to-measure contribution to knowledge-building involves the training of a cadre of broadly educated specialists in rural development. Each of the cooperative agreements has brought together scholars sharing a common set of interests to explore a common set of problems. They have, as part of their work, taught students, employed doctoral and masters' degree candidates as research assistants, and served as advisors for a significant number of theses. Many students have undertaken field research in pursuit of their assistantships or dissertations and these field experiences have doubtless had an important impact on the intellectual and philosophical formation of those involved. A number of individuals from third world countries have also been intimately involved in the work or teaching resulting from the cooperative agreements.

For example, Cornell's project identified and helped develop the applied skills of Harvey Blustain, Arthur Goldsmith, Kathryn March and Arthur Williams; Michigan State's projects advanced the work of Dunston Spencer, Michael Weber, and John Strauss;<sup>13</sup> Berkeley's brief activities introduced Stephen Peterson to AID officers; and the Land Tenure Center's research brought Steven Lawry and James Riddell to the attention of the development community. Others could be named, for many of the country studies involved young people who will one day be part of an established pool of experts on rural development. The importance of this is emphasized by noting that some directors of cooperative agreements (such as Carl Eicher, Dale Adams, and Norman Uphoff) and even the institutions themselves (such as the Land Tenure Center, Rural Development Committee, or Development Alternatives) gained substantial experience from work on prior AID activities. Clearly, one aspect of the cooperative agreement mechanism's success in knowledge-building has been its potential for training new generations of specialists and building or enriching existing centers of expertise.

#### 7. Strengthening Multidisciplinary Work and Knowledge

Most of the cooperative agreement enterprises reviewed here are in agreement that a broad set of factors impinges on the process of rural development and that a specialist in rural development must

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<sup>13</sup>Indeed, MSU points out they have produced a dozen Ph.D.s, some of whom are now regular AID staff and all of whom will continue to work on off-farm employment topics.



be broadly familiar with knowledge from a range of disciplines. The cooperative agreements have significantly promoted the education and development of this kind of broadly educated specialist. Presumably, this expands the network of skilled professionals available to assist AID in the future. The interdisciplinary studies reported in the publications reviewed demonstrate the utility of such approaches.

These range from the overview studies by Development Alternatives that combined economists, political scientists, and anthropologists, to the country- and problem-specific studies by Cornell which forged effective teams of country specialists and generalists in the various social sciences.

However, while expanding the notion of multidisciplinary research and building models for carrying it out, the overall cooperative agreement exercise has failed to promote linkages between individual cooperative agreements. Indeed, some projects have ignored the work products of others: for example, Development Alternatives' thinking on participation in integrated rural development projects has eschewed consideration of Cornell's sustained progress on that topic. Hence, in our opinion there is a substantial need to: (1) forge linkages between the work products of the projects reviewed here; and (2) undertake activities in the field which focus on two or more of the kinds of issues which were the centerpiece of earlier cooperative agreements. Of necessity, such work would have to be interdisciplinary.

## 8. Forging Networks of Specialists

Interactive networks are important to academic evaluation of knowledge-generating activities. Publications reviewed in Section II are not sufficient to establish that the cooperative agreements succeeded in their network functions. However, those newsletters reviewed indicate how important they can be to the expansion of new knowledge. Of these, the Cornell Rural Development Participation Review provides a model for mixing short articles, reviews, and news in a format likely to be successful in holding networks together and spreading knowledge generated by the project. To sustain networks, it is essential to share research products; the distribution of newsletters is not enough. Clearly, the wide distribution of publications from Cornell and Development Alternative, for example, testifies to the knowledge-sharing potential of networks. On the other hand, the limited capacity of the Land Tenure Center to distribute its findings appears to have prevented interested specialists from drawing upon its work product, the LTC newsletter notwithstanding. In general, most cooperative agreement contracts have provided ample funds for in-house publication and distribution of findings, a funding practice that should be continued.

Again, it must be noted that interaction between the cooperative agreement projects appears to have been inadequate. Early on, core members of the projects met to discuss their substantive activities. Unfortunately, this practice did not continue. In our opinion, more direct networking between core staff of projects would address problems discussed in other sections of this review.

## **E. How Relevant is the Research to Practitioners?**

### **1. Providing Useful Research Products**

A review of the research output of the nine cooperative agreements does much to dispell the shibboleth that studies by academics are unlikely to be useful to field professionals. There is much in these publications that can lead to fuller analysis of task environments, better calibration of policy advice, improved program and project design, timely diagnosis of implementation problems and management opportunities, useful criteria for formal evaluation, and enhancement of general planning capacities.

We have not collected data that would enable us to address whether missions have found the studies reviewed in Section II to be useful. It should be noted, however, that an earlier AID evaluation concluded that they were.<sup>14</sup> Moreover, we have not reviewed the more fugitive reports and memoranda that are often important to the direct action needs of field missions. Still, in our opinion, the research output of the nine cooperative agreements contains much that is of potential value at the operational level of policies, programs, and projects. As indicated, researchers have in general taken donor interventions as the central focus of the concern and have asked the important question, how can the performance of rural development efforts be improved? Some of the research efforts have taken care to generate explicit and operational guidelines to assist program and

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<sup>14</sup>Montgomery, Carroll, and Robinson (1981:21-29).

project officers in their activities. This has been a particularly strong characteristic of the work on integrated rural development undertaken by Development Alternatives, Inc. The central issue addressed in this part of the report, however, is whether the cooperative agreements have generated programmatic insights that are operationally accessible. In short, how well have the nine cooperative agreements communicated programmatically relevant knowledge to practitioners?

## 2. Reaching Field Practitioners with New Ideas and Approaches

The means of communicating programmatically relevant knowledge are varied: (1) state-of-the-art papers; (2) reports on long-term applied research; (3) handbooks; (4) newsletters; (5) country and project specific evaluations and reports; (6) seminars and workshops in Washington and abroad; (7) technical assistance missions; (8) personal communication; (9) videotapes; and (10) simulation exercises. We have only reviewed systematically the written documentation of research in the first four categories, and therefore are unable to judge other forms in which knowledge might have been disseminated. However, it is clear that the communication of programmatically relevant knowledge in the research output we reviewed is highly variable, ranging from quite high to minimal.

An important difference among the research reports is the extent to which they are consciously addressed to rural development practitioners. It is apparent, for example, that the reports generated by DAI are clearly intended for practitioners, not academic

colleagues. The language, length, and format are all designed to communicate effectively with busy officials who have little time for reading or reflection. Similarly, some of the reports generated by the Michigan State project on alternative rural development strategies directly address individuals who are responsible for the design and implementation of agricultural research and extension programs.

It is equally apparent that researchers at Berkeley and the regional planning and area development project at the University of Wisconsin were attempting to communicate primarily with academic colleagues. Their reports are lengthy and often abstract. Documents produced by Cornell, Ohio State University, and the Land Tenure Center tend to fall between the two extremes. There is no one-to-one correlation between good research and good knowledge dissemination. The audience addressed by the research output is more important in determining whether programmatically relevant knowledge is disseminated effectively to practitioners.

An important implication of these observations is that a critical component of knowledge-building research through the cooperative agreement is an explicit strategy for disseminating this knowledge to practitioners in Washington and abroad. A dissemination strategy would include (1) a concise statement of intended audiences and means to reach each of them; and (2) a plan for gathering, synthesizing, testing, codifying, and presenting information in response to the needs and interests of the intended audiences. To our knowledge, only Development Alternatives has given much attention to

an explicit dissemination strategy. However, even if all the projects had focused explicitly on developing a concrete dissemination strategy for their research materials, their efforts might not have produced much of positive utility because it is not presently known within AID or the cooperating institutions what the most effective means are for reaching field practitioners.

The cooperators would be well-served by an in-house survey, evaluation, and set of recommendations produced by AID to give guidance on appropriate methods to disseminate knowledge to practitioners. This would be a cost-effective means to provide guidance to all cooperating institutions and to generate criteria by which to evaluate their programmatic utility. If such guidance were made available by AID, then the failure to communicate effectively could be laid more squarely at the door of the cooperating institutions. Currently, a considerable amount of this failure must attach to AID itself. With more guidance, the cooperators could be assessed on the basis of their efforts to develop and implement a dissemination strategy by asking the following questions: (1) Was there a dissemination strategy? (2) Was it followed? (3) Were there any documents specifically prepared as operational or policy guidelines? (4) What media were utilized for reaching various audiences? (5) How widely disseminated were these outputs? (6) Were any documents translated into other languages?

### 3. Producing Guidelines for Action

It is our judgment that a large number of rural development policies, programs, and projects could be measurably improved if what

we have described as working hypotheses generated by the nine projects were seriously operationalized at the field level. However, this judgment is incomplete unless it is also noted that the nine projects must make the results of their research not simply field relevant (what much of it is) but also field accessible (what is characteristic of only a few projects). That is, programmatic guidelines that are buried within weighty documents or that are stated in abstract or academic prose can be highly relevant to practitioners but quite inaccessible to them. Practitioners have severe limitations imposed on their time and they need to resolve specific problems within focused interventions in the most timely fashion.

Preparation of state-of-the-art papers is generally done early on in a cooperative agreement. A few projects end with the publication of a book that summarizes accumulated experience and findings. Neither is adequate for the field practitioner. If AID is to capture the output as guidelines, then publication must be made a specific contractual requirement for the final period of the project. Otherwise, the rush to complete mission work or the need to publish an academically acceptable manuscript will take precedence over this essential concluding exercise.

#### 4. Building AID's Institutional Memory

AID's Washington and mission offices witness the passage of an extensive number of studies and the gradual turnover of staff specialists. Clearly, a major problem for the agency is the maintenance of an institutional memory of what has been learned that is applicable to current and future problems.

The scope of the cooperative agreements reviewed allows a substantial opportunity for consolidating and updating this steady flow of information. State-of-the-art papers and guidelines can do this, particularly if they are informed of AID's internal reports and can meld them well with academic literature and case studies. In addition, special reports in cooperative agreement publication series can make the innovations of one mission known to other missions.

The key to this appears to be the designation of AID project officers who know the specialty area of the cooperative agreement and have the drive to contribute actively to the intellectual activities of the project. It is relatively easy to pinpoint which of the nine cooperative agreements had such officers. Still, there are no examples of such officers participating in the drafting of project studies. This is unfortunate, for many of the AID officers have both the academic training and experience to contribute to this kind of undertaking.

Whether officers are actually involved they should play a conscious role in ensuring that the agency's steady flow of reports and case studies are selectively channeled to the contractor they manage. And they should have the experience to review publication output and promote its linkages to AID's institutional history and accumulated knowledge.

In addition, the research output of the cooperative agreements funded by AID should be fully catalogued and indexed in a format that is both useful and accessible to practitioners in Washington and in



the field. Currently, there is no such listing and the potential to retrieve information is clearly limited. The creation and maintenance of a rural development index of publications should be a priority area for extending the impact of the cooperative agreements in a way that enhances AID's institutional memory.

#### 5. Benefiting Host Governments

The publications reviewed in Section II do not include trip reports or workshop materials. Even if they did, it would not be possible to decide if the cooperative agreements benefited host governments. One measure of benefit is whether such research increased the probability of program intervention success. This cannot be determined here. However, several observations can be made that are related to recommendations for future cooperative agreements.

Our experience working in LDC development ministries makes us question how useful the studies and papers we did review might be to host governments. Of particular concern is length. If many of the publications are too long for busy donor officials, they are entirely too long for civil servants and specialists from the third world. There is also the language problem. Few of these materials have been translated into other languages. Examples of what might be done vary from Development Alternative's translations of full report to Cornell's Yemen studies which contained Arabic summaries. Finally, there are few examples of specialists from the third world actively participating in major reports. A good example of the value of such involvement is the work of E. Chuta on non-farm employment at Michigan

State. That cooperative agreement also stands out in its use of foreign graduate students.

Clearly, achieving the objectives of knowledge-building and programmatic utility would be enhanced through increased collaboration with host governments and national research centers and their staff. Such collaboration, however, must be firmly supported by the missions.

#### F. Recommendations for Increasing Knowledge-Building

##### 1. Making Research More Rigorous

In our judgment, the cooperative agreement mechanism has resulted in substantial contributions to social science knowledge about rural development. At the same time, our review of the research output of the nine projects indicates that the contributions of the cooperative agreements would be enhanced with self-conscious attention to more rigorous standards of social science research. Specifically, we recommend the following actions:

- a. Initial work plans for cooperative agreements should contain explicit consideration of broad research strategies, needs, and appropriate methodologies. They should contain:
  - 1) A clearly identified topic with well-defined boundaries that distinguish it from related research enterprises.
  - 2) A concrete discussion of the most important questions or hypotheses that need to be addressed.
  - 3) A long-term flexible work plan for exploring these questions and hypotheses, including concern for how they will be studied and where research will be carried out. This should include a plan for long-term applied research projects and a plan for

capitalizing on short-term technical assistance missions to generate comparable data sets.

- 4) An explicit discussion of the methodologies most appropriate to research on the topic in question and the kind of data most appropriate for its analysis.
  - 5) A strategy for methodological modifications should time constraints or lack of data impede the research plan.
  - 6) Points 1-5 notwithstanding, flexibility should exist to alter the identified topic and related questions and hypotheses if research suggests that the initial problem was incorrectly or inadequately defined.
- b. Initial work plans for cooperative agreements should be broadly self-conscious about the conceptual or theoretical framework underlying the identification of research problems, generation of assumptions and hypotheses, and the analysis of data produced.
- 1) Where possible, researchers should be encouraged to point out how their approach or analysis might have been different had they followed alternative conceptual or theoretical frameworks.
  - 2) Researchers should be encouraged to link their analytic work to larger efforts by others to increase the theoretical sophistication of the field of rural development.
- c. The initial task of a cooperative agreement team should be to produce, within a three-month period of start-up, a coherent and analytic state-of-the-art paper. This document should contain:
- 1) An explicit consideration of alternative theories of rural development as they are relevant to the topic of research.
  - 2) An identification and operational definition of concepts central to the topic of research,
  - 3) An explicit discussion of frameworks for analysis of the topic.

- 4) An initial statement of important hypotheses that need to be tested through analysis of existing data or field research.
  - 5) Where appropriate, a consideration of the conditions under which important hypotheses are expected to hold true.
  - 6) A discussion of appropriate methodologies for expanding knowledge about the topic of research.
  - 7) A proposed strategy that provides direction for long- and short-term research under the cooperative agreement.
- d. Long-term research projects for particular missions should be carefully selected to ensure that they become cases that contribute to building knowledge relevant to the cooperative agreement's research topic. Providing service to missions should not take precedence over topic relevance in the selection of long-term research relationships.
  - e. Individual research monographs produced under the cooperative agreement mechanism should be encouraged to set forth explicit discussions of theory, concepts, hypotheses, data sources, methodological choices, and data quality. Project management should be responsible for certifying the quality and readability of individual research monographs prior to their publication under the cooperative agreement.
  - f. Literature reviews on the topic of research should be undertaken only after explicit reasons for doing so have been established and a set of important questions the review is expected to address is established.
  - g. Individual research monographs and literature reviews should be explicitly linked to the concepts, frameworks, and directions for research established in the initial state-of-the-art paper.
  - h. Individual research monographs should be explicit in differentiating among hypotheses, empirical generalizations, normative judgments, and advocacy statements.
  - i. Project management should oversee in self-conscious fashion the quality and tone of individual research

outputs as well as ensure that individual reports coincide with the long-term research objectives of the cooperative agreement.

j. Among the final products of a cooperative agreement should be a revised state-of-the-art paper that reflects knowledge gained after the initial paper was written at the start of the project. This should be done as part of a Final Report.

- 1) The Final Report of each cooperative agreement should include a concise statement of "what has been learned." In this statement, consideration should be given to how technical assistance missions and long-term applied research efforts have contributed to the research output.
- 2) The Final Report should be particularly conscious of the multiplier effect of the research to enable AID to assess the secondary effects of its research program.
- 3) The Final Report should be explicit in pointing out how ongoing processes of change are likely to alter the cooperative agreement's research findings and in identifying important new research gaps revealed through the research process.

k. Cooperative agreements should be extended for additional periods when it appears that more time is likely to lead to a useful consolidating report. Extension should also be considered where it is clear that further research on the topic is essential because of expanded understanding of the issues or because research findings are being altered by processes of rapid change.

## 2. Making Cooperative Agreements Contribute to Building Expertise

The process of knowledge-building has important side effects that AID should recognize and encourage. Foremost of these are the training of future American experts, the building of expertise in developing countries, and the dissemination of research findings to research and training centers. Based on our judgment that the work

product of graduate students is of generally high quality and our awareness that there was little AID or third world contribution to the research publications reviewed, the following recommendations are made:

- a. AID should encourage the use of graduate students in cooperative agreement research activities, provided they work in conjunction with and under the supervision of recognized experts.
- b. Where possible, cooperators should be encouraged to work with development research institutes in countries where they are undertaking long-term research for missions.
- c. Cooperators should be encouraged to involve foreign graduate students on their cooperative research assignments.
- d. The publication of Ph.D. dissertations based on cooperative agreement research should be encouraged by AID and given financial support through the project funds.
- e. AID project managers and mission personnel should be encouraged to play substantive roles in the generation of cooperative agreement research.
- f. AID should recognize the importance of wide distribution of the knowledge generated through publications that meet academic standards. Toward this end, cooperative agreements should provide ample funding to encourage the widest possible distribution of research findings to experts in other institutions who are involved in research and training activities.
- g. Research reports should be produced in a well-designed format that has an identifiable logo. Papers should be numbered and efforts made to ensure preservation of reports in a few selected research libraries and donor archives.
- h. At a minimum, each country-specific publication should contain a brief translated summary in the country's language. Important synthesis papers should be translated into French and Spanish.

- i. Networking is important and should be continued. In this regard, AID should encourage the publication of project newsletters as the foundation for networking activities.
- j. Aside from promoting networks, AID should make specific efforts to bring principal researchers working under all cooperative agreements together for annual discussions about their findings and possible linkages between them.

### 3. Making Research More Relevant to Practitioners

In our judgment, the research output of the nine cooperative agreements holds considerable potential for improving the performance of rural development programs and projects. The major problem encountered in the research output is not necessarily whether it is relevant but how its relevance can be communicated effectively to the rural development practitioner. The contributions of the cooperative agreements to programmatic improvements can be measurably enhanced through a number of easily implemented actions. Specifically, we recommend the following actions:

- a. AID should undertake a modest but critical in-house survey and analysis of the methods that prove most effective in communicating programmatic knowledge to regional bureaus, field missions, host country officials, and other rural development specialists involved in AID-funded projects and programs. The results of this study should be made widely known to present and future cooperators.
- b. Proposals for undertaking cooperative agreements should contain an explicit and well-defined dissemination strategy for reaching rural development practitioners with programmatic knowledge. This strategy should include a statement of:
  - 1) intended audiences;
  - 2) what their needs and interests are;
  - 3) the range of channels and formats available; and
  - 4) how the strategy is appropriate for reaching them.

- c. Budgets for cooperative agreements should include ample funding for the pursuit of a well-defined and appropriate dissemination strategy. In particular, they should include appropriate funds for publishing programmatic guidelines and advice or making such available through appropriate media (videotapes, etc.)
- d. Annually, cooperative agreement project management at the cooperating institution should produce a report on the programmatic relevance of the research effort, indicating specific findings and providing data on the project's efforts in disseminating knowledge.
- e. Each research report published under the cooperative agreement should contain a section or chapter on the programmatic implications of the research. Where appropriate, researchers should prepare a programmatic abstract of the individual reports.
- f. Action-oriented guidelines should be a final product of a cooperative agreement project. Sufficient time needs to be provided in the project's final year to ensure that this task is completed.
- g. Project officers within AID should make vigorous efforts to evaluate and assist the implementation of the dissemination strategy within the regional bureaus and field missions.
- h. The research output of the cooperative agreements should be thoroughly catalogued and indexed. The index should be made widely available to practitioners.

#### 4. Making Cooperative Agreements More Cooperative

It is tempting to go beyond our terms of reference and reflect on the procedural and administrative aspects of cooperative agreements, for these affect the quality of research. We are cautioned, however, by the fact that this review is drafted on the basis of the written record and not on a review of the administrative processes that underlie it. Nevertheless, it may be useful to make several recommendations which can be deduced from that record.



On the basis of unsystematic discussions with cooperators and AID officials, it is apparent that AID is a highly pluralistic organization. Different views of the purpose of a given cooperative agreement often are held by different units within AID, and different research objectives often are sought by such units. Developing work plans and terms of reference require much administrative time. Often they are so substantive in nature that only researchers can negotiate them with central units, regional bureaus, and missions. Something needs to be done about this administrative time, for there are substantial "intellectual opportunity costs" to researchers charged with carrying out the contract and building knowledge.

Moreover, it is clear that AID and cooperating institutions have different interests and needs that they expect to fulfill through the cooperative agreement mechanism. Especially where the cooperative agreement is based in a university, the research team is primarily interested in long-term research opportunities and the opportunity to build a research team around a multiyear project. AID is primarily interested in having ready access to groups of skilled professionals who can offer specific technical assistance to field missions as well as provide policy, program, and project advice to field missions and the regional bureaus. In fact, an implicit bargain is struck between the agency and the cooperating institution: the opportunity to carry out research efforts is to be matched by cooperator's commitment to provide valuable and timely technical assistance to field missions and bureaus. While there are many AID officials committed to the impor-

tance of basic research and general knowledge-building, most of those with operational responsibilities are primarily interested in obtaining site-specific technical assistance in support of particular policies, programs, and projects. If the cooperating institution is not able to supply this, then officials do not consider the cooperative agreement to be useful. An important measure of the success of a cooperative agreement from the perspective of AID is the extent to which bureaus and missions are willing to commit project and program funds to obtain the services of cooperative agreement professionals.

Inevitably, there is tension in this relationship between AID's interest in how to make a policy, project, or program work better and a scholar's concern for critical analysis of broader issues. Nevertheless, we believe that a number of actions can be taken to ameliorate these tensions and to improve the performance of the cooperative agreement mechanism so that the interests of both the cooperators and AID are better served. Specifically, we recommend that the following actions be taken:

- a. During the first three months of a cooperative agreement, the Agency's project paper should be merged with the contractor's proposal to produce a specific statement of purpose and objectives that is agreed to by AID central bureaus.
- b. Annual work plans should emphasize substantive requirements as well as administrative and product requirements.
  - 1) To obtain quality research AID needs to make its expectations absolutely clear.
  - 2) Care should be taken to ensure that it is feasible to achieve such expectations within the research environment provided through AID.

- 3) While AID should set expectations and standards, it should allow the research institution to determine how to meet them.
  - 4) Recommendations and guidelines set forth in this report should provide the basis for identifying such expectations and standards.
- c. Project officers within AID are a critical component in the success of the "bargain" struck between the agency and the cooperating institution. Among positive qualities for selection are:
- 1) Thorough familiarity with agency operations and prior experience in missions or the regional bureaus of AID.
  - 2) Knowledge of the project topic and familiarity with the operations of academic institutions.
  - 3) A proven ability to interact positively with regional bureaus and field missions to promote the utilization of the personnel and knowledge of the cooperating institution. The ability to establish and maintain collegial relationships over an extended period within the agency and with the cooperating institution is essential.
- d. If at all possible, such AID officers should serve in that capacity for the duration of the cooperative agreement, unless they prove ineffective in promoting the mutual and productive objectives of the project.
- e. Project officers should be encouraged to participate in substantive research work, particularly the preparation of the state-of-the-art papers and guidelines.
- f. AID's project officers should begin early on to work with contractors, bureaus, and missions to identify research relevant short- and long-term consulting assignments that contribute to the knowledge-building objectives of the cooperative agreement.
- g. Project teams at the cooperating institutions should include senior members who have had experience at the mission level in working for or consulting with AID. These individuals should be known as flexible and accommodating professionals who have demonstrated

sensitivity to regional bureau and mission needs and opportunities.

- h. Specific members of the research team, some of whom are at the senior level, should be explicitly committed to residing abroad for an extended period to oversee and/or conduct long-term technical assistance and applied research missions.
- i. Senior team members should not become so involved in technical assistance that they are unable to use their experience and intellect to contribute significantly to the knowledge-building process.
- j. AID and the cooperating institutions should become more explicit about and comfortable with the bargains that can be struck to achieve the goals of both parties. The cooperators must appreciate AID's concern for specific advice about specific problems in specific locations; AID must provide some flexibility to the cooperators to identify broader issues of concern and to set reasonable time frames for high quality research.
- k. AID and cooperating institutions should, through a process of consultation, establish a concrete and appropriate time frame for meeting the multiple objectives of a cooperative agreement. This process should establish what tangible results can be expected within one year of start-up, two years of start-up, etc. Annual discussions should review progress and involve an evaluation of the methodological and programmatic quality of work undertaken.
- l. We have noted no obvious relationship between the topic of the cooperative agreement and the contributions made to knowledge-building or programmatic influence. Some have suggested, however, that it is important to the success of a cooperative agreement from the perspective of AID for the topic to be fairly narrow and well-focused. The issue of appropriate topics and their scope should be more fully discussed.

5. Improving the Utility of Cooperative Agreements:  
Recommendations from Three Specialized Reports

a. Improving Research Methodology

After a review of the research methodologies employed in the publications of cooperative agreements, Donald P. Warwick recommends that future cooperative agreements give more explicit attention to methodological standards. Towards this end, he suggests the following guidelines:

1. Normative Judgments and Conceptual Clarity:

- a) Researchers should draw a clear distinction between the normative and empirical components of their studies. When a study introduces normative judgments or engages in advocacy, moral biases should be explicitly stated and not be merged into or identified with empirical findings.
- b) The concepts used in social scientific research should not rely on persuasive definition or otherwise prejudice matters that are properly the subject of empirical investigation. When a value-laden concept is used, explicit attention should be given to the moral judgments behind it.
- c) Studies should avoid highly aggregated or diffuse concepts whose operational meaning is subject to widely varying definition and interpretation. Concepts should be as precise as possible and should be explicitly defined.
- d) When a key concept contains a number of separate elements or dimensions, they should be spelled out and defined. In studies involving quantitative measurement, conceptual definitions should be linked directly to corresponding empirical indicators.

2) General Issues for Methodology and Research Design:

- a) Research reports should provide specific information about both data sources and the

quality of those sources. In studies based on sample surveys, information should be reported on sample selection, questionnaires or interview schedules, coding and analysis procedures, and, where relevant, the estimates of errors. For research drawing on other data sources, detail should be supplied on the type and quality of the information used and on analytic procedures. In general, reports should have enough information about methodology and data sources to permit an independent assessment of quality.

- b) Writers should openly and explicitly state the purpose or purposes of their studies. Such disclosure is particularly important when the study involves some form of advocacy and/or when readers may otherwise misjudge its true intent.
- c) When a study is pursuing two or more incompatible objectives, the report should comment on how those incompatibilities have been handled. Such commentary is particularly necessary when sacrifices in methodological quality have been made or are being recommended in the interests of speed or policy relevance.

### 3. Specific Issues for Methodology and Research Design:

- a) The choice of methodology and research design should be guided above all by the objectives and specific requirements of the study in question. Both the strategies for gathering information and the degree of rigor in study design should be adopted to the particular research questions involved. When the study will attempt to assign causality, the design chosen should be powerful enough to permit valid causal attribution.
- b) When possible and appropriate, the research design should include the collection of data by two or more complementary methodologies. Within such designs special efforts should be made to obtain independent assessments of key phenomena through different data sources.

- c) Whatever the specific data source, the report should address the question of sampling. It should first indicate how the sample was chosen and the rationale for that procedure. Second, the report should discuss how the sampling strategy used affects the generalizability of the findings to the larger body about which conclusions will be drawn.
- d) To the extent possible, sample surveys should follow accepted norms of probability sampling at all stages. When practical obstacles make it difficult to apply these norms, the researchers should consult a sampling specialist about ways of working in that situation. In studies aiming to estimate population characteristics or to compare different groups from the same population, quota sampling should be avoided.
- e) The procedures used in selecting cases should yield a fair representation of the broader universe about which generalizations will be made. While it is not necessary to use probability sampling, the selection criteria should ensure reasonable coverage of diversity and avoid biased choice.
- f) In writing research reports involving cases, authors should indicate their selection procedures and the likely bias produced by these procedures.
- g) In studies aiming to generalize to a larger domain, the use of single case studies should be avoided when possible. Single cases are acceptable when the purpose of the research is to explain those cases or to develop hypotheses, but not when it seeks to generalize to a range of cases.
- h) The source(s) of information used in constructing case studies should be clearly identified and the quality of those sources should be discussed. If there are ambiguities in the data or disagreements on significant facts and interpretations, these should be noted in the report.

- i) The portrayal given in case studies should fairly reflect the situation being described. Case descriptions should not be slanted toward the views advocated by the researchers nor should they omit significant details.
- j) When judgments are made about the success or failure of a given case, the specific criteria of success and failure should be mentioned. If multiple criteria are involved, the case should present information on all relevant criteria rather than offer a single aggregated assessment of success or failure.
- k) When the analysis relies heavily on a key criterion variable, such as a major indicator of success, failure, or effectiveness, the study should develop alternate measures based on different methods. Systematic discussion should also be provided of differences in the patterns of association between predictor variables and the different measures of the criterion variable.
- l) Generalizations made on the basis of research should not extend beyond the limits of the information available and should respect the nuances and complexities of that information. Particular care should be taken in generalizing about a population from data based on a sample.
- m) The characterizations made of individuals, groups, programs, organizations, situations, or events should be faithful to the variations and complexities involved. Special efforts should be made to present a differentiated picture of complex phenomena and to avoid condensations or simplifications that may be misleading.
- n) Authors should normally present a balanced picture of the evidence available on a given point. Evidence in support of the researcher's favored hypotheses should not be overstated nor should negative evidence be omitted.
- o) If a writer chooses to engage in advocacy, that intention should be openly disclosed so that readers may make their own assessments of its effects. Even when the author's purpose is advocacy, the state of the evidence should not be misrepresented in the interests of persuasion.



- p) When research reports include policy recommendations they should lay out the empirical bases for those recommendations. The discussion should indicate how, specifically, the research findings relate to the recommendations.
- q) Authors should clearly indicate the normative bases of their recommendations. They should be explicit about the values and principles behind the recommendations and offer some commentary on why those values and principles apply to the situation in question.

#### **b. Increasing Research Utility**

John D. Montgomery reviewed the findings of the nine cooperative agreements to identify: (1) whether they contributed to better policy; and (2) how their policy relevance might be improved. After reviewing 32 major publications, he concluded that: (1) most of the reports offer advice that seems appropriate to government decision makers; (2) the decision makers most frequently addressed are mission directors and project managers; (3) the policy findings are usually clearly stated, although often hidden in long reports; (4) it is not easy to determine the degree of empirical support that underlies policy recommendations; (5) there is no necessary correlation—even a negative one—between good research and policy relevant research; (6) there is no strong correlation between the nature of the research institution and the quality of the research or its policy relevance; (7) combining research with consultancy assignments is probably beneficial to both activities; and (8) donors are more likely to succeed in conveying the results of research to various levels of users than the researchers themselves.

Based on these conclusions, Montgomery makes several recommendations:

- 1) Donors desiring to develop a research base for policy should incorporate a decision making framework (including the identification of decision makers whom the information is supposed to help) on the research assignment.
- 2) Research for policy purposes should not be assigned on the basis of an anticipated preference for action on the part of consulting firms or for theory on the part of universities. Other considerations like capacity to retain and deploy high quality personnel and the clarity of the assigned task or the monitoring of performance are more likely to produce desirable policy outputs.
- 3) If a donor wishes to improve the policy relevance of research in its programs, an interim check using some form of "decision overlay" (see Section III-B) as both a test and a learning device can serve as a tool of management for that purpose. Donors should always apply a "decision overlay" with a parallel "methodological overlay" so that the researchers are aware that standards of both utility and soundness are to be applied to their work.
- 4) Donors should be patient with institutions embarking on research assignments whose relevance to operations is unclear, or in fields that have not yet attained a level of consensus necessary for accumulative data gathering and interpretation.
- 5) Donors should not try to separate the research from the consulting functions contractors can provide; they probably should not always insist upon merging them in the same institution, if the resources in personnel, library facilities, and management are not available.
- 6) Donors should prefer achieving a balance between research and consulting functions within the same contracting organizations if they are receptive to that combination of efforts, but should not expect good performance in both of them if the personnel involved are reluctant or apathetic about either the research or the consulting service.

- 7) Donors need separate units to disseminate research findings and current doctrine about subjects where the best judgment is approaching consensus as to policy outcomes. Such reports need not be presented normatively nor as official government statements, but should be considered a service to decision makers at all four levels. An alternative mechanism would be for donors to establish an independent clearing house for disseminating the policy implications of sponsored research.

c. Promoting Research Contributions to Knowledge About Households and Women

Pauline E. Peters reviewed the publications of the cooperative agreements to assess how they address the roles of households and women in the process of rural development. She finds that research on women is typically treated as a special field of inquiry rather than as part of research on broad issues that affect women or are affected by them. As a result, valuable insights into important aspects of rural development are lost. She also encountered a tendency for advocacy to replace analysis when reports turn to the subject of women. In addition, it is not clear that those referring to women are aware of the increasing sophistication of present knowledge about household organization and gender. Finally, there is much need for greater conceptual clarity about the term "household," for the reports reviewed often have a tendency to use the term household interchangeably with the terms farmer or farm.

The three central recommendations flowing from these conclusions are that: (1) gender should be treated like any other variable, such as age, class, or occupation; (2) households as units of analysis should be more carefully defined; and (3) greater

analytical attention should be given to intra- and inter-household linkages.

More specifically, Peters suggests the following guidelines:

- 1) Greater conceptual clarity is required to improve research methodology and to enable more precise conclusions and actionable findings to be drawn.
- 2) Particular care has to be exercised in generalizing from one case or data from one area, from a single or an undifferentiated category of population, and from inappropriate data bases.
- 3) A more rigorous distinction is needed between reporting research findings on gender-related issues and making a statement of advocacy. Care should be taken that the pressures of advocacy not lead to highly selective presentation of evidence such that only the negative effects of change are reported, neglecting the positive, neutral, or contradictory effects.
- 4) Imprecise specification of the units of analysis undermines the force of many conclusions presented. Particular care is required with "household" because it is used as a "lay" as well as a "technical" term.
- 5) An assumption that the household is necessarily a site of joint preferences or utilities has been seriously questioned in recent research; hence, the issue should be carefully treated in future research.
- 6) Farming systems research and other research frameworks for investigating the production, consumption, and investment activities of rural populations need to integrate information into their analytical and methodological techniques on multiple decision makers within households and on inter-household networks.
- 7) Particular productive regimes where multiple strategies of livelihood are the rule have led to insightful discussion of the permeability of household boundaries. Now, this type of understanding needs to be systematized and applied to other cases.
- 8) While poor or needy women and householders must continue to be foci of research and action, researchers and practitioners should not equate women with disadvantage. Different categories of women at

all socio-economic levels need investigating if a fuller analysis of development is to be achieved.

- 9) Similarly, the focus of any inquiry into the affects of rural development on different categories of women or households must be expanded beyond production.
- 10) Research on rural development will benefit from a more precise distinction between gender as a key principle of differentiation and women as social actors within specific social units.
- 11) Emphasis should be placed on the incorporation of gender and of intra- and inter-household relations into analysis. Both the integration of gender as a significant variable and the more precise assessment of the key units of production, consumption, and investment are best seen not as separate "topics" in the analysis of rural development processes and policy, but as essential components in an integrated, structural framework of analysis.

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## SECTION II

### REVIEW OF NINE COOPERATIVE AGREEMENTS

Section II contains reviews of the research output of nine cooperative agreements. The analysis undertaken in each of these reviews provides the basis for the assessment and recommendations presented in Section I. These reviews were written by four members of the HIID professional staff, with each reviewer having expertise in the area of the cooperative agreement reviewed. Two other members of the HIID professional staff assessed the research output and advised the reviewers on matters relating to issues of methodology and perspectives on household and gender. Their insights are reflected in the reports written by the four reviewers. The cooperative agreements reviewed and the author of each report are as follows:

- A. Rural Development Participation  
Cornell University  
Merilee S. Grindle
- B. Organization and Administration of Integrated Rural Development  
Development Alternatives, Inc.  
John M. Cohen
- C. Alternative Rural Development Strategies  
Michigan State University  
Merilee S. Grindle
- D. Rural Non-Farm Employment  
Michigan State University  
John W. Thomas

- E. Performance Management  
National Association of Schools of Public Affairs and  
Administration and Development Project Management Center, U.S.  
Department of Agriculture  
John W. Thomas
- F. Rural Financial Markets  
Ohio State University  
Malcolm F. McPherson
- G. Managing Decentralization  
University of California, Berkeley  
Merilee S. Grindle
- H. Land Tenure  
University of Wisconsin  
John M. Cohen
- I. Regional Planning and Area Development  
University of Wisconsin  
Merilee S. Grindle

Each of the reviews seeks to provide: (1) an overview of the cooperative agreement; (2) a summary of the theoretical framework and central concepts; (3) a review of the research findings and their programmatic implications; (4) an assessment of the research methodology; and (5) a set of suggestions for filling remaining knowledge gaps in the area, if any. Most reviews address these topics in this order; however, because of some variation in the objectives of each cooperative agreement, there will be some variability in the issues addressed.

It should be stressed that these reviews do not cover the full set of reports produced by each project; only major studies, publications, and reports were reviewed. Since an important part of the work product of each project involved mission-related reports, the following reviews are not intended to be assessments of the cooperative



agreements. They are, rather, reviews of the social science-related research output of the nine projects. Moreover, it should be noted that the publication series of some projects include studies completed prior to the cooperative agreement. Finally, the publication series also include findings of related research not funded by AID or based on mission consulting. As a result, it should be noted that the bibliographies of works consulted at the end of each review cite only part of each project's documented work product.

REVIEW ARURAL DEVELOPMENT PARTICIPATION

Rural Development Committee  
Center for International Studies  
Cornell University

1. Overview of the Cooperative Agreement<sup>1</sup>

The project on rural development participation at Cornell University was the first effort to use the cooperative agreement mechanism to bring together academic research, field experimentation, and technical assistance for AID-funded projects. The agreement was established in order to increase donor ability to respond to the Foreign Assistance Act of 1973 that mandated participatory involvement of beneficiaries in development projects funded through bilateral assistance programs. In the early and mid-1970s, efforts to increase the participation of low-income groups in the planning, implementation, and benefits of development projects in third world countries had been frustrating to officials attempting to carry out the congressional mandate, largely because it was not clear what constituted "participation" nor how it should be encouraged. As a result of this concern, the Cornell project was established in 1977,

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<sup>1</sup>One of the co-authors of this report, John M. Cohen, was associated with the Cornell project and participated in drafting some of its publications. In order to ensure a fair review of Cornell's written record in a manner consistent with the other eight reviews, Cohen was excluded from any discussion or review of this section of the report until it was in final draft and used to prepare the recommendations in Section I.

after two years in which researchers at its Rural Development Committee had worked with AID to help identify and resolve major conceptual and practical problems in achieving participation in rural development projects. As defined by researchers, the task undertaken at Cornell was to "[a]nalyze the operative elements of 'participation' in rural development efforts, identify experience contributing to a better understanding of participation in rural development; [and] disseminate such knowledge to [promote] participatory rural development" (Rural Development Committee, 1982:57).

The Cornell project has produced a large research output.<sup>2</sup> In addition, the Rural Development Committee has published a long list of related papers funded by other AID projects or other sources. The research output from the participation project includes a series of state-of-the-art papers, monographs resulting from extended field research in countries such as Botswana, Jamaica, the Philippines, Nepal, Tanzania, Sri Lanka, and India, among others, as well as research monographs of a broadly comparative nature. This work is of high quality. It is sophisticated and analytic and it consistently recognizes the variability of social and political interactions while maintaining a firm awareness of more general patterns and lessons that can be identified through research. It is not too much to argue that researchers at Cornell have defined the topic of rural development participation and have established the Rural Development Committee as

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<sup>2</sup>The research output is listed in Rural Development Committee (1982):79-82).

a leading center for research on participation in rural development projects. Throughout the discussions of local organizations, local government, resource management issues, and other participatory activities in the research output, there is a significant effort to use empirical research to generate middle-range propositions about the requisites, nuances, and impact of participation on rural development. It is difficult in the course of a short paper of this nature to convey the breadth and depth of the research output; in the pages to follow, only the broadest generalizations and most important insights that it has produced will be considered. These generalizations and insights are well-articulated in the state-of-the-art papers (see Cohen and Uphoff, 1977; Esman and Uphoff, 1982: Rural Development Committee, 1982).

## 2. Assumptions

The most basic assumption underlying the Cornell project is explicit in much of the research output: participation by beneficiaries in rural development projects can measurably improve their impact on indicators of social and economic development in rural communities. Participation is assumed to be central and instrumental to project success. It is important to stress that this assumption has been consistently affirmed in the Cornell research output, as discussed in later pages (Cohen and Uphoff, 1977:3-4; Rural Development Committee, 1982; Esman and Uphoff, 1982; Uphoff, Cohen, and Goldsmith, 1981:281; Whyte, 1981). Unlike the research effort on decentralization at Berkeley, therefore, the Cornell project does not take a highly

critical approach to the central research concept; rather, it focuses on "devising and applying means for making development more participatory" (Rural Development Committee, 1982:vi, emphasis in original). In short, participation is broadly regarded as an essential "key" to development (Whyte, 1981).

Other operational perspectives underlie the research effort that reflect this positive approach to the benefits to be gained through participation:

- a. It is explicitly affirmed that rural development can be achieved without major structural transformations in society if project planning and implementation processes incorporate participatory perspectives.
- b. It is generally assumed that participation at the level of rural development projects is sufficient to bring increased benefits to local communities; broader forms of participation, often considered more overtly political (mass mobilization, voting, peasant movements, etc.), are considered beyond the scope of concern.
- c. It is explicitly acknowledged that rural communities are not homogeneous and that particular groups of rural inhabitants--the poorest, the most remote, women, ethnic and religious minorities--are frequently extremely vulnerable to domination and exploitation by more elite groups at the local, regional, or national level.
- d. It is explicitly assumed that mutual benefits can be achieved through participation, such that both governments and rural inhabitants will have an interest in pursuing participatory efforts, even though their reasons for doing so may differ. Thus, with some exceptions (see Brown, 1982; Roe and Fortmann, 1982; Fortmann, 1980) the approach favored by the Cornell project is not centered on expectations of conflict so much as on seeking out common ground among potentially divergent interests. This perspective is buttressed by selection of research sites that provide a reasonably supportive environment for participatory projects.

- e. The positive consequences of participatory strategies are also generally assumed to span project types. That is, whether considering a water resource management project, an integrated rural development effort, a research and extension service, or an agricultural production effort, all are generally expected to be more effective if they are participatory.
- f. There is an implicit assumption in the research output that local inhabitants can increase their influence over national policies and politics through effective participation at the project level. Project participation is therefore considered to be an effective "school" for engendering broader forms of participation.

### 3. Conceptualization

A major contribution of the Cornell project has been to operationalize the concept of participation in development projects. Participation, it is argued, is not a thing that can be specifically defined but is rather a "general category for related but often quite different things, usually activities but including sometimes material and attitudinal contributions" (Cohen and Uphoff, 1979:5). Four kinds of participation are identified: (1) participation in making decisions about the intent and form of projects; (2) participation in implementation processes through the mobilization of resources or cooperative activity; (3) participation on terms of receiving benefits from projects; and (4) participation in the evaluation of projects (Cohen and Uphoff, 1977:7-10).

This broad conceptualization has led researchers at Cornell to a view of participation that stresses its dimensions. These dimensions respond to three central questions about participation:

(1) what kind of participation? (2) who participates? and (3) how does participation occur? Dimensions of the concept that correspond to the question of who participates specify local residents, local leaders, government personnel, and occasionally foreign personnel (Cohen and Uphoff, 1977:10-15). Considerable effort is taken to identify subgroups of local residents who tend to be marginalized from rural development projects and participation in them (see Uphoff, Cohen, and Goldsmith, 1979:91-159). In response to the question of how rural development participation occurs, the conceptual framework focuses attention on identifying where the initiative for participation comes from, what inducements are provided for participation, what the structures and channels for participating are, what the duration and scope of the participatory effort are, and what degree of empowerment results from participation (Cohen and Uphoff, 1977:15-17). This analytical framework, it is argued, allows a researcher to identify and evaluate the content of participatory efforts in rural development projects.

It is further specified that participatory efforts are pursued within a particular context that affects the success or failure of the effort. Thus, the analytic framework for studying participation includes identification of factors inherent in the participatory activity (who? what? how?) as well as contextual (environmental) factors affecting it. Under the latter category are a series of broad variables that, it is argued, have to be considered. These are:

(1) the physical and biological setting of the participatory effort,

and the (2) economic, (3) political, (4) social, (5) cultural, and (6) historical factors that influence responses to the what, who, and how questions (Cohen and Uphoff, 1977:18-19). In addition to the participatory context, aspects of the specific project being pursued are specified as having an impact on the effectiveness of participation within that project. In this regard, the analytic framework directs attention toward the content of the project in terms of: (1) its requirements for technology and resources, (2) the tangibility, probability, immediacy, and distributional structure of its benefits; (3) the program linkages and flexibility; and (4) its administrative accessibility and coverage (Cohen and Uphoff, 1977:19-21).

This framework implies a very broad definition of participation and identifies a wide range of factors that impinge upon the what, who, and how questions that are specified. The concept is made more manageable in terms of research, however, because it is consistently linked to the project level. That is, research on rural development participation begins and concludes with a focus on participation in specific projects and the contribution of participation to the success or failure of the particular project in question. Thus, it is clear that the objective of participation for the Cornell research is to improve the impact (or success) of specific projects. Only incidentally is there concern for broader issues of effective local power to influence the trajectory of national development policies or politics.



#### 4. Major Hypotheses and Findings

The major hypotheses in the research output are closely related to the assumptions underlying the project's approach to participation and the analytic framework that it specifies. In a report written before the cooperative agreement was initiated, Uphoff and Esman (1974) indicate that their findings from research on sixteen Asian countries reveal "a strong empirical basis for concluding that local organization is a necessary if not sufficient condition for accelerated rural development, especially development which emphasizes improvement in the productivity and welfare of the majority of rural people" (quoted in Esman and Uphoff, 1982:9). A study of participation in public works programs in Nepal confirms that "the more vocal a rural community becomes in demanding services, the steadier becomes the supply of services" (Pradhan, n.d.:86). Similarly, "The more rural people have a chance to make decisions by themselves, the greater becomes their willingness to contribute materials and human resources" (Pradhan, n.d.:89-90). In summarizing prior research, Cohen and Uphoff (1977:3-4) indicate that "national success, measured in terms of both agricultural productivity and social welfare measures, is strongly correlated with effective systems of participatory local organizations linking rural communities to national centers of decision making and implementation."

A recent state-of-the-art paper includes an extensive comparative and cross-sectional analysis of 150 case studies of local organizations (cooperatives, clubs, and interest associations) and a

broad sampling of relevant literature is explicit in testing a number of the linkages hypothesized in the analytic framework (Esman and Uphoff, 1982). In this study of the institutional dimension of participation, it is argued that local organizations can contribute to rural development by facilitating the delivery of services, by mobilizing resources and activities, and by enhancing the flow of information and demands between officials and local inhabitants, confirming the conclusions of earlier work by Uphoff and Esman (1974). Esman and Uphoff (1982) identify a series of characteristics that are frequently associated with the success of local organization. Thus, "relatively small, informal, base-level organizations, linked vertically and horizontally with other [local organizations] offer greater promise since they combine the benefits of solidarity and of scale" (Esman and Uphoff, 1982:ix). In addition, the study indicates that participatory and egalitarian values and roles are important characteristics of successful local organizations. Informality, consensual decision making modes, voluntary membership, and agreed-upon obligations are also associated with the success of local organizations (Esman and Uphoff, 1982:ix). The local organizations evaluated tend to be vulnerable to failure in performance because of variation in who participates in them or who dominates them (local elites, government officials, types of leadership or membership) or because of their internal organizational problems (internal cleavages, lack of appropriate skills, malpractices). The study also confirms the importance of "promoters, organizers, or facilitators in 'catalyst

roles'" and the importance of a "learning process approach" for the performance of local organizations (Esman and Uphoff, 1982:X).

Other findings of the Esman and Uphoff case study analysis are interesting because they suggest that "in general, environmental constraints do not shape [local organization] success or failure... [local organizations] may further development objectives under a wide variety of conditions" (Esman and Uphoff, 1982:ix). Similarly, in a comparative study of participatory development programs in South Asia and the United States, Blair (1981) finds striking parallels between these national efforts in terms of their origins and the short- and longer-term success they achieved. These more recent findings deserve attention because they suggest that issues of site-specificity and contextual environment are possibly less important than is indicated in the conceptual framework developed in 1977.

It should be noted that these findings about the generalizable influence of contextual factors differ from several reports produced by the Cornell project. These tend to confirm that specific contextual conditions greatly aid or impede rural development participation efforts. In a state-of-the-art paper on the feasibility and application of participatory efforts, it is argued that "the more unequal the distribution [of assets], the more difficult it is to have broad participation in decision making and in benefits" (Uphoff, Cohen, and Goldsmith, 1979:29). Similarly, structural impediments to the participation of women in rural development projects are documented by Gellar, Charlick, and Jones, (1980), Whyte (1981), Cohen

and Uphoff (1977), Charlick and Vengroff (1983), and Staudt (1979).

An important set of findings about contextual variables emerges from research on mixed pastoral and agricultural regimes in Africa, notably in Botswana. These findings focus attention on the impact of "seasonality" on the local community and the impact of change on the institutions available to promote rural development participation. Brown (1982:2) concludes, for example, that seasonal migration for pastoral pursuits and other income-generating activity in four villages in Botswana means that "the village boundaries are not the relevant geographic boundaries for people's main social and economic concerns." Where this is the case, village-centered organizations to promote rural development will not be effective and other kinds of organizations need to be identified that can be more effective. Similarly, he concludes that in the village studies, traditional and modern institutions coexist and are continually undergoing change. This becomes a significant problem for encouraging participation when traditional organizations such as those that center around the village headman have weakened, but modern organizations centered around official or interest-related activities are not yet strong enough to replace them. Roe and Fortmann (1982) also indicate that seasonality affects the rural water sectors and results in the difficulty of locating central economic and social interests within the villages. They call for efforts to create "communities out of localities" (Roe and Fortmann, 1982:188). They also argue that the concept of the household must be carefully used in situations in which

the household may be dispersed geographically at various seasons or in pursuit of divergent functions. Thus, just as the community is found to have permeable boundaries, so is the household.

The findings indicated in these studies confirm the hypothesized relationships indicated in the conceptual framework while at the same time differing from the generalized finding of the comparative study of local organizations done by Esman and Uphoff (1982). From a social scientific perspective, among the most interesting studies by the Cornell researchers are those that delve into the structural impediments to participation in third world countries. These reports, while perhaps less immediately useful to the field practitioner, address basic theoretical questions more directly than most of the research output. More generally, the dissonance in research findings suggests that considerable investigation should be directed toward understanding the impact of environmental or contextual variables on participatory efforts at the project level and the conditions under which they do or do not affect the performance or outcomes of participatory strategies.

Similarly important from a knowledge-building perspective are the reports that focus on the relationship between conflict and participation. In a study of participatory socialism in Tanzania, Fortmann argues that it has not been effectively pursued in that country, largely because of the conflicting interests of bureaucrats and peasants. These interests differ, she indicates, because of differing but equally legitimate and understandable goals. She

concludes that "much of the enthusiasm for participation is based on the implicit assumption either that one side has the answer or that the various sides can decide on a mutually satisfactory solution. But if the sides differ significantly, there may be no mutually satisfactory solution" (Fortmann, 1980:119). Such a perspective could call into question the assumption found in much of the research output that there is mutual benefit to be gained by local inhabitants and governments in pursuing participatory strategies, even if their interest differ. Fortmann indicates that peasants, when faced with the imperative to participate in development projects they do not consider to be beneficial, can simply withdraw, refusing to participate, and therefore acquire the power to bring about the failure of the project (1980:118; see also Roe and Fortmann, 1982). Similarly, Goldsmith and Blustain (1980) indicate that participation in an integrated rural development project in Jamaica was limited to those aspects of the project that could only be acquired through organizational membership. Most of the benefits that members perceived to flow from the project were too limited and the dominance of bureaucratic agencies too great to induce more active participation (see also Blustain, 1982). The studies alert researchers to the question of why local inhabitants will or will not participate in development projects. Too frequently, perhaps, it is assumed that they are available and eager to participate in a wide variety of projects if only they are given the opportunity to do so.

Findings from other studies are suggestive because they begin to explore types of participatory strategies and to link their potential contributions to program objectives and contextual conditions. Several field studies focus on programs of animation rurale typically found in French-speaking West African countries (see Charlick and Vengoff, 1983; Gellar, Charlick, and Jones, 1980; Charlick, 1983). In their study of this program in Upper Volta, Charlick and Vengoff (1983) identify and evaluate three techniques commonly used to pursue animation rurale--linkage, top-down, and bottom-up strategies. Their findings are important, for they indicate the results that can be expected from utilizing each of the strategies (Charlick and Vengoff, 1983:7-8). Linkage approaches to animation rurale are found to be the most productive in increasing the viability and impact of rural development projects. Top-down approaches are associated with improving general material well-being but are not likely to increase equity or participatory dimensions of rural development. Bottom-up approaches are useful for enhancing participation and viable local organizations, but they are only weakly associated with improvements in material well-being.

Important also in the papers on animation rurale are the indications of what the process can be expected to accomplish in general and the conditions under which such goals can be achieved. Gellar, Charlick, and Jones (1980:168-170) conclude that: (1) the amount of political and economic change that can be anticipated from strategies like animation rurale is limited when it is the state that

promotes them; (2) the success of such strategies is dependent upon national leadership support and development goals; (3) such strategies are frequently useful to governments to ensure project success but are not likely to be popular with them when they encourage broader forms of participation; (4) contextual factors are central to the success or failure of such strategies; (5) animation and similar strategies must be integrated into broad policy and administrative frameworks; and (6) researchers and evaluators should consider animation and similar strategies in terms of their relative success or failure vis-a-vis alternative strategies.

The Cornell researchers also add insight into the role of women in participatory efforts (see especially Staudt, 1979; March and Taqqu, 1982; and Taylor, 1981). Significantly, gender is incorporated as a variable to be studied along with others (age, ethnicity, social class, etc.), and women's organizations are treated as organizations with characteristics that can be analyzed along with other kinds of organizations in Esman and Uphoff (1982). Thus, women and women's organizations are included in a broader universe of households, groups, and organizations whose problems, contributions, and needs can be studied in terms of a participatory perspective. In one case (Gellar, Charlick, and Jones, 1980), women's groups are found to be highly successful in gaining benefits for their members but at the same time, material benefits are achieved by them only to the extent that these, and the behavior that generated them, could be defined as "typically female" activities that offer no threat to what are



traditionally male roles. In general, the research output addresses the issue of gender from a conceptual framework and methodological approach that does not single it out as "special" but treats it as an important variable for research similar to other important variables.

#### 5. Methodology

The attention to conceptual clarity about participation in the Cornell project is a fine example of methodological rigor. This concern with conceptualization: (1) helps to prevent overly aggregated analysis of participation; (2) suggests broad directions for data collection, such as on who is participating in what and how; and (3) provides useful leads for the development of specific empirical indicators.

In addition, the Cornell project demonstrates the utility of combining complementary methodologies and data sources in a single study. An example of such methodological integration is evident in the study by Roe and Fortmann (1982) on the rural water sector in Botswana. They combine a large sample survey of rural households, extensive field observation, and data resulting from interviews and observation of water management groups. In addition, the authors use their own extensive experience in the country, their familiarity with related literature, and informal interviews with a wide variety of relevant persons. Similarly, the approach taken by Gellar, Charlick, and Jones (1990) in studying animation rurale in Senegal systematically utilizes other scholars in the field to summarize their observations on the issues under study. The research by Goldsmith and

Blustain (1980) on integrated rural development in Jamaica also brings together three data sources: participant observation, interviews with informed persons; and a sample survey of farmers.

The project on rural development participation also raises some methodological issues. For example, in some reports (Gellar, Charlick, and Jones, 1980; Uphoff, Cohen, and Goldsmith, 1979; Whyte, 1981) are very clear about the sources and types of information used, while such attention to specificity is weak in others. Thus, Staudt (1979:50) lists a variety of "women's issues," such as special health care needs, less access to education than men, and overt physical abuse toward women. These issues are identified "by locating sex disparities in work, opportunities, and resource control," a source that provides the reader with little basis for judging the frequency or salience of such questions to women in general. Roe and Fortmann (1982) on the other hand, are specific about their data sources when discussing questions of water use in rural areas but vague when analyzing central government perceptions of resource management in communal areas.

A second issue concerns explicit attention to sampling standards in survey research. Several of the Cornell studies make use of sample surveys as a major source of information about rural areas (Roe and Fortmann, 1982; Brown, 1982; Pradhan, n.d.). Roe and Fortmann are explicit in indicating the sampling standards they adopt, while Goldsmith and Blustain (1980) and Pradhan do not provide enough information to permit evaluation of their sampling design.

A third methodological issue involves the selection of cases. The question of sampling arises as well in the choice of cases to illustrate a point or to form the basis for additional analysis. Questions about the representativeness of cases arise in several of the papers in the Cornell project. In a discussion of women's status, Staudt (1979:6-8) chooses examples from four regions: The Andean Highlands, East Africa, West Africa, and China. Would the conclusions reached about women's participation be the same if different examples had been chosen? The danger of choosing cases selectively to make a point is particularly great in papers that advocate actions to ameliorate general problems.

Questions about the adequacy of information about cases are germane to the study of Esman and Uphoff (1982) on local organizations and rural development. For their research, the authors sought 150 reasonably complete case studies of local organizations in developing countries. Each of the cases chosen is coded on 55 variables and then the relationships among the variables are analyzed. The information used for the coding is literature on the organization in question. Although the information available on one case is likely to be much better than that on another, all have to be coded with the same scales on the same set of variables. Although the authors do not discuss the difficulties of this type faced in coding, nor do they indicate the degree of coding reliability (inter-rater consistency), it is safe to assume that there were problems of comparability across organizations. Greater detail on the coding procedures and difficulties would help

interested readers to form judgments about the quality of the resulting data.<sup>3</sup>

There are additional concerns to be addressed about measurement. Questions about the translation of concepts into quantitative measures can be raised about several of the studies, particularly those by Esman and Uphoff (1982), Williams (1981), Roe and Fortmann (1982), Goldsmith and Blustain (1980), and Pradhan (n.d.). Williams (1981) makes a serious effort to develop objective indicators of government performance, but is less cautious than he should be in using aggregate data for this purpose. The difficulty with such data is that the fit between the concept, such as "capability," and the aggregate measures, such as revenue collected, is often approximate or poor. Esman and Uphoff (1982) also develop systematic procedures for coding qualitative case materials, but need to be more explicit about the problems involved in applying such procedures. In evaluating the validity of the measures obtained, it would be helpful to have a coefficient of coding reliability and a more general discussion of the strengths and weaknesses in these particular data.

#### 6. Programmatic Implications

Ultimately, the Cornell project on rural development may be less important for the specific programmatic guidelines it has produced than for its positive role in familiarizing field practi-

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<sup>3</sup>This detail is included in the book manuscript that presents the local organization's study in full.

tioners with the importance of participation and advocating it as a central aspect of any rural development project. In doing so, the Cornell researchers have given the elusive concept of participation a concrete and relevant definition for field practitioners. In this regard, the work of the project took an "apple pie" concept that all could applaud but few were able to apply and linked it to the organizational component of specific projects. In doing so, it is isolated from the concept of political participation (see Uphoff, Cohen, and Goldsmith, 1979:279-284) and made a "necessary but not sufficient condition for project success." Participation therefore has become an activity, resource, or attitude to get things done more effectively at the project level.

In addition, most of the studies done present a series of recommendations for increasing participation. Some of the more generalizable guidelines are broadly relevant to field practitioners. Practitioners are alerted to the fact that participation is not a separate activity but must be an integral aspect of all specific planning, implementation, and evaluation activities in rural development projects (Uphoff, Cohen, and Goldsmith, 1979:28). Similarly, they are advised that participation is most effectively achieved through local organizations (Uphoff, Cohen, and Goldsmith, 1979:28). Field practitioners are also advised that the goal of participation is to strengthen the linkages between local communities and national or regional centers (Uphoff, Cohen, and Goldsmith, 1979:29-30). More specifically, they are told that local organizations should:

(1) be organized around a concrete goal; (2) begin with a simple task; (3) be based on present skill levels of members; and (4) be focused on tasks that can only be done through cooperation. Similarly, the findings of Esman and Uphoff (1982) provide programmatic guidelines in alerting practitioners to the importance of small, informal, and task-oriented groups in stimulating local participation. Several studies indicate the utility of the role of promoter or animateur.

#### 7. Gaps in Knowledge

The Cornell project on rural development participation can be credited with a major contribution to the field in terms of defining the concept and specifying centrally important linkages among the variables that facilitate or constrain participation at the project level. The research output has produced a usable and widely adopted conceptual framework and while this perspective has not generated macro-level theories related to cause and effect relationships, it has made participation a more accessible idea to field practitioners.

To generate additional knowledge about participation, researchers might take a more critical approach to the utility of participation in various rural development activities. An area that remains to be explored is the possibility that participation contributes differentially to various types of projects. Research could usefully explore whether some types of projects will inevitably fail if they are not designed and implemented in a participatory mode and whether other types of projects can be successful even without extensive participation. In a related fashion, it would be useful to

undertake greater efforts to identify contexts in which participatory efforts will be extremely difficult if not impossible to pursue and environments which are facilitating of such efforts. This would encourage more research into one of the important unanswered questions broached by the Cornell research: how important are contextual variables to the success or failure of participatory efforts?

More research should be directed toward the question of why local inhabitants might or might not be motivated to participate in rural development projects. Such research would be important in responding to the findings of Fortman (1980), Brown (1982), and others that conflict, not mutual benefit, is often perceived in the relationship between local inhabitants and government officials in charge of implementing development projects. This research could usefully be linked to the issues raised in the interesting theoretical literature on public goods, public choice, and collective action. Such a focus could provide important insights into the kind of activities that are most likely to elicit participatory responses from local inhabitants. Similarly, this research should ask more explicitly what participation requires from participants in terms of their time, commitment, skills, risk-taking resources, and seasonal availability. This kind of focus would go far in correcting easy assumptions about the evident benefits of participation for potential participants and therefore their ready availability to become involved in rural development projects.

Future research on rural development participation should continue the important focus on the problems involved in encouraging

participation among important subgroups or rural inhabitants--the landless, women, marginal farmers, pariah groups, etc. This research would necessarily highlight the importance of conflict in efforts to encourage participation and might generate new insights into the relative contributions of confrontation or consensus, homogeneous or heterogeneous membership, local elite involvement, and the task orientation of different forms of organization. The large number of professionals and graduate students who have participated in the Cornell project will doubtless ensure that these important questions receive attention in the future.

Perhaps the most evident task that remains to be addressed is explicitly operational. Substantial research energies should be directed toward specific and applied "how to" questions. While the Cornell project has made important contributions to advocating participatory modes and demonstrating their utility, it has not offered field practitioners the kind of specific methodologies that are needed to initiate, support, or maintain specific participatory organizations in specific development projects. This is clearly a needed step to increase the impact of the work that has been done at Cornell and offers the possibility for generating and refining important theories about collective decision making, incentives for participation, leader-follower interactions, learning processes, and conflict. Thus, focusing explicitly on the "how to" question should enable future researchers to utilize the conceptual framework developed by Cornell in linking theoretical aspects of social and political interactions at



the group and individual level with concrete methodologies that could be applied by field practitioners to specific projects in specific contexts.

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REVIEW BORGANIZATION AND ADMINISTRATION OF INTEGRATED RURAL DEVELOPMENT

Development Alternatives, Inc.  
Research Triangle Institute

1. Overview of the Cooperative Agreement

In September 1978, Development Alternatives, Inc. (DAI) and Research Triangle Institute (RTI) entered into a four-year agreement with AID to assist donor missions and host governments in the design and implementation of integrated rural development (IRD) programs and projects. This was not a cooperative agreement for several reasons, the most important of which was that cooperative agreements cannot be entered into with private firms. Still, the activity was conducted in a manner similar to other cooperative agreements and is reviewed as such.

In this regard, it should be noted that the contract between AID and DAI/RTI was quite specific as to what was to be produced. It seems likely that the high productivity of the project in comparison to some of the university-based cooperative agreements discussed in other annexes may be due to such specificity, a finding that has relevance to the design of future cooperative agreements.

Focusing on large-scale rural development projects, this activity had two major objectives: (1) to assist the field staff of host governments and donor agencies in dealing with organizational and administrative problems that arise in major rural development

projects; and (2) to undertake knowledge-building comparative analysis of organizational arrangements and managerial actions that have positively or negatively influenced the success of such projects. The total financing for the project, including mission add-ons, was approximately \$3.8 million.

As of 1982, members of the project's interdisciplinary core staff had provided technical assistance to 23 projects in 18 countries. Published field reports have been released on projects in Botswana, Cameroon, Colombia, Egypt, Ecuador, Honduras, Indonesia, Jamaica, Kenya, Liberia, Nepal, Panama, Philippines, Sudan, and Thailand. Field work has included designing management components for projects, assisting to improve management performance in on-going projects, and evaluating project strategies. Additional data has come from other AID funded DAI rural development projects in Indonesia, Sudan, Tanzania, and Zaire. Drawing on the experience gathered through these field activities and building on a wide-ranging review of the literature on IRD and project design and implementation, the core staff have produced several major analytical papers on topics related to the cooperative agreement.

## 2. Central Assumptions

The major assumptions underlying the knowledge-building efforts of the cooperative agreement are:

- a. The main objective of development is to promote broad-based welfare, well-being, and empowerment for rural people;
- b. If properly designed and implemented, IRD projects can promote such development;

- c. Management problems generally result from poor project design work but even where proper design is achieved it is not a sufficient condition, good management practices being essential;
- d. The key constraints hampering effective design and implementation are uncertainty and complexity in the task environment, dilemmas inherent in organizational design, and a limited number of critical administrative problems;
- e. A set of guidelines can be forged out of accumulated rural development experience and the wider social science literature to address effectively these key constraints;
- f. These guidelines need not be grounded in solid empirical evidence if their applicability can be logically established, their advice squared with the experience of seasoned observers, and their content generally phrased;
- g. Actual use of proposed guidelines must be tailored to context-specific conditions which are too diverse to be addressed in analytical papers;
- h. There are no generally applicable rules about the organizational location or related linkages for IRD projects; organizational location and linkages suitable for one stage of project implementation are not necessarily appropriate for other stages;
- i. Orderly sequencing of IRD project activities is the key to determining which organizational location and linkage arrangement is appropriate for dealing with the need for changing organizational and administrative approaches over the history of the project;
- j. Presentation of guidelines for improving organization and administration of IRD projects must be made in a format that is attractive and useful to busy and committed development practitioners.

Several of these assumptions are also findings of the research undertaken by DAI/RTI.

In the early stages of the project, the core staff generally assumed that it was possible to design and implement successful IRD projects without having to affect major structural change in the task environment. However, by the end of the research period, at least some members of the DAI/RTI team clearly concluded that without structural change effective development would not occur (Honadle, 1981). Finally, core staff assumed that researchers could draw on both structural and cognitive traditions in the social science literature without concern for the theoretical integrity of the analysis.

### 3. Theoretical Framework and Central Concepts

While linked to the fields of organizational behavior and public administration, the theoretical framework for the cooperative agreement is not expressly revealed in the project's publications. This hampers the utility of these publications to those interested in knowledge-building. At best, one discerns an eclectic use of a wide range of development concepts organized around a broad definition of IRD and a logically formulated process-oriented, problem-focused model.

Skirting the conceptual debates that plague the literature, the DAI/RTI core staff adopted a sweeping definition for IRD: "...the process of combining multiple development services into a coherent effort to improve the well-being of rural populations" (Honadle, et al., 1980:4). From this perspective they formulated a coherent and practical framework that eschewed academic debates and focused on



"implementation as the conversion of resources into goods and services, the use of those goods and services by target groups, and improved welfare resulting from that usage" (Honadle, et al., 1980:32). A broadly descriptive state-of-the-art paper was drafted on the basis of this definition (Honadle, et al., 1980). Entitled Integrated Rural Development: Making It Work?, the paper sets forth a broad conceptual framework that allows DAI/RTI core staff to organize the wide ranging analytical and empirical literature on IRD. While simple and not directly grounded on macro-level social science theory, the framework serves its purpose well, for it allows DAI/RTI to use publications focused on integrated rural development projects to formulate guidelines useful for designing, implementing, and evaluating large and complex rural development projects.

#### 4. Research Findings and Programmatic Implications

The DAI/RTI cooperative agreement produced a broad set of publications. Eight journal articles, two book chapters, and two book-length manuscripts broadly advertise the project and its in-house publications. These are published in a consistent format as field reports, research notes, working papers, and state-of-the-art papers. In addition, a number of miscellaneous papers are available through DAI.

A review of these publications illustrates how broadly DAI/RTI and AID view the focus of the cooperative agreement. Topics range from African land law and decentralization issues (Honadle, 1981) to supervising agricultural extension and rapid rural reconnaissance for

development administration (Honadle, 1979). The research output suggests that AID viewed the focus of the undertaking as being much broader than that implied by the title of the project and the core staff extended their interests beyond the narrower limitation of IRD organization and administration. Because of the broad view taken by the contracting parties, it was not long before the project came to see its focus as being on the organization and administration of all large projects, and not just on IRD projects. Discussions with the participants in other projects indicated that this scope created problems for other cooperative agreements in the AID rural development portfolio, for the Berkeley project had difficulty defining itself in the light of the DAI/RTI project's scope and Cornell had steady jurisdictional skirmishes with the IRD project over who had responsibility for analyzing rural development participation.

Careful reading of the DAI/RTI work product reveals alternative views on what should be emphasized. While agreeing on the need for pragmatic focus, the authors of the initial state-of-the-art paper Making it Work? subsequently take two different paths. The first group centers its concerns on "problems encountered in designing and implementing complex rural development projects" (Morss and Gow, 1981) and the second focuses its attention on "processes of complex rural development project design and implementation in uncertain environments" (Honadle, et al., 1981). The latter group has produced more publications than the former. Moreover, its publications are much broader. Two books are currently in preparation that will be

restatements of each view: (1) George Honadle and Jerry VanSant's Implementation and Sustainability: Lessons from Integrated Rural Development; and (2) David D. Gow and Elliott R. Morss' Implementing Rural Development: Problems and Remedies. Finally, the process-oriented group's views prevailed in the publication of guidelines for project planners and managers, entitled Making Rural Development Self-Sustaining (Development Alternatives, Inc., 1982). This late 1982 publication summarizes much of the project's work and casts it into an applied set of recommendations.

The project's principal state-of-the-art paper, Making it Work? sets out to: (1) provide immediate practical guidance to practitioners; and (2) focus further inquiry on specific questions about the organization and administration of IRD. The paper begins by identifying four management objectives of IRD projects: (1) applying resources; (2) delivering goods and services; (3) supporting local use of these goods and services; and (4) improving the welfare of project beneficiaries. From this perspective its authors examine the organizational and managerial issues involved in achieving this sequence of objectives. In the process of doing this, the paper covers a wide range of options for: (1) designing the organizational structure of an IRD project; (2) linking the project to external organizations; (3) managing the project; (4) increasing the probability that the project will reach and involve its intended beneficiaries; and (5) establishing the potential for the project's welfare impact to become self-sustaining. In presenting these options, both their advantages and disadvantages are discussed.

Of particular importance is the paper's identification of eight common obstacles to implementation: (1) resistance to integration and coordination of IRD activities by participating agencies; (2) managerial skill deficiencies among project managers; (3) inadequate management information systems; (4) lack of incentives for project staff or cooperating organization personnel to act in ways that support project objectives; (5) delays due to procurement bottlenecks; (6) inappropriate use of technical assistance; (7) non-response to project initiatives by intended beneficiaries; and (8) failure of benefits to continue flowing after project completion. Many of these obstacles are not specific to IRD projects alone, and hence the DAI/RTI discussion of them has a wider value to development practitioners.

Among the major points made by the the paper are that:

(1) many management problems can be traced to inappropriate organizational arrangements; (2) inadequate consideration of the importance of organization affects beneficiary response to IRD activities and their sustainability; (3) while organizational design demands the highest priority, each project's organization must be custom-tailored to the local context; (4) there are no clear administrative guidelines that can be recommended as part of a universal strategy; (5) training of project managers is essential throughout a project's life; (6) projects should accept uncertainty, be designed with maximum flexibility, and be grounded on the principles of the "process" model; and (7) staff incentives are very important to the successful implementation of IRD projects.

The paper is a major advance in the literature on IRD because it sets forth a clear notion of how one goes about designing and implementing projects. This is done through the provision of 68 useful but highly general propositions and the creative use of summary lists and tables. Indeed, the summary table format developed by DAI/RTI merits careful attention by those who write applied and analytical papers for AID professionals, for they clearly convey summary information and may be effective in gaining the attention of potential users with little time to read lengthy documents.

The next set of findings and policy recommendations produced by the project are more narrowly focused. The first centers on the uncertainties encountered in carrying out IRD projects and the way a process-oriented perspective can respond successfully to them. This view is consolidated in the publication, Fishing for Sustainability: The Role of Capacity-Building in Development Administration (Honadle, 1981). This is a creative and useful attempt to establish the concept of "capacity-building" as central to the development effort, and thereby point out how critical public administration and organization theory is to overcoming today's major development constraints.

The central argument in that paper is that capacity-building is the guts of development. Taking a strong advocacy position, the paper argues that structural and cognitive change is needed if development is to occur. Hence, rural development projects should be designed with capacity-building as the central, explicit objective. The thrust of the paper extends beyond IRD to all rural development

projects. It defines capacity-building as the task of strengthening local institutions so they can absorb new resources and use them to sustain development dynamics after the initial resources are exhausted. In essence it is described as a key strategy for achieving sustainability. The links between this paper and Making it Work? are clear. What the process-oriented group has done is give high priority to two of the major findings of that state-of-the-art paper and to expand them at length.

In doing this, ample description of the concept is provided. This description concentrates on: (1) what capacity-building involves; (2) what the key elements of a capacity-building strategy are; (3) the implementation curve of a capacity-building strategy; and (4) the approach donors should take in supporting capacity-building. Briefly, capacity-building is described as a learning process based on a partnership between knowledge providers and knowledge recipients that gives conscious attention to increasing beneficiary control over development resources. For this to happen, it is acknowledged, substantial structural change is probably essential and major cognitive change is required by major actors. Capacity-building is more likely to be successfully promoted when there is: (1) risk sharing between implementors and beneficiaries; (2) involvement of actors at multiple levels; (3) existence of appropriate incentives; (4) demonstrable success; (5) collaborative activities; (6) use of an existing resource base; and (7) emphasis on learning. Central to the achievement of these patterns is a project that is flexible and

designed as a learning exercise, a concept with substantial roots in organizational theory literature, pioneered in the mid-1970s by DAI and recently advocated strongly by David Korten under the NASPAA-DPMC cooperative agreement. In essence, the paper's approach to capacity-building is founded on the development of skills and explicit attention to the importance of providing incentives and promoting beneficiary empowerment.

The capacity-building paper represents a solid attempt to move beyond the list-building exercise of Making it Work? toward a dynamic model for promoting development. As such it represents a solid contribution to the literature on project design and implementation. It also contains an interesting set of policy recommendations for donors and host governments. Most important, it is charged with ideas that call out for further treatment.

Unlike the dynamic and longitudinal approach taken by the process group, the problems-oriented group takes a more direct cross-sectional look at critical implementation constraints. The objective of this approach is to "alert" both the policy maker and the project staffer to the problems they are likely to encounter and what might be done in advance to alleviate them" (Morris and Gow, 1981:2). Toward this end, the group has selected nine specific problems to analyze:

- (1) participation and decentralization;
- (2) information systems;
- (3) political, economic, and environmental constraints;
- (4) managing and structuring technical assistance;
- (5) organizational placement and linkages;
- (6) timing;
- (7) counterpart shortages;
- (8) differing

agendas; and (9) sustaining project benefits. Each of these problems is spelled out in a crisp, tight style that is sensitive to both the applied interest of AID and the time pressures of the agency's professionals. A parallel format is used for describing each problem: (a) problem definition; (b) reasons for the problem; (c) alleviating the problem; and (d) research strategy for studying the problem.

While the discussion of each problem is brief, it is also instructive. Certainly the list of problems and general overview of their characteristics provides designers, managers, and students of rural development projects with a useful checklist of anticipating, recognizing, and analyzing problems. Particularly useful advice is given as recommendations for alleviating problems.

The analytical paper on critical problems proposes a useful approach for linking applied field studies to the knowledge-building process. Recognizing the difficulty of melding field mission needs with systematic research demands, the following procedures are adopted:

"The field team assembles in Washington at which time three or four problem areas are selected for intensive study; criteria for selection include: the purpose of the field visit, the nature of the project, and the interests and expertise of the field team.

"Upon return from the field, the team will submit brief reports on each problem area studied; and

"The primary purpose of base field data collection efforts is to provide concrete anecdotal evidence of our problem set and what might be done to alleviate them."

It proved difficult for DAI/RTI to promote the systematic application of these research guidelines, for reasons mentioned in the methodology



section. Still, they represent a first step toward developing a methodology for dealing with one of the thorniest problems hampering the cooperative agreement approach.

The wide-ranging concerns of some of the minor analytical papers merit a brief review, for several of them make useful contributions to the literature. Arguing that design is a necessary but not sufficient condition for effective IRD implementation, the paper on Using Organization Development (Armor, 1981) presents an interesting consulting strategy aimed at improving the management of IRD projects. Specifically, Armor describes an approach labeled "organizational development," spells out its utility, and offers several case studies illustrating its application. Described as a nonprescriptive approach, organizational development has as its objective assisting managers to understand the problems they face and to address them through such techniques as team building, intergroup problem solving, goal setting meetings, survey feedback analysis, and direct counseling. This social psychology approach to improved management is a promising beginning for further conceptual development and applied research. Likewise, a paper on Beyond the Rhetoric of Rural Development Participation (Gow and VanSant, 1981) provides a useful set of guidelines for getting participation components designed into IRD projects. These design rules are preceeded with a discussion of: (1) reasons for promoting participatory strategies; (2) prior approaches to stimulating participation; and (3) major constraints preventing effective participation in IRD projects. A number of

potentially useful but not empirically established guidelines are offered in support of the proposed design rules. Surprisingly, this ancillary paper provides more crisp guidelines than any generated by Cornell University's cooperative agreement on rural development participation.

In a paper on a Management Team Approach (Mickelwait, 1982) four ideal types of management contract strategies are reviewed: (1) personal contract or individual direct hire approaches; (2) university contract strategy; (3) private firm or body shop contract; and (4) management team strategy based on a permanent staff and involved home office. After examination of the strengths and weaknesses of each approach, a strong argument is made for the management team strategy. Briefly, this is an approach where field managers and the project implementor's headquarters staff carry out coordinated activities which are jointly determined and mutually agreed upon. The headquarters unit uses its experience to devise strategy within the confines of project objectives and resources and the field unit concentrates on tactics, work plans, and operations schedules. Typically, this approach requires a field team leader with a stake in the institution contracted for technical assistance, and a home office staff with rural development experience and capability to review project progress, provide short-term assistance, and contribute to strategy formulation. It also requires a contractor with a clear conceptual approach to development issues and institutional commitment to carefully dividing responsibilities between field and headquarters

to ensure maximum flexibility in the face of changing project needs and general task environment uncertainty.

While the management team approach makes sense, the paper's contribution is limited by the fact that the argument is based on unspecified evidence, the treatment of the university and private firm approaches is stereotypical and biased, and the argument is self-serving since the recommended approach is obviously that developed by DAI on the rural projects it undertakes for AID. Clearly, all three types have advantages and disadvantages to AID. What is needed is a systematic study of these so each can be improved. The paper suggests this need but fails to acknowledge that only an hypothesis is being offered and that the model is based on a few bad cases rather than systematic observation.

The last example is an interesting paper by Soesiladi, (1981). His A Counterpart's Perspective provides an unusual and different perspective on the high technical assistance content of most IRD projects. The paper points out how difficult it is for governments to refuse foreign technical assistance components in the IRD projects they want funded and provides insights for selecting those expatriate advisers and technicians who inevitably accompany IRD projects. As such, it contributes to professional understanding about how to make technical assistance more effective.

The nearly two dozen publications related to applied field research contain much interesting information. All are country-specific but some contain analysis of broad comparative value. Most

appear to have had utility to the missions and host governments that commissioned them. Country and rural development specialists also will find them useful. Unfortunately, there is no paper that organizes the products of these papers into an analytic whole. Briefly, the kind of knowledge contained in them includes: (1) a case study of the processes underlying the design of an IRD project in a highly charged political climate and the formulation of insights learned for use by future design teams (Panama: Gow, et al., 1981); (2) a review of design and implementation problems encountered in undertaking an IRD project in a remote region (Sudan: Barclay, et al., 1981); (3) an analysis of the organizational structure, implementing agency linkages, financial flows, and beneficiary participation in a creative IRD program (Equador: Jackson, 1981); (4) a review of organizational and management strategies for guiding horizontal relationships among cooperating organizations and an application of that knowledge in a workshop for an integrated area development project (Philippines: Carney, et al., 1980); (5) a case study of the role of technical strategy, beneficiary participation, information flow, project environment, and benefit sustainability in an IRD project aimed at improving farm family welfare (Jamaica: VanSant, et al., 1981); (6) a case study of the effects of IRD management decisions on beneficiary participation in decision making, local organization involvement in project selection, and leadership ability and performance among poor farmers (Indonesia: VanSant and Weisel, 1979); (7) the design of IRD management strategy, a market-oriented strategy, and a methodology for

monitoring improved welfare for project families (Jamaica: VanSant, et al., 1981); (8) a model for the design of a viable information system for an IRD (Nepal: Gow, 1980); (9) a description of how to plan and conduct a middle level management workshop aimed at developing new managerial skills (time management, setting priorities, delegation, motivation, communication, and decision making) and promoting organizational understanding and capacity necessary to use those skills (Liberia: Armor, 1979); (10) the design of a methodology for a local level workshop aimed at increasing the administrative capacity and problem solving abilities of IRD project staff and village leaders (Jamaica: Honadle, et al., 1980); and (11) the design of a pilot training project to increase the project management skills of provincial level government officers, an analysis of conditions for effective administrative decentralization, and guidelines for monitoring decentralization efforts (Egypt: Walker, 1981).

Findings in the state-of-the-art paper, in the middle range set of publications, and in the country-specific papers are all forged into programmatic recommendations in a publication entitled Making Rural Development Self-Sustaining: A Guide for Project Planners and Managers (Development Alternatives, Inc., 1982). The objective of this paper is to provide project designers and managers with strategies for building organizational capacity that will allow project benefits to be sustained after donor funding terminates. In this regard, the paper argues that for benefits to be sustained, a project must: (1) build external political and macroeconomic policy

support; (2) identify and secure continuing access to financial resources; and (3) promote organizational capacity through the building of systems and training of personnel. To facilitate achieving these ends, the paper: (1) analyzes the effects of external constraints, develops ways to assess such constraints, and offers guidelines for adjusting to them; (2) suggests issues involved in analyzing the cost-effectiveness of project activities, addressing recurrent cost questions and promoting cost-effectiveness procedures; and (3) outlines the requirements for organizational capacity, provides insights into how to evaluate existing capacity, and sets forth guidelines for analyzing issues of organizational placement and linkages, participation and decentralization, incentives, administrative systems, management development, and technical assistance. In addition, the handbook presents a method for gathering information on organizational support, finances, and capacity on a timely basis. It provides a pertinent bibliography for each section, although works cited are largely confined to DAI/RTI publications done under the project.

These topics are covered in a spiral handbook that is organized with tab dividers. The text is easy to read and uses a creative format to facilitate rapid reading. Examples from actual case studies are used as illustrations, and wide margins are provided to allow the user to make notes. Like all such handbooks, it runs the risk of being too general and brief to be relevant to specific problems in specific contexts. Nevertheless, it appears to be a

useful final product for a cooperative agreement to have generated in that it summarizes the work product and relates it to the field level. These guidelines have been circulated to missions for comment and opportunities have been sought by DAI to apply them in project activities. The results of this test and the final version of the guide are not yet published.

#### 5. Research Methodology

The methodology of the DAI/RTI publications varies with the topic, but typically involves a combination of: (1) literature review; (2) case analysis of AID and IBRD projects; (3) interviews with IRD staff and managers; and (4) reflections on past and present field research (e.g., Development Alternatives, Inc., 1975); Gow, et al., 1979). None of the papers covered here draws significantly on sample surveys, anthropological observation, or other structured methods of gathering field data.

The problem all cooperative agreements have in formulating a coherent and systematic research strategy is reflected in the DAI/RTI materials. None of the analytic papers published by the core staff presents a detailed research strategy for linking field work with knowledge-building efforts. As a result, only a few field studies appear guided by the major analytical papers and refer to only a few of the field studies.

Of the 21 published field reports, nearly half were written before the publication of the two major conceptual papers (Hbnadle, et al., 1980; Morss and Gow, 1981). Presumably, these field reports were

not guided by the frameworks developed in those two papers. Of the remaining field reports, only a few appear to have consciously tried to follow the directions developed in the two major conceptual papers (for example, Jackson, 1981; Gow, et al., 1981; Owens, et al., 1981). In addition, none of the project's analytic publications refer to more than two or three of the 10 to 13 field reports extant at the time they were drafted.

Two core members of the DAI/RTI team recognize the lack of a detailed research design for the cooperative agreement and the limited number of explicit linkages between field reports and analytic papers (Morss and Gow, 1981:1). In their opinion, this results from three important but compromising patterns that emerge when a research enterprise is asked to serve field missions and host governments. First, the resources available under the contract do not allow much time to develop a conceptual framework and research guidelines prior to the request for service to missions. Second, terms of reference for technical assistance in the field generally emphasize mission or host government agendas rather than the contractor's knowledge-building interests. Third, the technical expertise required for each field mission varies, making it difficult to forge a solid link between those providing field service and those working at the home office to expand knowledge about how to improve the organization and management of IRD programs and projects.

Still, the DAI/RTI work product illustrates the importance of "action research." In many cases it is not clear that more rigorous



research would have led to more reliable and useful findings. It may be that the future of applied research belongs to those who can effectively merge action and learning. If so, the DAI/RTI approach in this cooperative agreement merits further reflection.

An additional methodological issue needs attention. Few of the relational statements or guidelines for practitioners are supported by footnotes referring to particular studies, cases, or experiences that generated them. (This is particularly the case with the important state-of-the-art paper published in 1980 by Honadle et al.) While a number of statements and guidelines make good sense, it is often difficult to determine if they are: (1) hypotheses to be tested over the course of the cooperative agreement; (2) empirical generalizations established by research of the core group or reported in the general literature; or (3) guidelines grounded on practical experience. The lack of direct reference and the completeness of the guidelines suggests that they are often the product of thoughtful deductive analysis grounded on a general knowledge of the literature and personal experience with particular cases. If this is the situation, then questions are raised about whether "empirically-based knowledge" has been generated and, if not, what future research is needed to test, expand, and modify the guidelines presented in the DAI/RTI publications.

Again, it is likely that the statements and guidelines in major DAI/RTI publications are empirically grounded in the literature. However, outsiders must depend on the written record. If empirical

evidence is not stated, how is intersubjective evaluation of the evidence to be carried out?

Examples of this methodological issue are found in three papers. The most empirical study focuses on 21 AID projects (Crawford, 1981) and uses the data generated to confirm without expanding implementation problems identified earlier by one of the analytical papers (Morss and Gow, 1981). At the opposite extreme is Nine Critical Implementation Problems, which provides no empirical reference in support of the problems identified or the rationale for their selection. Standing between these two cases is the paper, Integrated Rural Development: Making it Work?. It has an extensive bibliography covering a rich set of literature on rural development and 68 relational statements cast as propositions but not directly related to any specific set of empirical cases.

However, the absence of empirical proof and footnotes may have some positive benefits. First, busy professionals may tend to avoid papers that look heavily academic. Second, it can be argued that a major achievement of the DAI/RTI publications is that they are produced in a format that rapidly and effectively conveys programmatic information. These potential positive effects also have corresponding costs, the most immediate of which is the loss of academic credibility. Whether such credibility is important is open to question, but it is closely related to whether empirical and intersubjective knowledge has been forged out of the project's activities. In this regard, it would have been useful for the authors of the project's

papers to have drafted annexes that discussed the amount of empirical evidence supporting the guidelines offered, or to have published reports in different formats for different audiences.

#### 6. Gaps in Knowledge

The core staff of DAI/RTI have made important advances in:

(1) identifying important processes in the design and implementation of IRD projects; (2) pinpointing significant problems limiting the effectiveness of IRD projects; and (3) identifying useful guidelines for better organizing and administering IRD. However, as pointed out earlier, it is not possible to determine whether these advances are empirically grounded.

Hence, the time may be ripe for moving beyond the limited and often ill-documented cases that underlie these advances and undertake more systematic field research to test, refine, and expand them. Until the leading findings are better tested with a broader range of data, the benefits of this first round of work will not be fully realized.

Two examples illustrate this point. First, the most important hypotheses presented in the Integrated Rural Development: Making it Work? paper merit the kind of empirical testing DAI undertook in its now classic study of Strategies for Small Farm Development. Particular attention should be given to specification of the conditions under which the hypotheses operate and the kinds of task environment variables that are likely to warp or alter their predictive power. Second, the paper on Nine Critical Implementation Problems needs to be

seriously tested and expanded. While the authors of that paper state that the nine problems were selected on the basis of "corporate experience, the literature and the knowledge of individual team member," it is not clear from the paper how the choice was made. Were these the most frequent problems mentioned by managers and/or donors? Whatever their frequency, were these the most important problems in the light of some conceptual model of implementation? More important, it seems certain that there are more than nine critical problems. Perhaps the most problematic of the papers in the DAI/RTI series is the one reviewing 21 AID-funded IRD projects. It is surprising that the review confirmed the nine identified in the state-of-the-art paper but found no additional ones. Other significant problems might include faulty design, material resources for field implementers, processes followed in project design, lack of client demand, and fit between project and surrounding social structure and culture. Hence, it would be useful to go beyond the "official framework" and seek greater empirical richness in an effort to improve upon the utility of the problem-oriented approach to IRD organization and administration.

The cooperative agreement has enhanced knowledge on how to design IRD projects in the face of task environment uncertainty and implementation constraints. What is needed now is more analysis in the face of very predictable patterns and sources of success. For example, a case can be made that an effective approach to implementation problems is to avoid them to the extent possible by mobilizing positive forces. Politics, for example, is not only a constraint, but

also one of the most powerful sources of success. Viewed in a broader perspective, the question is not only how political difficulties can be avoided once a project is launched, but how political support can be won during design and implementation. Or, for example, while DAI/RTI analysis often views "key implementers" in a negative light as sources of differing agendas, they are also the driving force behind successful implementation when properly respected and motivated. In short, by merely altering the focus on IRD organization and administration, new perspectives can be discovered that will add to programmatic understanding about how to design and implement more effective projects.

Today, IRD projects are under attack. Vague charges are made that they do not work. There is an increasing possibility that the baby will be thrown out with the bathwater. The DAI/RTI publications provide ample ideas and evidence to support the view that under appropriate conditions and with proper design and implementation work, IRD projects can reach many of their objectives. What DAI/RTI's analytical papers do not do is review the IRD experience from the perspective of the ongoing attack on IRD as a rural development strategy. Clearly, one of the major gaps in our knowledge about IRD is the lack of a sustained evaluation of the IRD experience, the emerging critique of it, and the ways in which social science knowledge can be applied to respond to those critiques. Given the accumulated knowledge on how to carry out IRD efforts, it would be a major mistake to abandon the approach without research on this critical question.

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REVIEW CALTERNATIVE RURAL DEVELOPMENT STRATEGIES

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1. Overview of the Cooperative Agreement

Basic research was not intended to be a central priority of the cooperative agreement on alternative rural development strategies that was undertaken by Michigan State University in 1977. Similarly, long-term applied research was expected to be a secondary priority. The cooperative agreement was established primarily to enable AID-Washington and field missions to have ready access to a group of skilled professionals who could offer assistance in the planning and pursuit of rural development activities, especially through the instrument of the country development strategy statement (CDSS). Thus, extensive technical assistance was and has remained its primary purpose. Nevertheless, the cooperative agreement has generated important research outputs, some of which have been influential in stimulating the adoption of new approaches to understanding old problems. This has been particularly true of research on farming systems, marketing, and techniques of data analysis. Program-matically, research outputs have been particularly concerned with issues of adoption and cost-effectiveness of the strategies they propose. Extensive assistance to field missions in Cameroon, Thailand, Bolivia, Honduras, Pakistan, and the eastern Caribbean also

generated experience and data that is reflected in the research output. In general, the research papers and other research-related documents are a result of broad familiarity with the literature on rural development and the extensive field experience of the MSU professionals; they also derive from long-term research undertakings in Cameroon and Thailand that have generated two series of focused working papers.

The cooperative agreement on alternative rural development strategies is notable for the attention it has given to addressing academic, mission, and host country audiences with the results of its research. Often this has occurred less through its publications than through its efforts to influence mission activities and capabilities, to train and supervise graduate students, and to involve host country nationals in ongoing research tasks. In this regard, MSU has encouraged a multiplier effect for its research findings. The MSU research team has also been concerned with directly influencing the organization and management of agricultural research and extension systems in third world countries.

## 2. Assumptions

The research output of the cooperative agreement has consistently affirmed a number of central assumptions:

- a. It is a basic assumption that rural development implies broad-based socioeconomic change that goes far beyond simply altering technologies in order to increase efficiency. Thus, a holistic approach to rural development is adopted.
- b. It is assumed that rural development is a farm and agriculture-oriented process, although it has major

implications for nonfarm-related activities such as off-farm employment, health, and education.

- c. The research output assumes explicitly or implicitly that rural development means reaching small farmers with increases in production and opportunities for improved welfare.
- d. Research efforts begin with the assumption that technological innovation is critical for improving productivity at the farm level.
- e. Small farmer decisions to adopt, reject, or alter innovations are assumed to be based on their realistic assessment of production and marketing opportunities, relative advantages in shifts in household labor allocations, community and household norms, and understanding of local ecological conditions and their variation over time.
- f. It is consistently held that rural development is constrained by a variety of local and extra-local economic and political conditions and that expectations for change should be tempered by knowledge of these factors.
- g. There is also throughout the research effort an implicit or explicit assumption that the rural household, particularly the farming family, is the basic unit of consumption and production in the rural economy.

This set of perspectives forms the basis for wide-ranging exploration of strategies to achieve more effective rural development.

## 2. Major Hypotheses and Findings

For somewhat over two years the activities of the cooperative agreement team were not highly focused and its energies tended to be underutilized by AID field missions. This was due to a decision that CDSS analyses were to be done by missions without the assistance of consultants. It also resulted from the decision of a major mission not to use one of the MSU specialized core staff persons. As a

result, during the early years, the project struggled to find a coherent focus and completed research and writings on activities undertaken prior to the start of the cooperative agreement (e.g., Holdcroft, 1978). While this makes it difficult to know what was learned during the project, efforts have been made to focus on the eclectic output of the project, a focus that may give it a coherence that it lacked in its initial period.

During the first few years, a series of reports on various approaches to rural development were produced. They explore an important hypothesis: rural development strategies will not be successful unless and until they take a holistic and multidisciplinary approach to understanding the rural household and the rural community. Thus, an initial paper in the MSU rural development series written by Akhter Hameed Khan reviews a long history of rural development strategies, lays bare their underlying assumptions and ideological perspectives, and indicates that none of them was sufficiently anchored in a broad appreciation of the constraints on development (Khan, 1978). Benedict Stavis (1979) argues that agricultural extension services typically fail because they are based on the assumption that innovation is in the interest of the farmer and that ignorance, tradition, or poor communication are at the base of failures to innovate. Instead, Stavis indicates that failures must be linked to the "structural context through which the innovations are selected and communicated" (p.2). Similarly, an exclusive concern with production in agricultural research and extension activities will

mean a failure to appreciate the constraints and opportunities for farm activities introduced by marketing systems in third world countries (Riley and Weber, 1979; Fox and Weber, 1982). Country-specific evaluations carried the same message: a study of a program to promote animal traction on farms in Upper Volta was critical because the program failed to anticipate the implications of the new technology for the adopting households (or the technical support they needed from the promoting agency) (Barrett, Lassiter, Wilcock, Baker, and Crawford, 1982).

This central hypothesis is complemented by a second: rural development strategies in vogue at particular historical moments can be traced to ideological and theoretical assumptions that tend to blind their advocates to the realities of rural economic and political contexts. Thus, in a review of community development efforts between 1950 and 1965, Lane Holdcroft (1978:26) argues that, "Community development had great appeal to leaders of developing countries and external donor officials because it provided a nonrevolutionary approach to the development of agrarian societies." Ultimately, the movement failed because of the naive or self-serving adoption of theories of rural development that ignored "[f]actors such as distribution of land ownership, exploitation by elites, or urban domination" (p.21). Changing conditions, although essential to rural development, were beyond the efforts of even the most well-meaning and committed community development workers. Holdcroft suggests that many of the underlying assumptions of more recent integrated rural develop-

ment approaches are similar to those of the community development movement and are similarly vulnerable to failure. Carl Eicher and Doyle Baker (1982:61-63) argue that the integrated rural development approach is based on unrealistic assumptions about the costs of social services and the gains in food productivity they are expected to generate. Carl Eicher (1982) similarly indicts dependency and political economy approaches for their "failure to provide a convincing understanding of the motivations of rural people, and the role of technological change" (p. 159). The research output has consistently pointed out the "trendy" nature of research on rural development.

In 1980, the research effort of MSU found a more specific focus in farming systems research approaches. In retrospect, the critical perspectives of the early reports were an important forerunner to this later concern. Since 1980, those involved in the MSU cooperative agreement have devoted much of their time to exploring the feasibility and utility of the adoption of farming systems research by agricultural research and extension organizations. Considerable technical field support has been devoted to country-specific applications of farming systems research methodologies. Gradually, the approach has acquired growing support within AID. According to Eicher and Baker (1982:159), the "primary goal of farming systems research (FSR) is to design research programs which are holistic, interdisciplinary, and cost-effective in generating technology which is appropriate to the production and consumption goals of rural households in specific micro-environments."

The research effort at MSU has argued: (1) that farming systems research should not be the only approach to agricultural research; (2) that the household in rural areas should be the basic unit of analysis; (3) that indigenous technologies are usually based on generations of trial and error adaptations to the needs of the household; (4) that nonfarm activities are an important part of the rural household's consumption and production activities; (5) that small farmers must be active participants in testing new agricultural practices; and (6) that farm level testing will have a positive impact on national research systems (Eicher and Baker, 1982:159). Farming systems research is therefore directed toward increasing the "productivity of the farming system in the context of the entire range of private and societal goals, given the constraints and potentials of the existing farming systems" (Gilbert, Norman, and Winch, 1980:2). As such, it is proposed as a holistic but site-specific approach that provides an alternative strategy for making traditional agricultural research and extension activities more relevant and successful in reaching small farmers.

The MSU project has been effective in stressing the importance of understanding farm level decision making within a broader context of the household, local organization, and national and international contexts and policies. There is a significant attempt, evident especially in an important and extensive review of relevant literature produced by Eicher and Baker (1982), to synthesize perspectives derived from political economy and those generated by micro-level studies of individual behavior.



Nevertheless, the research output of the MSU project on alternative rural development strategies has not been highly theoretical in nature; rather, it has been consistently addressed to pragmatic issues that are concerned with tools of the trade for agricultural research and management. For example, a review of the experience of farming systems research in Honduras between 1977 and 1981 resulted in a series of recommendations about how to manage and integrate such research in a national research institution, how to generate data, and how to implement farm trials (Galt, Diaz, Contreras, Peairs, Posner, and Rosales, 1982). Similarly, a brief and readable summary of the potential uses and disadvantages of micro-computers and programmable computers in agricultural research provides a summary of the problems involved in alternative technologies, especially in the third world, and warns against the full scale adoption of technologies that cannot be supported with personnel, electricity, foreign exchange, servicing and spare parts, or need in many settings (Weber, Pease, Vincent, Crawford, and Stilwell, 1983). Once again, the concern is for direct field application of available technologies, with a final list of recommendations directly relevant to users.

#### 4. Methodology

Much of the research output of the MSU project is generated on the basis of broad familiarity with the literature on rural development and the field experience of the researchers. Central among these efforts is an extremely comprehensive and readable review of the

literature on the rural economy of Africa, especially those aspects of research most useful to economists and agricultural economists (Eicher and Baker, 1982). This document considers some 1400 books and articles on farm and nonfarm rural development activities in subSaharan Africa and provides a concise evaluation of it. In addition, the critical reviews of previous rural development strategies (Khan, 1979; Holdcroft, 1978) summarize a broad literature and experience that points the way toward a rationale for farming systems research approaches. The findings consistently affirm the central assumptions and hypotheses of the entire research undertaking. In some cases (in particular, Stavis, 1979), researchers have consulted a broader social science literature to underpin arguments about political and structural constraints on farm communities and households.

The more recent focus on farming systems research is enriched by the experience of MSU researchers and their own efforts to develop a viable strategy for rural development that would be particularly appropriate for adoption by agricultural economists and biological researchers. In this work they have been particularly concerned to assess the methodologies and techniques that are useful to those doing micro-economic research in rural areas and to those who use research to design or improve projects. Eicher and Baker (1982:72-94) consider the methods and related concepts most frequently adopted to generate data for micro-economic analysis—case studies, infrequent surveys, and multiple visit or cost route surveys—and assess them in terms of their practical implications and output for program and project design

and for a variety of analytic techniques in use. Crawford (1982) presents one such analytic technique in detail in presenting a methodology for stimulating constraints on small farmer agriculture in Northern Nigeria. The research team has also been concerned about the appropriate utilization of micro-computers and programmable calculators for research and program management in agriculture (Weber, Pease, Vincent, Crawford, and Stilwell, 1983; Eicher and Baker, 1982). Similarly, Strauss (1983) presents an econometric model of the consumption and production of food of rural households based on survey research undertaken in Sierra Leone.

Recent work on farming systems research has gone beyond a review of the literature to specific interest in ongoing efforts to apply this approach. In particular, M.P. Collinson (1982) reports on the efforts of CIMMYT and others to pursue farming systems research approaches in East Africa. The field research, consisting of an evaluation of applied experience, is used to generate a series of recommendations about how the approach can usefully be adopted by other organizations or other countries. Collinson also presents observations about potential problem areas when farming systems research efforts are undertaken. Of the documents reviewed, this one offers the greatest insight into operational guidelines for field practitioners. Indeed, the Collinson report provides step-by-step procedures for carrying out a farming systems research approach that will result in usable information about needed technological innovations and appropriate services for small farmers. It is a

logical accompaniment to the Gilbert, Norman, and Winch paper (1980) that provides a fuller description of the approach along with well-tempered enthusiasm for it as a viable program option for missions and research and extension organizations. Another empirical experience with the adoption of farming systems research is considered in Galt, Diaz, Contreras, Peairs, Posner, and Rosales (1982).

In terms of the methodologies used to generate insights and recommendations in the research output, the study by Barrett, Lassiter, Wilcock, Baker, and Crawford (1982) on animal traction in Upper Volta can be commended for its attention to issues of sample selection, its concern for the empirical detail provided on important variables, and the judicious tone of the analysis that avoids over-generalizations or simplifications of often ambiguous data. Strauss (1983) is much less exact about how a sample is drawn from geographic areas and households in Sierra Leone; this study and Crawford's (1982) would gain in credibility if greater detail were provided on the sampling methodology employed.

In terms of the suggestions about methodology for farming systems research that are apparent in several studies, a number of issues deserve greater attention. First, researchers concerned about promoting farming systems research need to be clearer about what the approach includes and excludes. This is important in order to develop a more parsimonious list of factors that need to be considered in field research and procedures that are timely and manageable. Second, advocates of farming systems research need to work toward specifying

common and generalizable tools such as questionnaires, interview techniques, sampling procedures, and guidelines for coding and interpretation of data. The literature on survey research and program evaluation should be consulted in this process. Third, explicit attention needs to be given to establishing criteria to judge the effectiveness and efficiency of field tests on technology innovation. Attention to these methodological issues would make findings and recommendations more consistent and generalizable from one study site to another.

#### 5. Programmatic Implications

The impediments to wider field adoption of a farming systems approach are not so much inherent in the research effort as they are in the approach itself. Thus, while there are strong reasons why farming systems research approaches could be usefully applied, they are (as is recognized by MSU project staff) complex and time consuming and often require great changes in the attitudes and behavior of researchers and extension organizations. They require practitioners to be familiar with a broad range of social, economic, political, and technical factors that affect small farm agriculture. They require multidisciplinary research in which economists and other social scientists are integral to the effort, not subordinate or peripheral to it. Moreover, they require that field practitioners be adept at assessing site-specific constraints on farm-level productivity in short periods of time. Clearly, then, they require a relatively skilled and dedicated team to carry them out. Indeed, considerable

effort at MSU has been directed toward exploring means to enable a farming systems approach to be more readily acceptable to field practitioners and more successfully implemented when adopted. The project has been concerned with strategies to demonstrate the cost-effectiveness of the approach, to reduce its time requirements, and to overcome inherent and discipline-specific obstacles to interdisciplinary research (Eicher and Baker, 1982; Galt, Diaz, Contreras, Pairs, Posner, and Rosales, 1983). However, the impediments to wide adoption will not be easily overcome and should be subject to more exploration and experimentation.

The farming systems research approach has sharpened the concern of the MSU research team for the quality and appropriateness of research being done by national agricultural research services (Eicher and Baker, 1982; Galt, Diaz, Contreras, Pairs, Posner, and Rosales, 1982; Collinson, 1982). In general, national research institutions are not organized or motivated in ways that make the adoption of farming systems approaches feasible. Therefore, findings of the MSU work on farming systems research are disseminated largely through field visits by project staff and the efforts of AID Washington staff to popularize the approach through its missions. In addition, support has come from among the most prestigious and successful of the international agricultural research institutions, especially IRRI and CIMMYT. These institutions have also collaborated in field experiments with the approach. They serve a valuable role in disseminating a particular approach to the international and broad

national research extension community of practitioners. Attention could usefully be given to how their role might be expanded, particularly in terms of the training activities they undertake.

#### 6. Gaps in Knowledge

This review of the research output of the MSU cooperative agreement on alternative rural development strategies has indicated the early experience of the project in identifying highly generalizable problems with a large number of approaches to rural development; the approaches were revealed to be based on frequently erroneous assumptions about the nature of small farm activities and they often failed to consider rural households as units central to complex systems of constraints and opportunities for increasing productivity. The shortcomings of previous experience provide a rationale for the adoption of farming systems research approaches and this rationale is strongly purveyed in recent project work. At this point, it would be useful to ask, in as critical a fashion as prior approaches were addressed, to what extent farming systems research is being presented as a "new panacea" to rural development with some of the same shortcomings. This is not suggested as a direct criticism of the approach itself or of the efforts of the MSU researchers. Rather, it is an effort to suggest an important area for additional research.

Importantly, it should be acknowledged that farming systems research approaches are based on an assumption that development in the form of appropriate technological innovation and increases in productivity for small farmers is feasible without major structural changes in the

society, major conflict over the distribution of resources, or revolutionary confrontations. Ongoing field experience should be carefully assessed for data that would illuminate such a perspective, as it is an important unanswered question in existing research efforts. If the farming systems research approaches carry with them fewer of the ideological and perceptual shortcomings of other approaches, then it is important to indicate why and how this has occurred.

In a broader sense, there is room for the MSU research team to address a series of issues that is clearly related to their ongoing concerns. These range from research on the linkages between agricultural production and marketing to the policy framework for promoting more effective food security systems (see Shaffer, 1983). Both these broad topics imply research to explore urban-rural linkages more fully and to explore the relationships between macro policy and micro behavior. Related also to MSU's interest in marketing are the issues of institutional innovations that would encourage greater efficiencies and comparative institutional analyses that would provide insight into alternative ways to improve performance.

Two other issues constitute a second generation of research concerns. First, at the risk of discovering even greater layers of complexity, household economics as the organizing basis for farming systems research should be pursued with greater attention to intra-household units of production and decision making and the impact that technological change is likely to have on shifting roles within



households. More attention to the role of women within the household, their contributions to decisions about production and consumption and the impact that technological change is likely to have on shifting roles within households is implied here. If farming systems research is to be further pursued as an approach to rural development, concern for intra-household divisions of labor, authority, and roles must become an integral part of the analytic frameworks for studying the rural household. In fact, whether the rural household is the basic unit of consumption and production in specific rural economies should be treated as an empirical question.

Important in the analysis of intra-household decision making is the need for greater attention to gender as a variable that can significantly affect factors such as the allocation of household labor and expenditures. This can become a factor in differentiating among small farmers and in assessing their response to technological innovations, especially where female farmers or female heads of farming units are involved. Thus, gender should become an important variable just as size and quality of landholding or education and income are in assessing the response of the household to technological innovations. Similarly, changes in the relative importance of agriculture for rural households should be explored for their contributions to perspectives about increasing productivity and encouraging innovations. Thus, as smaller proportions of family income are derived from farming activities because of off-farm employment and trade opportunities, how does decision making about farming activities change? How do relationships

to the land change? How do seasonal demands for labor influence farm and nonfarm income-generating strategies? An important focus of further research should be explicitly with these processes of change within household units (see Crawford, 1982).

Another second generation issue involves exploring the economic and social linkages that extend beyond the household to local, regional, and national structures that affect both consumption and production. Just as taking the household as the site of the "joint" preferences or utilities of household members can be very misleading, so can the failure to set any particular domestic unit within a larger framework of interaction result in misunderstanding of the dynamics of rural development and change. The development of perspectives and theories to explain how household and inter-household and intra-household interests become aggregated for cooperative activities, marketing, organizational pursuits, or community-level governance are important here. Understanding households as a unit subject to a set of constraints and opportunities that determine the options available is a positive step away from viewing rural individuals as independent decision makers. The next step should be a focus on structures within and beyond the household that incorporate the activities of household members in terms of production and consumption. This kind of research can usefully be linked to the identification of "recommendation domains" of households that share enough significant characteristics to allow them to be identifiable targets for cost-effective extension activities.

Continued research on methods for operationalizing farming systems research is also recommended, including greater attention to organizational, personnel, and disciplinary impediments to its adoption. If it is indeed a valuable approach, then a large effort must be directed to exploring both opportunities and impediments to its adoption. Case studies of efforts to implement a farming systems approach, such as the Collinson (1982) paper, could be valuable tools to promote such understanding of its adoption. Finally, while the MSU project is currently committed to farming systems research approaches to rural development, it should neither abandon a critical perspective on the approach nor fail to remain alert to new developments that might promise alternative strategies.

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REVIEW DRURAL NON-FARM EMPLOYMENT

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1. Overview of the Cooperative Agreement

In 1977 the Office of Rural Development and Development Administration of AID entered into a cooperative agreement with Michigan State University to undertake a study of rural non-farm employment. Michigan State's Department of Agricultural Economics was the organizational center within the University for this work. The agreement represented the continuation and expansion of work the department had been doing for several years previously, at a much lower level of support. The project ended in December 1982, with a total of 44 papers and eight theses produced and another five in progress. In the process of doing this work, MSU collaborated with at least three third world institutions and the ILO. AID then established a new cooperative agreement with MSU on small enterprise approaches to employment that runs from September 1982 to August 1985.

The cooperative agreement on rural non-farm employment resulted from the recognition that under- and unemployment are an almost universal phenomenon in developing countries. Although difficult to quantify because of its seasonal and disguised nature, it is clearly a major development problem and one that contributes to a number of others such as rural malnutrition and urban migration.

Quantitatively, much of this unemployment is rural, and there is a growing recognition among development specialists that agriculture cannot absorb enough additional labor to deal with the problem. In 1977, however, not enough was known about the nature, extent, or structure of off-farm employment. Therefore, it was difficult for AID or national policy makers and program and project designers to create the policies and programs needed to increase rural off-farm employment. Given Michigan State's previous work in this area, it was expected that they could add further to empirically based knowledge about the nature and prospects for off-farm employment. It was also expected that MSU would be able to provide AID with policy-relevant advice and technical assistance to missions in specific countries that were concerned about creating new off-farm employment opportunities.

## 2. Assumptions

While some analysts have expressed the view that rural unemployment can best be dealt with by expanding agricultural production, there is little evidence to support this view. Although agriculture has some potential to create additional employment opportunity, this is probably not enough to solve the problem of rural unemployment over time. As a result, this cooperative agreement was undertaken with "the basic purpose of enhancing the ability of AID missions and host country institutions to identify and implement programs and policies that generate off-farm employment and income opportunities benefitting the rural poor" (Chuta and Liedholm, 1979:1). The dual assumptions of the importance of off-farm employ-



ment for dealing with both unemployment problems and poverty is explicit.

The authors of this state-of-the-art paper also note, "This growing interest [in off-farm employment] stems from and reflects the increased international concern for equity and employment objectives, and the corresponding reduction of emphasis on the earlier strategies that had focused primarily on growth and output objectives. The de-emphasis of growth and output objectives reflects a disillusionment with the inequitable results of rapid growth in certain countries and the disappointing results of attempts to industrialize by establishing large-scale, urban-based, capital-intensive industries" (Chuta and Liedholm, 1979:1). The authors then go on to quote Morawetz' assertion that "remarkably little is known about its [off-farm employment] composition and characteristics." Thus, the cooperative agreement assumed the importance of off-farm employment and set its task to learn more about its extent, its composition, its characteristics, and the actions that could be taken to promote it. This assumption is not challenged or questioned in the work done under the agreement and the work is focused on acquiring more detailed information on non-farm employment as an important aspect of development. The research component of the project is focused on systematically documenting the extent, composition and characteristics of off-farm employment.

The cooperative agreement classifies off-farm employment in three general categories: (1) manufacturing; (2) commerce; and (3) services (Chuta and Liedholm, 1979:8). However, the work of Michigan

State has focused primarily on small-scale rural manufacturing activities or, as they came to be called, the small-scale enterprises (SSE). This decision was made as MSU's research evolved because researchers found that much of the employment in commerce and services was linked to small-scale enterprise, either through the provision of inputs or the marketing of the products of such enterprises. MSU also decided that by concentrating on small enterprise as an integrated system for procurement to final sales, they could better develop policy and program relevant recommendations and operational assistance. This decision was undoubtedly a correct one in terms of the relative magnitude of employment created by alternative activities and of using resources to obtain maximum learning and to provide policy and program assistance. Nevertheless, forms of off-farm employment that are not connected with small-scale enterprise are also important, particularly in areas such as Bangladesh where rural unemployment is particularly high. It would be valuable to know more about these activities.

As MSU researchers explored what was of importance to the small-scale enterprise, they came to recognize that marketing, both in terms of procurement of inputs and of marketing of output, was critical. They focused on marketing in relation to its impact on small-scale enterprise rather than as a separate source of off-farm employment. This was part of the larger transition from an initial focus on specific employment-creating activities in commerce, manufacturing, and services to a focus on the small-scale enterprise

subsector rather than on employment creating per se. It is important to note, however, that the change is not as substantial as it may seem. The issue of employment remains central in the consideration of the small-scale enterprise and the shift represents the evolution of thinking based on the careful accumulation of empirical evidence as to how off-farm employment is increased through rural enterprise.

### 3. Major Hypotheses and Findings

The Michigan State Rural Non-Farm Employment Project has consolidated knowledge of the topic, defined it as a separate and significant dimensions of development and advanced our knowledge substantially. Before the project, there were a number of micro level studies and data from countries that suggested that non-farm employment was important, but the information was scattered and incomplete. As a result the topic was not defined and no serious attempt had been made to generate appropriate programmatic and policy actions necessary to promote non-farm employment or small-scale enterprise. Michigan State has consolidated previous knowledge and redefined the topic, generating substantial new knowledge of this important topic. In the process they have shifted the perspective from non-farm employment to small-scale enterprise. The acceptance of this shift is reflected in the research topics covered by this project that look at future research needs. The topic of small-scale enterprise is the one identified for future work rather than non-farm employment.

The Michigan State work can be divided into three broad categories that follow each other more or less chronologically. The first

is a consolidation of existing knowledge in the initial state-of-the-art paper. This paper defines the topic and the issues and shapes the nature of the second broad phase of research, which focuses on gathering additional empirical information on the basic parameters of non-farm employment and on more carefully defining the policies that influence the performance of small-scale enterprise. The bulk of the MSU work is concentrated in this area, and many papers provide the empirical verification or extension of findings contained in the state-of-the-art paper. Some also explore at greater depth the issues that the paper raises. Included in this are policies designed to promote small-scale enterprise. The third area may be termed the development of strategies to promote small-scale enterprise. Included in this are some policy issues, but the focus has shifted from documenting the magnitudes of small-scale enterprise and the factors that affect its performance to approaching the issues in strategic terms and looking at the activities and policies that might promote small-scale enterprise. It is important to look at each of these stages of the MSU work in turn.

The first phase of the MSU work was the monograph by Chuta and Liedholm, *Non-Farm Employment: A Review of the State-of-the-Art*. This monograph consolidates the findings of a number of existing micro level studies. It effectively defines the non-farm employment issue and establishes that the sector is much more important than has been generally recognized. As a share of total employment, the authors summarize evidence to indicate that non-farm employment engages a

minimum of 20% of the rural population as primary employment and another 10 to 20% as a secondary source of employment (pp. 3,5). Similarly, at a minimum, 20% of rural incomes are derived from nonfarm employment (p. 6) and in many countries and regions the amount is greatly in excess of that amount. General findings show that wages and incomes generated in non-farm employment generally exceed those earned in agriculture, and as farm size becomes smaller over time, non-farm incomes become larger (pp. 13,14). With development, non-farm employment increases in both relative and absolute terms (p. 17). Therefore, we can be quite sure that it is of growing importance over time. Likewise, there is a positive relationship between rural income and the demand for non-farm income (p. 23). The question is also asked whether rural non-farm enterprise operations are limited almost exclusively in the rural sector. The authors discover to the contrary that the goods produced in rural enterprises enter both the urban and the international markets in significant measure (p. 29).

Turning to the characteristics of non-farm employment in comparison to urban-based larger scale enterprises, rural small enterprises are generally more labor intensive than other segments of the economy, but labor productivity is higher in larger enterprises with more capital stock. Capital productivity, however, is generally higher in rural enterprises (pp. 31,34,36). Given that capital is generally scarce in most developing countries, rural small enterprise can be considered more efficient in most situations. The point is pursued further and in specific comparisons rural small enterprises

are found to be more labor intensive and more efficient per unit of capital than urban based counterparts in the same industry (p.41). The implications of this for development are important as savings from wages are low but profits from rural enterprises are found to be higher than urban based enterprises (p. 46).

A series of initial policy conclusions are also suggested. Among the price-related policy issues is the question of interest rates. Urban enterprise benefits more from the artificially low interest rates and such rates reduce the rural credit supply (p. 55,56). Foreign exchange and tariff policies are found to have a substantial effect on rural non-farm enterprise (pp. 56,58). Minimum wage policies affect urban enterprise far more than rural, which are generally less influenced by the formal economic and legal system. Among the non-price effects are infrastructure development policy which is often designed to assist urban and rarely rural enterprise (p. 60). Industrial policy, which often is designed to promote manufacturing enterprises, frequently has a negative affect on rural small scale enterprises. Import licenses, permits, etc., are often beyond the reach of rural small enterprises both in geographic and bureaucratic terms (p. 61). These policy-related findings suggest the ways in which policy can effect and promote or deter rural non-farm enterprises that are the primary generators of non-farm employment.

Many of the subsequent MSU studies build on this state-of-the-art paper; some gather more empirical data on the issues covered in that study, while others go beyond them and look at policy implica-

tions of these initial findings. Byerlee, Eicher, Liedholm, and Spencer (1982) build on the earlier paper to explore a series of macro policy issues. They conclude that the evidence they collected in Sierra Leone suggests that increased consumer incomes, import substitution, and increased equity in incomes will increase the demand for the products of small-scale enterprise. On the supply side, the authors find that factor price distortions in interest and wage rates affect large scale enterprise far more than small, but that small-scale enterprises pay higher duties on imported inputs and lack the tariff protection accorded larger enterprises (p. 335). They also confirm the findings of Chuta and Liedholm that labor intensive small enterprises are more efficient users of capital and that the suggested employment-output conflict is not a serious problem. Finally, the authors examine whether changes in factor prices would affect the choice of technique. They conclude that changes would alter the optimal technologies and probably result in lower production costs and increased labor use (p. 335).

In a later paper, Liedholm and Chuta (1983) present empirical evidence from Sierra Leone. They conclude that policies to improve technical and managerial skills could be effective at promoting small scale enterprise (p. 8). They further find that while small entrepreneurs feel capital shortage was their greatest difficulty, it is not an overriding constraint. Small entrepreneurs do need access to formal capital markets which they currently have in very limited measure. However, this capital should not be provided at artificially

low rates of interest, as is common in many capital markets, for these rates dilute labor use and encourage artificial capital intensity (pp. 9-11). Finally, they confirm the substantial affects that fiscal monetary and wage policies have on small-scale enterprise (p. 11).

Other studies build and enhance our knowledge of the area of non-farm employment. Small-scale enterprise is a major employer of women to a far greater degree than previously recognized. In addition, women play critical management and technical as well as labor functions in small-scale enterprises. Evidence of this is found in both Jamaica (Davies, Risseha, and Kerton, 1979:14,15) and Egypt (Working Paper 23, no author, pp. 36,49).

A severe threat to small enterprise is discovered to be government action to subsidize larger scale competition in either the public or private sector. Particularly in activities providing food supplies, such as bakery goods or dairy products, there are examples of government subsidizing competition and thus wiping out many small enterprises and jobs. An example of this is found in Egypt (Working Paper 23), and a similar situation has occurred in India.

Additional evidence of urban and international markets being served by rural small-scale industry is found. In several countries small enterprises provide important exports, particularly in the field of handicrafts in India, or athletic supplies in Pakistan. These markets are usually overlooked in official policy, or in some cases the exports are taxed, reducing important incentives and opportunities to earn foreign exchange. The early evidence that rates of profit are



higher in rural small enterprise than in larger scale businesses is confirmed in several studies. Likewise, returns to capital investment tend to be relatively higher. Further evidence is also found to suggest that savings rates and rates of reinvestment are higher in small enterprises. Particularly surprising and revealing is the fact that small enterprises are much less dependent on external sources of capital than large enterprises. Evidence of these points comes from Kenya (Kilby, 1982:40), Colombia, and Sierra Leone.

Another set of papers deal with the techniques of promoting small off-farm employment through small-scale enterprise. Donald Mead's studies of Thailand and Indonesia (described in more detail below) deal with the issue of industry organization, specifically subcontracting systems, to see how production processes can be disaggregated to promote small industry. Other papers, such as Mabawonku's on apprenticeship training or Fisseha's on management, look at specific techniques that can be used to strengthen small-scale enterprise.

The third broad category of work under the cooperative agreement represents the consolidation of the previous two stages into a strategic approach to the small enterprise subsector. Many of the works previously mentioned contributed to this stage. However, the two works that best symbolize this shift in approach are Mead's studies of subcontracting already mentioned. In these, small-scale enterprises are the focus, and they are viewed as an integrated system. The focus is no longer on the characteristics or dimensions

of small-scale enterprise but on the entire subsector, from initial inputs to final product. This perspective opens the way for effective policy prescription to promote small-scale enterprise. Once the full system is understood, then the questions become, as Mead concludes, "the circumstances under which production/distribution systems may operate in dis-integrated ways, enabling small producers to participate in the growth of the economy and thereby bring benefits to producers and nations alike...it is only in selected circumstances that such a pattern is both beneficial and feasible. Focused research is required to reveal whether these circumstances apply to a particular industry, in a particular country, at a particular point in time" (Mead, n.d.:28). His questions set the agenda for future research.

Michigan State has moved from definition to detailed description to a new approach to non-farm employment, a focus on the small-scale enterprise subsector. They have in the process generated a wealth of knowledge about non-farm employment and small-scale enterprise. They have provided an extensive picture of the subsector and set the agenda for ways in which future knowledge in this area may be advanced.

#### 4. Methodology

The Michigan State team began their work with an extensive literature review that constituted the basis for their state-of-the-art paper (Chuta and Liedholm, 1979). This summarizes the literature and empirical research on the topic of off-farm employment from all

sources. That paper establishes a conservative knowledge baseline on which the field research work could be built. Generally, the MSU studies can be divided into country studies and overviews of the topic (51 fall in the former category and 9 in the latter). These former categories in turn may be divided into studies of general issues, surveys, and investigation of small enterprise enhancement techniques. The relative numbers of each of these are indicated in the table.

As can be seen from these figures, country research dominates the work. Countries were chosen both because of MSU contacts and because of AID interest and requests for assistance. There is little comparative work except in the general overview pieces. A number of studies concentrate on Thailand, Sierra Leone, and Egypt. Additional studies focus on Nigeria, Kenya, Haiti, Jamaica, Indonesia, Bangladesh, Honduras, and Botswana. This focus seems appropriate for a period of empirical testing of a range of propositions. It is to be expected that having established the dimensions of the issues, the next phase will include more cross-country comparisons and the conclusions will be more prescriptive.

Michigan State has entered into collaborative research arrangements with a number of third world institutions to conduct field research under the cooperative agreement. It appears they have been particularly successful at this and have maintained the quality of the work through such collaborative arrangements. In addition to third world institutions, MSU has also collaborated with the ILO on several studies. This collaboration has had multiple benefits. It

Table 1**Broad Classification of Reports Done Under  
M.S.U. Off-Farm Rural Employment Project**

	<u>Country Studies</u>	<u>Overview Papers</u>
General Issues	12	5
Field Surveys	8	
Employment Enhancement Techniques	5	4
Manufacturing	22	
Commerce	2	
Services	2	

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has provided MSU with access to detailed knowledge and information. It has enriched their insights and findings with third world perspectives, it has helped with institutional capacity in third world countries, and it has provided MSU scholars with a foreign base that enables them both to collaborate on research over time and to be continuously available to assist AID. Finally, and perhaps most important in MSU's perspective, it has enabled them to get national scholars and leaders interested in and focused on the issue of rural non-farm employment. This has specific benefits in terms of nationals

of various countries who move between research institutions and government, having an influence on national policy as it relates to off-farm employment.

There is no final summary paper for the cooperative agreement, despite the early intention to produce such a document. Chuta and Liedholm's paper, Rural Small-Scale Industry: Empirical Evidence and Policy Issues, which is to appear in Carl Eicher and John Staatz, Agricultural Development in the Third World (forthcoming), summarizes the findings, but does not provide the type of summary and policy recommendations that would be most useful to the policy maker, since it is designed for a different audience. Hopefully, since this cooperative agreement has been followed by a new agreement, such an updated state-of-the-art paper will be forthcoming.

##### 5. Programmatic Implications

The quality of the research and the generation of new and empirically based knowledge on the topic of off-farm employment under the MSU cooperative agreement has been excellent. Knowledge about this area has been greatly increased and a basis has been prepared for important contributions to development programs. Whether the knowledge generated under this project is being translated into policy changes in the field is less certain. Many of the reports and papers produced under the project have concluding sections that deal with policy issues. However, it is less certain how readily available these are to field personnel. The findings, which may be very relevant to policy, are often available in places where field

practitioners are not likely to look. For example, the conclusion that "the demand for labor intensive commodities would be marginally favored by more equitable consumer incomes. On the supply side-- distortions in wage rates and interest rates are largely confined to the large-scale sector--small-scale sectors pay higher duties on imported inputs and equipment and at the same time lack the tariff protection of competitive products enjoyed by large-scale sectors" (D. Byerlee, et al., 1983:335), is a very important set of findings, but one that may not be dug out of an article in Economic Development and Cultural Change and translated into policy change in the short run by an AID mission staff or government policy makers. Policy relevant statements are made in most documents, but usually not translated into specific prescriptions for policy change.

Many of Michigan State's publications appear in scholarly journals, which indicates their quality, but does not make them easily accessible to the field. While MSU has circulated reprints, they have not had a monograph or reprint series such as Cornell or DAI that ensures that their substantial output is easily available. Such a series would have enhanced the accessibility of their work. Some of their output has been in the form of theses, and an abstract or summary of these would be useful and would enhance the value of the research for field operations.

It is quite appropriate to ask if it should be the responsibility of a university that has conducted a careful and high quality research program to undertake to provide detailed policy prescriptions

based on its work. Furthermore, as staff at Michigan State point out, their findings are not easily translatable into projects and these are a critical element of AID's activities. To the contrary, MSU staff have concluded that what is of importance is a strategy for the small-scale enterprise subsector. Such a strategy may suggest changes in macro-economic policies that have an adverse impact on small-scale enterprise, it may prescribe some credit assistance (at unsubsidized rates), it may suggest removal of subsidized competition in large-scale enterprises in both the public and private sectors, and it may indicate the necessity of training programs for management, production, and marketing. They emphasize the necessity to develop such an integrated strategy that spans the procurement of basic inputs to the marketing of final products. This, they emphasize correctly, is not easily translatable into specific projects, and this limits MSU's assistance to AID missions. They are convinced that building consciousness of the importance of the small-scale enterprise sector is essential. Unless both national leaders and AID officials become aware of the magnitude and importance of the sector and begin to try to promote its interests, the possibilities for growth will be limited. It is important to remember that the collaboration with many third world institutions has contributed to this objective.

Finally, on the issue of policy relevance, Michigan State points that they have built a pool of specialists knowledgeable in the field of off-farm employment. Under the cooperative agreement, 12 Ph.D. dissertations have been written. Several of those who have done

research under the agreement are now employed by AID and virtually everyone else has served as a consultant to AID. Thus, it is clear that the cooperative agreement has produced a pool of talent and expertise in this field that can be applied to both policy analysis and change in the area of small-scale enterprise.

It has previously been pointed out that a summary state-of-the-art paper would be a major contribution of the off-farm employment project. Those at MSU who have worked and done research under the cooperative agreement have synthesized what they have learned in ways that are not in print as yet. In their view the concept of off-farm employment has changed from the measurement of that phenomenon as expressed in the original state-of-the-art paper to a focus on the small-scale enterprise subsector. What they started to do—measure the magnitude, composition, and nature of off-farm employment—has generally been accomplished. They have accumulated evidence of the importance and characteristics of off-farm employment and the linkages between it and other sectors of the economy. From this they have focused on the small-scale enterprise subsector as the area with the outstanding potential to generate not only employment but also to contribute greatly to equitable development. They argue for an integrated subsector strategy that follows the small enterprise through all the stages of its activities. Integration should only be in the conceptual sense, they warn, the dis-integration of functions through the development of independent entrepreneurs at various stages of the production process, and the use of subcontracting, can greatly



enhance participation and the benefits of small enterprise development (Mead, 1982, and Mead, n.d.).

The MSU researchers have also begun to consider how the small enterprise sector can best be assisted and are undertaking experiments in this direction under their more recent agreement. They eschew the use of national programs for promotion of small enterprise and look instead to policy reform that will remove the obstacles to small enterprise. They argue for removal of subsidized competition and are planning a regional effort in a selected country that will tailor assistance to small enterprises on the basis of detailed knowledge. This represents a major shift in approach and commensurate progress from the beginnings of the agreement in 1977. It appears also to be just the type of knowledge-building and action-oriented thinking built on a solid ground of empirical study that the cooperative agreement mechanism was designed to achieve.

#### 6. Gaps in Knowledge

As already indicated, AID has awarded Michigan State a new cooperative agreement to extend the work that has already been done. The work under the new agreement will build on the findings that have documented the importance of off-farm employment. Under the new cooperative agreement, MSU will emphasize a strategic approach to small-scale enterprise subsectors that runs from macro policy, to organization, credit programs, and management. The emphasis will be on a broader strategy of promoting small-scale enterprise, since its importance for creating off-farm employment has been well established under the existing agreement.

As indicated earlier, in choosing to pursue the line of activity and inquiry that seemed to have the greatest potential for both development and employment generation, small-scale enterprises and the commercial and service activities associated with them, some choices had to be made. Other areas of off-farm employment that were originally cited have not been included. These are the commercial and the service activities not linked to small enterprise that generate rural non-farm employment. While these are of lesser importance, they are not insignificant. The MSU research does deal with commercial activities in connection with small enterprise, but not as it deals with agricultural marketing or arbitrage between markets as practiced in some areas. The service sector may be important for rural non-farm employment but has not been fully explored under this project.

Presumably the strategic approach to small enterprise development is reflected in other cooperative agreements such as that of MSU on alternative rural development strategies. In the past many have proposed such strategies, but MSU has given us a solid knowledge base on which to posit further action. They have generated a solid systematic knowledge base and have documented the role and importance of off-farm employment, then translated this into approaches for promoting the small-scale enterprise subsector. In doing so, they have shown the potential of a cooperative agreement to generate knowledge about development and to translate that knowledge into resources for specific action steps that can be taken to promote development.

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REVIEW EPERFORMANCE MANAGEMENT

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1. Overview of the Cooperative Agreement

The National Association of Schools of Public Affairs and Administration (NASPAA) and the Development Project Management Center (DPMC) of the U.S. Department of Agriculture have jointly held a cooperative agreement on project management effectiveness. This agreement was initiated in 1979 and ended in 1982. It was succeeded by a project on performance management that is scheduled to run from 1982 to 1988. Each organization has acted independently under the cooperative agreements. Neither was a subcontractor and there was no formal mechanism for coordination. Instead, each pursued the topics independently according to the comparative advantage of the institution. This is not intended to imply that they did not share work and ideas. Each was continually attuned to the work of the other organization. However, each operated independently in pursuing the objectives of the cooperative agreements.

Because of their location in Washington, D.C., both organizations have close ties to AID and have been closely associated with current priorities of this donor agency; both have attempted to find ways to assist AID in meeting its policy mandates and as a result they have been responsive to its operational needs. Because of the opera-

tional nature of both organizations, and because of the nature of the topics, their focus in implementing the agreements has been on improvement in operations rather than research in more traditional sense. They have produced only one state-of-the-art paper, Implementing Development Programs (DPMC, 1979), which appears to have been a final report for a previous project but also serves as a useful starting point for the first of the two current projects. The work of both has included conferences and meetings, the publication of bibliographies, and a strong focus on making information and current technique readily available to practitioners.

## 2. Assumptions

Both cooperating agencies accept projects as a basic operating mode for development. Projects are of course planned, financed, and implemented by organizations, so NASPAA and DPMC look carefully at the functioning of organizations in the development context. Yet projects and ways in which their operation can be improved receive central attention. The existence of established organizations and the centrality of projects is implied in the title of the agreements. Unlike the projects on decentralization, participation, or off-farm employment, these two NASPAA-DPMC cooperative agreements put less emphasis exploring new concepts. They are instead directed to increasing the effectiveness of existing technique. In that, the NASPAA-DPMC undertakings are similar to the agricultural credit agreement, except that they are not limited to a specific sector or type of program. In addition, within these cooperative agreements,

there is no serious look at policy issues because this is generally not an area in which AID has been involved. Other functions such as research or delivery systems receive only limited attention.

Similarly, there is no real challenge to the assumption of existing organizational structures. DPMC looks at ways in which the functioning of given organizations could be strengthened or enhanced by the introduction of new technologies, while the NASPAA papers, especially those of Korten, look at the need, prospects, and techniques for internal reform. Neither gives serious thought to substantial change in organizations, or to more radical structural change. In addition, it is apparent, especially in the work of the DPMC group, that the projects generally employ a systems approach to issues of development project management. They see the problem in holistic terms and a systems approach characterizes much of their work.

Finally, both organizations under the successive cooperative agreements accept and address themselves to ways of successfully carrying out the new directions mandate of Congress. Specifically, both assume the focus is on the poor and ways to reach the poor and to redress income inequality. They also generally adopt a rural focus and consider organizations and projects seeking to reach the rural areas and the poor in those areas.

The work under these cooperative agreements also specifically involves the role of AID. For example, David Korten's work, "People-Centered Planning: The USAID/Philippines Experience," is specifically

an examination of the process used by an AID country mission to develop a new approach to preparing its Country Development Strategy Statement. Although this is unusual, the work of both NASPAAA and DFMC do take explicit account of the existence and role of AID. This is, of course, consistent with the explicitly operational nature of these cooperative agreements.

### 3. Major Hypotheses and Findings

It is important at this juncture to treat the work of NASPAA and DFMC separately for the remainder of the review. While both organizations address similar problems, their approaches are different and each has a different and unique contribution to make.

The NASPAA contribution begins with the work of Korten and Uphoff (1981), which sets the intellectual course for much of the subsequent work of NASPAA. Korten and Uphoff argue that public organizations are an essential part of development programs. However, their record of performance in generating participation and effectively reaching and serving the poor is not good. Too often, Korten and Uphoff note, we "blame the victim" for this. The failure of programs and organizations working at the local level is generally attributed to the limits of the intended beneficiaries who are seen as ignorant, unmotivated, or lazy, while in fact it is the organizations that simply do not provide the services or problem solving capability that is responsive to local needs. The authors note that often such organized efforts make the problem worse by creating dependency. Simply put, they argue that most development organizations trying to



reach the poor are part of the problem rather than part of the solution. They also point out that most such organizations are the product of the program designers and not those who must work with these organizations.

To deal with this situation, the authors argue that the attitudes and behavior of the poor cannot be changed until those of the government staff with whom they must work are changed. Therefore, they "propose that donor agencies and third world governments seriously concerned with improving rural development performance engage in and provide support for experimentation leading toward the bureaucratic reorientation (BRO) of those agencies responsible for poverty-focused development action" (Korten and Uphoff, 1981:4-5).

Bureaucratic reorientation is a concept that runs through much of the NASPAA work and therefore deserves some elaboration. The authors point out a series of "inertial values" or presumptions that must be overcome. One presumption, for example, is that projects are equivalent to development. Korten and Uphoff give a vote of confidence to the personnel of development agencies by asserting that the poor performance of agencies is a result of "...bureaucratic systems that treat creative behavior as dysfunctional rather than a reflection of the inherent qualities of their personnel" (Korten and Uphoff, 1981:14). The authors then propose a "learning process approach to BRO." Proposed elements of this model are a central working group chaired by a high level official that directs the process, field based learning laboratories, process documentation, and plans and methods

that are revised on the basis of the type of intervention best suited to building community capacity. This process of learning pragmatically from the community has three stages: (1) learning to be effective; (2) learning to be efficient; and (3) learning to expand. The authors go on to identify key elements in organizational functioning that must undergo reorientation, such as strategic management, personnel procedures, or the reward structure. By following such a process, the authors believe organizations can become effective at assisting and promoting development for the rural poor. It is worth noting in passing that in the literature on field organizational behavior there has been a great deal of attention to what makes organizations successful. Something similar to Korten's "learning process approach" seems to be common among many successful organizations of very different types and in many different settings.

The approach is further documented and reinforced by Korten's paper, "The Working Group as a Mechanism for Managing Bureaucratic Reorientation: Experience from the Philippines." This paper takes the Philippine National Irrigation Administration as a case in how bureaucratic reorientation can be accomplished through a learning process. The case study provides much of the empirical base in support of the concepts of learning process and bureaucratic reorientation.

A paper by Korten and Carner, "People-Centered Planning: The USAID/Philippines Experience," suggests a rather different case of bureaucratic reorientation or learning process. They do not use the

terms in this case, even though the previous paper suggests that BRO and learning process are approaches for aid donor organizations as well as national development agencies. This paper reports on the methodology used by the AID mission in the Philippines to prepare its Country Development Strategy Statement. Starting with an AID country strategy focused on the development and growth of the modern economic sector, the CDSS was reoriented to focus on redressing the deficiencies in basic needs of the poor. The change in strategy took place within the AID organization and the paper shows how such a reorientation can be managed.

A third paper by Korten, "Learning from USAID Field Experience: Institutional Development and the Dynamics of the Project Process," addresses itself specifically to AID's current priority of institutional development, applying the learning process and bureaucratic reorientation approaches to the agency's goals. The paper distinguishes the learning process from a blueprint approach. It looks at agencies in Thailand and the Philippines to suggest how AID experience in supporting creative local institutions in a learning process approach to bureaucratic reorientation can produce more effective development results. Korten further argues that large amounts of money are not needed if this approach is employed. In this view, "the power of highly flexible money backed by a strong professional staff and a sense of strategy is enormous" (Korten, 1983:22). Thus, NASPAA and Korten have introduced the terms of learning process approach and bureaucratic reorientation, and hav

argued that these provide AID with a powerful instrument for promoting development. Both terms have remained poorly conceptualized, however.

In support of the perspectives of Korten and others, NASPAA has also held a Workshop on Social Development Management. The workshop proceedings dealt in detail with the ideas of learning process and bureaucratic reorientation and applied them to specific areas such as training in educational institutions. The role of aid donors in stimulating these processes was also discussed. NASPAA also published a useful annotated bibliography on social development management.

One other publication by NASPAA deserves mention. This is David Pyle's "From Project to Program: Structural Constraints Associated with Expansion" (1982). This paper, which is adapted from Pyle's thesis and an article published in Grindle, Politics and Policy Implementation in the Third World (1980), provides an interesting analysis of the problems associated with expanding a pilot project to a full-scale development program. Using a community health project in Maharashtra State in India, Pyle concludes that small-scale projects are generically different from large ones. He documents the inherent difficulties of expanding a small or pilot project to a large-scale development project. Pyle's work provides important guidelines for those who want to test project ideas on a small-scale before undertaking a full-scale program and his cautionary analysis provides a useful set of guidelines to project managers.

DPMC's work begins, as previously pointed out, with the same assumption of the relevance of existing development organizations. Its approach is one of complementing incremental change rather than stimulating "reorientation." The organization tends to adopt a systems perspective. The work of DPMC can be broadly divided into three categories. First, it produced a state-of-the-art review entitled Implementing Development Programs (1979) that represented the final report of a previous project and a starting point for the first of the two current cooperative agreements. Second, a series of papers that focus on specific techniques for program and project management have been produced. Third, the organization has produced work exploring a new conceptual approach to project management featuring the "Guidance System Improvement Approach." This latter accomplishment has been the intellectual centerpiece of DPMC's work. We will treat each of these three work products in turn.

Implementing Development Programs: A State-of-the-Art Review (Ingle, 1979) is a useful starting point for the work of the organization. The paper begins with a review of the literature, dividing it into two views of program implementation: (1) "compliance" and (2) the "politics of administration." Ingle suggests that the second is more applicable to conditions in the third world, and he explores a variety of environmental or contextual factors that influence the implementation of projects. Ingle goes on to identify six approaches to implementation and six strategies for managing implementation. He then turns to issues of the implementation of donor-assisted projects

and the factors that influence performance in these projects under various conditions. These tasks are accomplished through a clear synthesis of established knowledge and uncertainties for future exploration. He emphasizes the complexity, relativity of findings, and ambiguity across location and activity that characterizes implementation activities. He concludes with future directions for research. In his view, successful projects should be examined to see what can be inferred from the administrative and contextual environment. Folk management strategies should be studied to learn more about the environment in which third world management innovation will prosper. Reward structures should be altered to encourage innovative responses to program implementation opportunities and problems, rather than simply rewarding project planners and the financial decision makers. He contends that current models and approaches to program implementation lack relevance to operational personnel, so emphasis should be placed on simplifying and adapting existing approaches, a task in which "LDC institutions and personnel can be effective partners."

The work that is focused on technique suggests a number of new approaches, or ways in which new or improved techniques might be used to promote better management of implementation. Kettering's Improving Financial and Program Management (1982) was prepared in response to the needs of AID missions in the Sahel region of West Africa. It represents a practical manual for using good financial management practices in AID-supported development programs in the region. A

second paper in this category is a workshop report entitled Micro-computers and Agricultural Management in Developing Countries (Proceedings, 1982). This examines the experiences of several developing countries with microcomputers and provides a series of recommendations including: (1) the creation of a U.S. government unit to support and link microcomputer users in third world countries with users and suppliers in the U.S.; (2) more research on the unique applications of microcomputers in third world countries; (3) the creation of a donor policy on information and microprocessing technology; and (4) public and private support through technical assistance and training in microcomputer applications in the third world.

Training is the focus of two additional papers. The first is An Action-Training Strategy for Project Management and the second A Suggested Framework for Training in Developing Countries—Accelerated Learning Systems. These two papers focus on training and ways to improve its relevance in order to improve program implementation performance. Each of these focuses on ways to accelerate training and to build into it a feedback process that enriches the training process and evaluates the relevance of the training to actual tasks the trainee subsequently carries out.

Robert Herr's paper, Project Analysis: Toward an Integrated Methodology, is an attempt to incorporate noneconomic factors in project analysis techniques. The focus on rural development and poverty in the 1970s and early 1980s made this an important task.

Five broad categories of project analysis are identified. Emphasis is placed on seeing projects, and therefore project analysis, as a dynamic rather than a static process. The paper emphasizes that it is a first step toward examining project analysis techniques and considering the relevance of each type of analysis to the more demanding requirements of projects that have multiple goods, both economic and social.

The third category, a more conceptual approach to project management is reflected in four papers. The first, The Organization and Conceptual Approach of the Development Project Management Center, is an initial statement of the purposes and intellectual approaches of DPMC. The second is the report of a workshop entitled Promising Approaches to Project Management Improvement. This paper reflects the systems outlook of the core staff of DPMC. It emphasizes the crucial roles of all organizations to successful implementation. This approach really comes to fruition in two papers in the spring of 1983 on the guidance system improvement approach (GSIA). There are two key characteristics of GSIA, "it consists of a core of components (activities) which adopters can mix and match to fit local needs" and "the approach capitalizes on its divisibility by relying upon an incremental, partialization mode of implementation." Its presentation here is difficult to summarize briefly because, as the authors state, "because it is flexible, a highly specific description of the GSIA is neither feasible nor desirable" (Mandell and Bozeman, 1983:1-2). It is, however, an iterative systems model. It is supported by "an



integrated applied research system which we label a performance-sensing and learning user support system (PLUSS)" (Mandell and Bozeman, 1983:4). The GSIA is offered as an alternative to traditional monitoring and evaluation systems. The approach is similar to Korten's learning process approach and is even labeled an "action learning process" because it emphasizes the opportunities of individuals who experience frustration within a traditional bureaucracy to try new modes of operation and techniques that are untested. It appears to vary from Korten in that it emphasizes the individual rather than the group within the organization as the unit of action. While an innovative approach, it needs further testing and operationalization. The emphasis on a new approach is counter to Ingle's earlier conclusion that "current models and approaches to program implementation lack relevance to operational personnel, so emphasis should be placed on simplifying and adapting existing approaches" (Ingle, 1979:76).

#### 4. Methodology

Much of the work of both NASPAA and DPMC is based on philosophical approaches and observation of the development process. The NASPAA work, particularly that of Korten, is field-based, but not specifically empirical. In both cases, the number of countries in which the work has been based is very limited. It is not clear from the reports whether the new approaches have been tried outside the Philippines in Korten's case and Jamaica in DPMC's case. NASPAA has used Pyle's work based in India which is substantially grounded in

field observation, and Korten takes examples from Thailand and the Philippines. What is less clear is whether the work of either group has been seriously tested and proven successful in applied country situations.

The work of NASPAA is significant in that they, more than is the case in most cooperative agreements, have linked themselves very closely with AID mission operations. Korten's location in the Philippines, and his close work with the AID mission there have made his work particularly relevant to field operations. His current assignment working with AID Indonesia and as a secretary to the AID regional committee studying management in development suggests that his work may be particularly relevant to mission operations and will exert influence over future management of development programs in the Asia region.

##### 5. Programmatic Implications

Korten's work for NASPAA has been influential in evoking interest in bureaucratic reorientation and the learning process approach. These two terms have become integrated into the language of development. For example, both terms are used in the 1983 World Bank World Development Report, the theme of which is the management of development. While there are questions to be raised about Korten's formulation of how the BRO and a learning process take place, the ideas themselves have generated much discussion. Despite the problems of methodological approach that Korten uses; there can be no doubt of the relevance of the ideas, or the speed with which they have been disseminated throughout the development community.

DPMC's work is also highly operational. It suggests a different way to approach the project design and implementation process. Although it may be used by some missions, it has not had the widespread acceptance that Korten's work has. Its complexity suggests that as yet it may only be fully utilized in situations where the DPMC staff can assist in its implementation. Clearly, they are working toward a future stage in which the GSIA will be much more broadly known and used. On the technique side of their work, DPMC's efforts are highly operational. Their manual on financial management in the Sahel region or their work on microcomputers is highly relevant to current development activities. However, it is difficult to know the extent of that influence outside of these specific country applications.

#### 6. Gaps in Knowledge

The question of gaps in knowledge is not really relevant to the cooperative agreement on improving project management. Neither NASPAA nor DPMC has undertaken a comprehensive review of the subject of development project management. Each set out to develop new techniques to redress known problems or deal with new objectives in development programs. Ingle's 1979 state-of-the-art paper is a good statement of the state of project literature, but DPMC did not specifically build its work on that paper. The question that must be asked, therefore, is what are the next steps to be taken to build on the work of NASPAA and DPMC?

What seems most urgent is testing and adapting the concepts developed and making them easily applicable in third world countries. NASPAA and Korten are taking steps to do this in Asia. It would be useful to see what needs to be done to apply them elsewhere. Are modifications necessary in Africa or Latin America? Likewise, is DPMC's guidance system improvement approach practical for extensive and universal application without dependence on outside consultants? If so, it should be more widely used; if not, it should be tested and adapted for more widespread dissemination. Both NASPAA and DPMC have introduced new concepts and approaches into the knowledge of development. What is needed is their testing and adoption based on field testing and empirical evidence.

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REVIEW FRURAL FINANCIAL MARKETS

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1. Overview of the Cooperative Agreement

The Department of Economics of the Ohio State University began this four-year cooperative agreement in 1977. Its objectives were to combine applied research and consulting on selected topics related to rural financial markets. The base level of financing was \$1.7 million. With add-ons from individual missions it increased to \$2.1 million. The agreement was extended for eighteen months with no additional funding. Because of the broad coverage of the AID Spring Review of Rural Credit in the early 1970s, the focus of the work in the cooperative agreement was not on state-of-the-art papers. Rather, it consisted of identifying and conducting research on: specific rural financial market problems; providing short-term consultancy services to AID missions; producing a series of annotated bibliographies in the subject area; and conducting a number of workshops on selected topics. The most notable workshop was the Colloquium on Rural Finance at the World Bank in September 1981.

While a considerable amount of the research was undertaken by scholars at Ohio State, subcontracts with Arizona State and Syracuse University added significantly to the overall project. In particular, they increased the range of issues analyzed and the number of

countries considered. With respect to the latter, the project dealt with rural financial issues in Sri Lanka, India, Sierra Leone, Peru, Bolivia, Honduras, and Jamaica. All of this effort has generated a large collection of literature, some of which is reviewed below and some of which is still in preparation. The cooperative agreement also provided support for the work of more than two dozen graduate students (cf. Vogel, 1982; Ladman, 1982) to whom AID may be able to turn for future work on issues related to rural financial markets. In addition, the project communicated the advantages of financial liberalization through numerous workshops and discussion groups and gave some assistance to selected AID missions on how to deal with problems of rural financial markets in specific countries.

## 2. Central Assumptions and Theoretical Framework

The research begins with the assertion that "...most of the received wisdom on rural financial markets in low income countries must be discarded if this vital tool is to play a positive role in development" (Adams, 1977:Preface). The received wisdom that must be discarded is that (1) credit is a major constraint to agricultural development; (2) the existing arrangements in rural financial markets do not effectively supply credit to a broad range of agricultural producers; and (3) government credit programs can not only supply adequate levels of credit to rural areas but with appropriate interventions can direct this credit to selected groups for specific purposes.



These notions are challenged by comparing the consequences of the received wisdom with those of the "new view" of rural financial markets. The latter rests on four propositions: (1) access to credit is only one of several constraints to agricultural development in low income countries; (2) government interventions to direct the volume, terms, and uses of agricultural credit "are largely ineffective, inefficient, and almost impossible to enforce" (Adams, 1977:41); (3) government credit policy to promote agricultural development should emphasize the "liberalization" of rural financial markets; and (4) a basic component of such a policy is the maintenance of flexible positive real interest rates.

None of these propositions is original. They all derive from the long debates in monetary theory and policy literature which resulted in so-called "neoclassical synthesis," the basic theoretical framework for the analysis conducted in the project. Nevertheless, an important feature of the "new view" which should not be overlooked is that it represents one of the first attempts to consider rural financial markets in poor countries from a broad neoclassical perspective (as distinct from an approach that is Keynesian, Marxian, or monetarist). For example, proposition (2) recognizes that credit has interchangeable uses (in neoclassical terms, it has a high degree of substitutability) so that any assessment of its impact cannot be narrowly focused. Similarly, proposition (3) highlights the importance of achieving wide-ranging changes in the way rural financial markets are organized and regulated. The basic consequence of using

such a well-established analytic framework is that the project did not make any major contributions to new knowledge; rather, it increased the weight of evidence about the advantages of a program aimed at liberalizing rural financial markets (in the neoclassical sense of letting market mechanisms determine the relative prices of credit, its allocation, and its impact on income distribution).

### 3. Research Findings

Bouman (1981:1) summarizes one of the general conclusions of the project when he notes that "there is a growing consensus among researchers that the formal financial sector is not coping with many of the needs of the rural populace in the third world." Specifically, a major proposition emerging from this growing consensus is that government policies toward rural financial markets in many low-income countries are "...resulting in very inefficient allocation of resources, seriously discouraging the capital formation process, and causing major concentrations in income and asset ownership" (Adams, 1977:1). These are reflected in several problems which are common to rural financial markets in poor countries. Adams (1977:5-7) lists nine such problems: (1) capital erosion of formal financial institutions; (2) loan delinquency or default; (3) institutional resistance to agricultural lending; (4) little formal sector lending to the rural poor; (5) few medium- and long-term loans; (6) ineffective mobilization of financial savings in the rural areas; (7) large lender and borrower transactions costs; (8) little financial intermediation because of rural financial market fragmentation; and (9) the increasing concentration of assets and ownership.

The pervasiveness of these problems is evidence that the conventional approaches to agricultural credit programs (or more broadly, rural financial policies) have not achieved their stated aims of increasing agricultural output, expanding agricultural productive capacity, and assisting the rural poor (Adams, 1977:1; Kane, 1981:16; Vogel, 1981:1). It is one of the most important contributions of the project to show that these policies have not failed because of problems such as "the poor do not save," or "moneylenders are exploitative," or "small farmers do not use credit wisely." They have failed because the policies themselves are faulty (Adams, 1977:41). Among the many reasons why this is so, three stand out: (1) government intervention in rural financial markets creates adverse incentives; (2) credit is fungible, i.e., has interchangeable uses; and (3) credit allocation policies threaten the financial viability of the institutions involved.

Referring to the issue of adverse incentives created by conventional rural credit policies, Kane (1981:1) summarizes the general point well when he states: "The long-run consequences of development-promoting credit-allocation policies invariably run counter to their ostensible goals." This situation arises because none of the individuals or institutions that are subject to the credit-allocation policies has an incentive to behave in a manner that is consistent with the ostensible goals. To illustrate, Adams (1981) argues that "cheap agricultural credit" policies induces an excess demand for credit by borrowers and a deficient supply from lenders,

both of which lead to predictable distortions in rural financial markets such as credit rationing and low levels of savings mobilization. In the process, agricultural development is not promoted nor are small farmers assisted. Gonzales-Vega (1981a,b,c) makes the same point using the iron law of interest rate restrictions. This law, which is an assertion about economic behavior under rationing, holds that when lenders are forced to extend credit at interest rates which incur positive opportunity costs for them, their portfolios will systematically shift toward larger, safer loans that have lower associated lending costs. The more severe the interest rate restrictions, the more it reduces the access of small, risky borrowers to the market for cheap credit. This, in turn, reduces their access to productive factors and decreases the growth in their incomes (Gonzalez-Vega, 1981c:5-12).

Ladman (1981) examines the (explicit and implicit) borrowing and lending costs of the "credit delivery system." Finding that these costs are uniformly high and that they vary systematically across different classes of borrowers and lenders, he concludes that "borrower behavior converges with lender behavior to determine the structure of the market" (p.18). That is, because of transactions costs, particular lenders do not lend to specific groups of borrowers, and vice versa. Stated in more familiar terms, this is the well-known proposition that transactions costs fragment financial markets.

Ray (1981) considers the notion of optimal government intervention in the context of financial markets. One adverse incentive he

notes is an "inherent bias in favor of controls and regulations over market solutions to economic problems..."(p.20). In his view, the most serious flaw with government intervention is the presumption that governments know how to allocate resources when the market is not providing the right signals (p.21).

Finally, case studies from Honduras (Cuevas and Graham, 1982; Graham and Cuevas, in Graham et al., 1981) and Jamaica (Pollard and Graham, 1982a,b) provide data on several adverse incentives in rural markets. For instance, one result from the studies on Honduras is that both borrowing and lending costs are inversely related to loan size, meaning that small borrowers (typically the poor) are excluded from institutional sources of credit that have high lending costs. The studies on Jamaica conclude that food pricing policies during the 1970s had a significant impact on the structure of agricultural production. Based on the premise that "prices make a difference," Pollard and Graham show that farmers had rationally responded to the adverse incentives associated with export taxes and low domestic producer prices. One implication of this analysis, which is also mentioned elsewhere (cf. Adams, 1977:43; Adams and Graham, 1981:357; Bourne and Graham, 1981:7,27; Von Pischke, 1981:16; Ray, 1981:22-23; Tinnermeier, p.63, and Ladman and Stringer, p.65 in Graham et al., 1982), is that reform of rural credit markets alone is not an adequate policy for promoting agricultural development; other policy reforms have to be undertaken as well.

Perhaps no other research project on financial markets has given so much emphasis to the fungibility of finance. Since credit provides access to generalized purchasing power, borrowers cannot be prevented from diverting credit obtained for one purpose to some other (for example to use a fertilizer loan to buy cattle or clothes). That is, lenders can never be sure of the true purpose of any loans they provide (Kane, 1981:4). As Von Pischke and Adams (1980:720) note: "For all practical purposes, loans in cash or kind can be used to buy any good or service available to the borrower in the market." For their part, lenders can also interchange the use of their resources. For instance, they can redefine the purpose of the loan, make multiple small loans to the same client, and ration particular groups of borrowers (Kane, 1981:10-13; Von Pischke and Adams, 1980:724; Gonzales-Vega, 1981b). Thus, because finance is fungible, both borrowers and lenders can circumvent most of the controls and regulations which governments impose on rural credit markets.

Reflecting this, Adams (1977:41) suggests that governments abandon the administrative allocation of finance. As he notes, "these fiats are largely ineffective, inefficient, and almost impossible to enforce." Adams and Graham (1981:355) provide another perspective when they note that a serious objection to government credit allocation programs is that they are attempts to destroy the fungibility of credit. Specifically, they argue that: "This planning approach to the allocation of loans assumes that a borrower knows not what is best for him or her, that loans can be allocated like physical inputs, and

that the planner in the capital city can effectively make efficiency and equity decisions for thousands of heterogeneous borrowers."

In effect, the project makes two points about fungibility. The first is that because of fungibility, the allocation of credit is not an efficient or effective way of achieving improvements in agricultural productivity and rural welfare. The second is that where governments succeed in planning the allocation of credit, they undermine one of the useful properties of credit, namely, that it can (and should) be directed to uses that provide the highest possible return. Interpreted either way, these are important reasons for reducing government intervention in rural financial markets.

In passing, it is worth noting that fungibility results in major evaluation problems. Since the use of credit cannot be easily directed, its impact on rural areas cannot be easily evaluated (Von Pischke and Adams, 1980). With current assessment methods, there is little that can be said about the impact of credit on the activities of small farmers. Indeed, the only predictable effect is that credit eases a farmer's budget constraints. Perhaps the most significant result on this question that emerged from the project is the evidence of Cuevas and Graham (in Graham, et al., 1981) from Honduras that shows that an important source of informal credit for small farmers is larger farmers who receive agricultural loans from formal financial institutions. This practice is widespread--over 90 percent of formal sector borrowers engage in this on-lending, indicating both that credit is highly fungible and that "increased liquidity in the rural economy does work its way through the system" (p.36).

The problem of the financial viability of formal institutions in rural financial markets is directly related to adverse incentives in at least two ways. First, few incentives--moral, political, legal, or economic--exist for borrowers to repay their loans. Consequently, loan delinquency and defaults erode the real value of the portfolios of these institutions. Second, their supply of funds is unreliable and costly to administer since it comes primarily from the World Bank and other international agencies, central bank rediscounts, and direct government grants. Squeezed from both sides, most formal sector agricultural institutions in low-income countries are not financially viable.

However, some institutions, such as private commercial banks, which place a high priority on maintaining their financial viability, display predictable responses to government intervention in rural financial markets. For instance, Gonzalez-Vega's iron law of interest rate restrictions is a description of how institutions that are constrained to lend at low interest rates modify their portfolios to minimize the threat of the constraint to their financial viability. The behavior of individual portfolio-holders can be interpreted in the same way. When alternative investment opportunities are available, for instance in land, livestock, or stocks of commodities, individuals will accept low returns on savings deposits and expose themselves to financial risks. Thus, savings mobilization in low-income countries remains low.



Several papers deal with financial viability at length. Adams (1981:9) notes that a basic issue in credit programs is whether formal sector lenders receive enough revenues to cover their costs. Somewhat more provocatively, Vogel (1981) argues that savings mobilization is the "forgotten half of rural finance." He recommends that if financial institutions are to mobilize savings, they should substantially alter terms and conditions to include such things as a positive real interest rate on deposits, convenient locations for deposits and withdrawals, and prompt service. In addition to improving rural income distribution, improving resource allocation, and imposing a degree of financial discipline, such efforts to mobilize local savings would, as Vogel points out, give the local community more of a stake in the viability of the institutions involved. That such a stake is generally lacking is evident in the paper of Bourne and Graham (1981) who criticize the notion of supply-leading finance for the way it has ignored the problem of achieving some autonomous growth in the supply of funds (p.19). As Bouman (1981) notes, ensuring each participant of a stake in the process has been one of the basic elements in the success of the informal savings arrangements used by the poor.

The review of the financial system in Honduras (Graham et al., 1981) demonstrates the problems of preserving the financial viability of institutional sources of agricultural credit that have a major effect on the structure of the financial system. For instance, once the Banco Nacional de Desarrollo Agrícola (BANADESA) was reconstituted with all of the accounts and the portfolio of its defunct predecessor

(BANAFOM) (Gonzalez-Vega, in Graham et al., 1981), it soon began to experience the same problems of loan delinquency. Cuevas and Graham (in Graham et al., 1981) analyze this issue in detail, examining its extent (high), its pattern over time (increasing), its consequences (viability of the institution), and possible policy responses (cautious lending and more effective loan recovery). They urge further study of the problem, arguing that "too much is at stake in terms of institutional viability and the social costs of implicit subsidies, and income transfers are too high to be ignored" (p.29). For Banadesa, the problem of loan delinquency is compounded since it is obliged to extend credit to the (land) "reformed groups" (Ladman and Stringer, in Graham et al., 1981). These loans involve high lending costs (p.49), and because of the problems associated with poor land, flooding, and organization, the groups have a low repayment capacity. Delinquency rates exceed 35 percent (p.40).

As a final example, the evidence assembled by Poyo (in Graham et al., 1981) shows how aggressive savings mobilization helps preserve financial viability. After examining the portfolios of 18 credit unions in Honduras, he notes that the only institutions that were maintaining the real value of their portfolios were those with incentives specifically designed to attract savings. While most of the credit unions are stagnating because they are not adjusting their policies to deal with accelerating inflation, some are continuing to expand by raising the real interest rate on their deposits as well as the real rates on their loans. The lesson from this--and one of the

main lessons of the overall project--is that realistic interest rate policies ensure the continued viability of financial institutions without deterring borrowers.

#### 4. Programmatic Implications

Four implications for future programs in rural financial markets in low-income countries emerge from the research: (1) concessionary credit has not and is not likely to promote agricultural development; (2) supplying credit to the rural sector is only one of many policy issues; (3) government intervention should encourage the development of rural financial markets, an effort that external agencies can directly support; and (4) there will continue to be a need to increase the supply of credit to both farm and nonfarm enterprises in rural areas.

The use of concessionary credit to promote rural development does not normally succeed since credit is fungible (Von Pischke and Adams, 1980). Hence, there is only a low probability that the use of credit can be determined by administrative fiat (Adams, 1977:14; Kane, 1981:4; Adams and Graham, 1981:355). Furthermore, government restrictions that attempt to override the internal portfolio considerations of lenders by forcing them to provide credit at low interest rates to help small farmers are both inefficient and ineffective (Adams, 1977:29; Kane, 1981:11; Gonzalez-Vega, 1981a:4). The majority of low interest rate credit is allocated to large farmers and the borrowing costs that small farmers incur in their attempts to obtain this credit make it just as expensive as informal sector credit (Adams, 1981:1;

Ladman, 1981:18; Adams and Graham, 1981:350; Graham and Cuevas, 1982). Thus, concessionary credit does little to increase the productivity of small farmers, expand their agricultural output, or increase their incomes.

The closest any of the researchers came to suggesting that credit had a key role in agricultural development was Gonzalez-Vega (1981c:12) who concluded that "a key mechanism for influencing the distribution of wealth through time is access to credit." Overall, the project makes a deliberate attempt to de-emphasize the role of credit, something that differentiates the new view of rural financial markets from the received wisdom of supply-leading finance. Ladman's (1981) work is instrumental in this effort. For instance, based on an extensive review of borrowing and lending costs in rural financial markets, he argues that to improve the impact of credit in the rural areas, the whole credit delivery system has to be restructured.

Adams and Graham (1981:354) continue this focus when they note that for borrowers, the interest rate is only one of many considerations; others include the timeliness of the loan, the flexibility of repayment, and the potential availability of additional loans. In addition, several authors expand on the point by arguing that in general it is inappropriate to consider credit as a primary constraint to agricultural development (Tinnermeier, p.63, and Ladman and Stringer, p.65 in Graham, et al., 1981). As Tinnermeier notes, "Other essential ingredients like profitable technology, markets, organizations, and minimum human capital and skills may be needed before or at least along with credit."

The recommendation that government policies in low-income countries should encourage financial development has an intellectual history dating at least from the work of Gurley and Shaw (1955;1967), Goldsmith (1969), and later Shaw (1973), and McKinnon (1973). The project supports this recommendation in several ways. For instance, Kane (1981:18) suggests that official policies toward rural financial markets should "work with rather than against market forces" (italics in original). Similarly, Bouman (1981:24) argues that government attempts to promote financial intermediation in the rural areas should build on rather than destroy existing informal arrangements. Kilby, Liedholm, and Meyer (1981:25) provide a slightly different perspective when they note that even if the government liberalizes financial markets by implementing a high positive real interest rate policy, there is still a need for ways of improving the information available to lenders and screening potential borrowers. This is consistent with Ladman's (1981) conclusion that significant improvements in rural financial markets will occur when transaction costs are reduced.

According to Ray (1981:30) external funding agencies have an important role in promoting financial development if they foster "the decontrol and/or creation of viable and efficient markets." One way is through technical assistance to the financial institutions that emphasize savings mobilization. The AID-sponsored BANCOOP project in Peru is an example (Vogel, 1981, 1982). This approach would reorient the activities of the external agencies from their present concern of trying to improve the ability of the poor to save, to one of providing

incentives for the poor to save in financial forms (Desai, 1981:17). Another way for external agencies to help promote financial development is for them to reduce the costs their procedures impose on local financial institutions that administer their funds. As Graham and Cuevas (1982:7) show, the cost of administering the funds external agencies provide for small farmer credit is 7-8 percent—almost double the cost of administering funds that are mobilized locally.

That there is a need to increase the supply of credit to the rural areas is the only major point of the received wisdom that is not challenged by the new view. For example, the Honduras study recommends that a sustained effort be made to increase the supply of rural credit (cf. Graham et al., 1981, "Conclusions and Recommendations," pp.9-12; Gonzalez-Vega, in Graham et al., 1981; and Cuevas and Vogel, 1983). This general view is repeated throughout the project. Indeed, the many suggestions regarding the promotion of financial development can be interpreted as ways of either increasing the supply of credit at a given level of borrowing or decreasing the costs of lending for a given amount of credit. In this respect, the observation by Bourne and Graham (1981:10) that government policies (especially interest rate policies) that destroy financial intermediation should be considered as a social cost is particularly appropriate.

##### 5. Research Methodologies

Two research methods are used in the project. The first is to challenge the received wisdom of supply-leading finance on theoretical grounds. The second is to use evidence from case studies to support

the implications of the neoclassical view of financial market liberalization.

The theoretical challenge is directed both at the assumptions and implications of the received wisdom. To illustrate, a basic assumption of supply-leading finance is that because of the limited development and fragmentation of rural financial markets there is a need for government intervention to provide cheap agricultural credit as a means of increasing agricultural productivity (Bourne and Graham, 1981; Adams, 1981). This assumption also justifies the limited attention given to savings mobilization (Vogel, 1981) and, apart from the government-sponsored credit programs and institutions, the lack of emphasis on the general issue of financial development (Gonzalez-Vega, 1981a,b,c). By contrast, the neoclassical view asserts that it is precisely because of the pervasiveness of government intervention in rural financial markets that these markets are fragmented and have high borrowing and lending costs (Ladman, 1981); that specific groups such as small farmers and rural nonfarm enterprises have limited access to credit (Kilby, Liedholm, and Meyer, 1981); and, more often than not, this intervention leads to the replacement of existing informal credit arrangements and institutions instead of attempts to adapt them or to build upon them (Bouman, 1981:26; Kane, 1981:24).

In essence, the theoretical challenge of the new view is that government intervention both to force the rate of change of rural financial markets and to direct the use of a credit in rural areas actually has the opposite effects—these markets do not develop and

the use of credit becomes highly concentrated among the well-to-do (-Gonzalez-Vega's iron law). To counter these trends, the proposal of the new view (which is derived from the basic propositions of neoclassical theory) is to liberalize rural financial markets by establishing positive real rates of interest, introducing incentives to mobilize savings, and adopting measures that make rural credit policies consistent with overall monetary policy. The theoretical benefits of these actions are to reduce fragmentation of the rural financial markets (Adams and Graham, 1981:351) by lowering transactions costs (Ladman, 1981:24), and to promote financial development. These changes in turn should improve agricultural productivity, raise agricultural output, and redistribute income and assets to the rural poor (Adams, 1977; Gonzalez-Vega, 1981c).

The case studies range from descriptions of specific aspects of rural financial markets to detailed reports that use survey data accompanied by one or more econometric estimates. To illustrate, Bouman (1981) examines informal savings arrangements in Sri Lanka, giving particular attention to the ROSCA type schemes, i.e., rotating saving and credit associations. Vogel (1981, 1982) describes an AID-sponsored savings mobilization effort in Peru. Gonzalez-Vega and Southgate (in Graham et al., 1981) respectively review the general financial situation and agricultural pricing in Honduras. Gonzalez-Vega and Cuevas and Graham (in Graham et al., 1981) consider the structure and operations of BANADESA and its loan delinquency problem. Cuevas and Vogel (1983) expand on Gonzalez-Vega's reviews (in Graham,



et al., 1981) by providing evidence on the contraction in the financial sector of Honduras during the period 1978-1981. Poyo (in Graham et al., 1981) records some observations from a survey of 18 credit unions in Honduras.

More detailed quantitative analyses are contained in the studies by Kilby, Leidholm, and Meyer (1981), Cuevas and Graham (1982), and Ch. VI in Graham et al. (1981), Pollard and Graham (1982a,b) and Ladman and Stringer (in Graham et al., 1981). Kilby et al. use survey data from Sierra Leone to measure the impact of working capital on rural nonfarm enterprises. Cuevas and Graham examine survey data and accounting records of two formal sector institutions in Honduras to determine the significance of transactions costs in lending and borrowing. Their econometric results show that transactions costs and interest charges are direct substitutes (Cuevas and Graham, 1982:11). Ladman and Stringer survey 48 of the "reformed groups" in Honduras to determine their access to credit. They find that a large part of the credit is provided by BANADESA, its uses are not well-known, and much of it will not be repaid.

Finally, in two case studies that are only peripherally related to the main theme of the overall project, Pollard and Graham (1982a,b) examine the impact of agricultural prices in Jamaica on the structure of output and its division between domestic sales and exports. Using econometric evidence for support, they conclude that prices do make a difference to farmer's responses. This finding is consistent with other project results.

## 6. Gaps in Knowledge

Viewed broadly, the project did not add new knowledge. Each of its major propositions--the inefficiencies and ineffectiveness of credit rationing, the critique of supply-leading finance, the importance of positive real interest rates in financial development, and the concern with the total costs of borrowing and lending--has been dealt with in earlier literature. Where the project makes a significant contribution is by: (1) emphasizing the dynamic effects of the adverse incentives in rural financial markets associated with cheap credit policies; and (2) showing how the behavior of borrowers and lenders, which is determined primarily by total borrowing and lending costs, subverts the ostensible goals of these policies (given the political economy of credit, this subversion may have been intentional--a point some papers mention in passing but none emphasizes). In effect, the project provides a different perspective on several familiar themes and by doing so indicates some useful directions for further research in rural financial markets. These directions can be derived by asking the question, how can the policy recommendations emerging from the project be implemented?

When this is done, it becomes clear that the various suggestions throughout the project are incomplete as policy recommendations. The major suggestions concern the desirability of changing the existing government rural financial market policies, the need to reduce both borrowing and lending costs in these markets, and the importance of treating credit as one of many elements in a broader

policy setting. In particular, the basic policy question of who has to do what, when, how, why, with whom, and for whom has only been partially dealt with.

To illustrate, with regard to the desirability of changing the current policies in rural financial markets, Adams, Gonzalez-Vega, and others conclude that low interest rate policies were undesirable because of the adverse effects they have on economic efficiency, savings mobilization, and the income of the poor. (This answers the "why" and "for whom".) To change this situation, they recommend a policy of positive but flexible real interest rates (thereby answering the "what"). But, since none of the studies shows the extent to which economic efficiency is undermined, savings are not being mobilized, and/or the distribution of income is worsened, policymakers have no guides regarding how flexible the positive real interest rate policy should be.

Furthermore, although the research project shows that a positive real interest rate policy mobilizes savings (Vogel, 1981) and negative real interest rates have adverse consequences for financial development (Cuevas and Vogel, 1983), it inadequately deals with the political aspects of implementing a positive real interest rate policy. Adams and Graham (1981:360) recognize this omission. One way of filling this gap in knowledge would be to build upon the discussions of Kane (1981) and Ray (1981) of the political and bureaucratic factors responsible for the persistence of the current agricultural credit programs. In this regard, the observation by Ray (1981:2) that

"one difficult problem in formulating policy recommendations in developing countries is to understand how existing policies evolved and how they constrain the possibilities for reform" would be a useful starting point. Such an exercise would greatly enhance the new view of rural financial markets by showing how the necessary changes in policy can be implemented given the political and bureaucratic pressures that exist in practice.

Another gap in knowledge that the project highlights is the lack of a coherent model of the role of agricultural (or rural) credit in the whole economy. While several authors suggest that rural credit should be seen as one aspect of overall monetary policy, none of them attempts to show how rural credit fits into the whole financial system. Even if such an undertaking is beyond the intentions of any of the researchers, the absence of a broad analytical framework leaves the project with a sense of detachment, as though rural financial markets are something that can be analyzed and about which policy recommendations can be made separately from the rest of the economy. A similar comment applies to the discussion of rural households in Cuevas and Graham (in Graham, et al., 1981:Ch. VI) in which the internal distribution of credit and the concerns of women are not considered.

Taken together, both the micro setting of the household and the macro setting of the whole economy are treated in the project as if they are "black boxes." How rural credit relates to both should be the subject of future research.

As a final comment, there is a paradox associated with the much emphasized fungibility of finance. Since finance is fungible, its impact is both difficult to measure and difficult to discern (Von Pischke and Adams, 1980). Therefore, the question arises as to how those who advocate that rural financial markets should be liberalized can be so sure they will succeed irrespective of the policies adopted in urban financial markets. In this case, the drawbacks of not having an overall framework that places the rural financial markets in the context of the total economic system are serious.

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REVIEW GMANAGING DECENTRALIZATION

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1. Overview of the Cooperative Agreement

Between 1979 and 1982, a group of scholars at the University of California at Berkeley pursued research through a cooperative agreement with AID. The focus of their efforts was the broad concept of managing decentralization, a topic of considerable interest to field practitioners at the time the cooperative agreement was initiated. Decentralization, as a general strategy for program design and implementation, was a popular idea and was thought to hold considerable power to improve the management of rural development efforts. Expectations were therefore high at AID that skilled social scientists would be able to study the complexities of this concept and devise successful methodologies for putting it into practice in a wide variety of settings. In addition, the cooperative agreement was expected to provide insights into factors that would lead to the strengthening of local government institutions, particularly in terms of organization, financial management, and control systems. As with other cooperative agreements, AID was also centrally interested in having access to skilled professionals in the field of development administration who could provide technical assistance to field missions.

The Berkeley research on decentralization produced a number of studies of considerable importance for the academic community. Two edited volumes, representing important state-of-the-art research reports, have been published as part of a special series by the Institute of International Studies at the University of California, Berkeley (see Leonard and Marshall, 1982; and Ralston, Anderson, and Colson, 1983). These are now widely available research products of the cooperative agreement. In addition, project participants engaged in applied research and consulting missions to Kenya, Senegal, Sudan, Uganda, Somalia, Indonesia, the Philippines, Pakistan, Nicaragua, Ghana, and Egypt, producing field reports as appropriate (summarized in Project on Managing Decentralization, (1982). The project also organized a workshop on decentralized modes of agricultural administration held in Kenya in December of 1981. Part of the purpose of the workshop was to generate the involvement of professionals from third world countries in the proceedings (see Project on Managing Decentralization, 1982:Part VI-II).

## 2. Assumptions

The research undertaking at Berkeley was initiated with commitment to the central assumption that decentralization would greatly improve efficiency in rural development management and would result in greater project effectiveness (Leonard, 1982:4). Thus, although both AID and the Berkeley researchers recognized that decentralization was a multi-faceted concept with a wide variety of empirical manifestations and that it was frequently difficult to

implement, they also viewed it as a general strategy that should be promoted; rural development project design, implementation, monitoring, control, maintenance, and evaluation would inevitably all be better accomplished through a decentralized than a centralized management strategy. Interestingly, however, a number of those participating in the decentralization project adopted a critical approach toward the concept and repeatedly scrutinized these assumptions about its benefits and appropriateness to a wide variety of rural development undertakings. An important and valuable outcome to their research is that the conventional wisdom about this popular concept is questioned. While in general affirming that decentralization has much to offer, the Berkeley research identifies types of conditions under which it will neither increase efficiency nor stimulate effectiveness. It also identifies a variety of decentralization strategies that are not likely to be pursued because of the political and organizational constraints typically found in third world countries. Thus, what began as a basic assumption became an empirical question, an appropriate perspective from a social scientific point of view.

The Berkeley effort points out that proponents of decentralization tend to base their advocacy on implicit and explicit assumptions that "individuals and small groups know best their own self interests, that competition between them leads to efficiencies, and that there is some roughly equal initial endowment of capacity" (Cohen, Dyckman, Schoenberger, and Downs, 1981:149). In many third

world countries, these assumptions may be inappropriate if an important goal of government is to establish control over often diverse political and economic interests. Moreover, they argue, for political and administrative elites, competition may appear wasteful of energy and resources. Likewise, it is characteristic of many third world countries that inequalities are extensive and deep. Some decentralization strategies are therefore not appropriate to the economic, political, or administrative conditions in third world countries. Martin Landau and Eva Eagle (1981:11) argue that attempts to decentralize administration often run counter to the centralizing dynamics of modern organizations. Thus, they suggest that there may be an inherent contradiction for third world countries that are simultaneously seeking to modernize and to decentralize.

### 3. Conceptualization

Central to the Berkeley research output is an analysis of the concept of decentralization as it is relevant to rural development efforts. Because it is conceived to be a process rather than a condition, decentralization eludes concrete definition. Instead, Leonard (1982) argues that decentralization must be analyzed in terms of its dimensions. Four questions are specified to generate insight into the dimensions of the concept: "(1) What type of organization is involved at both the intermediate and local level? (2) Are the mediating organizations representative, private, or agencies of the central government? (3) Are the governmental bodies generalist or specialist? (4) Are the representative entities inclusive or alter-

native organizations limited to the poor?" (p.30). There are, following from these questions, eight major types of decentralization: (1) devolution; (2) functional devolution; (3) interest organization; (4) prefectorial deconcentration; (5) ministerial deconcentration; (6) delegation to autonomous agencies; (7) philanthropy; and (8) marketization. These are the general strategies of decentralization that are available to program designers for implementing rural development programs. Critical in the choice of decentralization strategy is an appreciation of two kinds of organizational linkages, those that create control relationships and those that create assistance relationship. According to Leonard, these linkages should be a primary focus for research on decentralization, for in general, the success of decentralization strategies depends upon the creation and maintenance of consistent and effective organizational linkages.

#### 4. Major Hypotheses and Findings

In a broad assessment of this concept, Cohen, Dyckman, Schoenberger, and Downs, (1981) indicate that the enthusiasm for decentralization and assumptions about what it can achieve must also be tempered with a realistic assessment of the kinds of problems it purports to address and the actual causes of those problems. Arguing that "[d]ecentralization must be seen as a process not a direct condition," they are particularly concerned with the political rationales that encourage or discourage the adoption of a variety of decentralization strategies (devolution, deconcentration, delegation to autonomous agencies, delegation to parallel organizations,

privatization, and workers' self-management). Their work suggests that governments may have an "approach-avoidance" complex about decentralization. On the one hand, government leaders recognize its potential to resolve a variety of organizational and economic development problems; on the other hand, they are aware of its potential to threaten their control over the allocation of resources in the society.

David Leonard (1982) is also centrally concerned with the impact of politics on administrative choices. He differentiates among education, public health, public works, and agriculture projects in terms of their susceptibility to elite control and corruption at the local level. Because of the constraints imposed by political realities in third world countries, he argues, administrative and organizational measures will not solve problems whose roots are deeply imbedded in social structures. This proposition receives considerable confirmation in a review of a variety of decentralization efforts in the Philippines, parts of Africa, the Commonwealth Caribbean, India, Thailand, and Latin America. In this review, Ralston, Anderson, and Colson (1983:113) conclude that, "decentralization in weakly organized local units usually leads to further penetration of the central power, which more often than not results in the extraction of what few local resources remain, including the most able among the local leaders. Decentralization usually favors the local elite." Under these conditions, then, decentralization can actually be antithetical to the interests of the rural poor.

The Berkeley research also proposes that decentralization is not a universal panacea for all organizational problems. Identifying a variety of tasks that must be carried out by organizations (programmed decision making, pragmatic decision making, bargaining). Landau and Eagle (1981) argue that distinct organizational formats are appropriate for each type of activity. Thus, programmed decision making relates to decisions that are routine and predictable and that can be incorporated in the decision rules found in standard operating procedures. Centralization and hierarchy may be the most appropriate organizational formats to handle this type of decision making. Decentralization might be much more appropriate for pragmatic decisions that arise because of variable local conditions. Decisions that involve a variety of contending interests and the need for bargaining to achieve a satisfactory outcome also imply decentralization.

In more general terms, Cohen, Dyckman, Schoenberger, and Downs, (1981:39) identify administrative, political, economic development, and value-oriented reasons for decentralization strategies and suggest that it is important "to choose among different types of decentralization depending upon one's analysis of the particular weakness held to result from overcentralization" (p.35)\*

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\*Administrative Reasons: better responsiveness to local conditions; bureaucratic coordination and integration; innovation and adaptability; project maintenance; appropriateness to service-oriented tasks; consumer participation and effective demand making.

Political Reasons: unity and stability; integration and support building; diminishing, strengthening or compromising with the control



Decentralization, they argue, is not the goal of decisions to pursue it; rather, increased efficiency, effectiveness, political control, or cohesion are sought by politicians, policy makers, and implementors. None of these goals can be achieved without altering the political context, for each affects the capacities of different groups (local citizens, state governors or provincial officers, local elites, central bureaucrats, etc.) to influence the allocation of resources.

While indicating these reservations, the research output of the decentralization project also affirms that positive expectations about the impact of decentralization on rural development projects are not misplaced. The success of rural development projects frequently does depend upon the capacity to make timely decisions in the field to respond to the wide variety of local circumstances that affects such projects. Thus, it is important for rural development practitioners to be knowledgeable about where, when, and how to promote decentralization activities. Several of the Berkeley research reports provide precisely this kind of guidance through exploration of important propositions (see Peterson, 1982; Steinmo, 1982). In the edited volume by Leonard and Marshall (1982) that contains both general commentary and specific case studies, David Leonard (1982) outlines the central "working hypotheses" about implementing decentralization

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of local elites over local activities; diminishing conflict at the center.

Economic Development Reasons: spatial distribution of economic activities; "small and local is good;" mobilization of local resources.

Value Reasons: participation; democracy; self reliance.

strategies that both guide and result from the project. He argues that decisions about how to organize the delivery of goods and services to the local level need to be based on an analysis of the nature of the program to be implemented and characteristics of the environment where it is to be implemented. This proposition is logically derived from the critical assessments of the concept of decentralization reviewed in previous pages. According to Leonard, choices need to be based on "(i) the programs' vulnerability to inequality; (ii) the nature of local elites and their interests; (iii) the nature and variability of interests among national agencies; and (iv) the distribution between national and local organizations of the capacity to meet the program's technical and administrative requirements" (p.8).

Each of these factors should be assessed in turn, according to Leonard, but with an understanding that each may affect the other. Vulnerability to inequality can be analyzed by considering the characteristics of the program in question: (1) the extent to which benefits are individual rather than collective; (2) the extent to which demand for benefits exceeds supply; (3) the extent to which there are trade-offs between quantity and quality of benefits; (4) the extent to which benefits depend upon the supply of other (scarce) goods and services; (5) the extent to which beneficiary participation is required; and (6) the extent to which coercion or monopolies of control are necessary to provide benefits. The propositions summarized above indicate that the greater the importance of any or

all of these factors, the more vulnerable will programs be to capture by elites and the greater will be the resulting inequalities.

Similarly, Leonard indicates that an analysis of the interests of local elites is important in selecting a decentralization strategy. "Where local leaders are responsive to the interests of the poor, then inclusive, relatively autonomous forms of local organization and government are to be preferred. Where such responsiveness is missing, limited local organization of the rural poor are needed with strong external support" (p.20).

A third factor to be considered is the availability and strength of support for the program from national political leaders and public agencies. He suggests that an analysis of this issue consider the question, "'Can a national agency be found to administer the program which has a positive commitment to these particular benefits reaching the rural poor?' If not, can such an agency or subordinate unit be created in this political context?" (p.21). To assess these questions, Leonard proposes that the "external alliances and the professional and institutional patterns of socialization" of the agency receive priority attention, for these are the factors most strongly correlated with agency commitment (p.21). Following from this, the less committed the national agency is, the more local autonomy is needed for the program (p.24).

The final factor that needs to be considered in the choice of decentralization strategy, according to Leonard, is the variable capacity of local and national organizations to manage the technical

and administrative requirements of a particular program. Technical features, program scale, level of administrative complexity, need for cooperation from other public agencies and the magnitude of resources required are program attributes that should guide the choice of where, when, and how to pursue decentralization strategies.

Additional propositions about where, when, and how to promote decentralization relate to differences among the rural poor, national governments, and donor agencies. Each operates from a distinct set of constraints that limits commitment to decentralization and the capacity to work for its effective implementation. Awareness of these perceptions and commitments should be an underpinning for selecting decentralization strategies. Leonard and Marshall (1982) set out a variety of ways to link public and private agencies to those who are to be beneficiaries of rural development programs. These focus on:

- (1) the development of effective leadership at the local level;
- (2) the organization of activities around concrete and simple tasks to be accomplished by small groups; and (3) the realistic expectation of what can be accomplished through decentralization and participation.

Concern with efforts to ensure the benefits of decentralization strategies in rural development is also apparent in a report by Christensen and Webber (1981) in which the problems inherent in over-centralized planning are explored. They argued that both planning and decentralization are conventionally understood to imply control, coordination, and certainty, conditions that are rarely achieved in practice but that, as expectations, hinder the search for wider

options for bringing about decentralized planning. They propose that planners should seek to (1) exploit uncertainty; (2) keep decision making systems open; (3) adapt on the basis of experience; (4) encourage "social learning" strategies; and (5) adapt plans to specific economic, political, and social contexts. They conclude that conforming to these should result in forms of noncentralized planning that are much more successful than centralized modes of decision making.

### 5. Methodology

The research effort of the cooperative agreement on managing decentralization is largely based on extensive review of the literature on decentralization. This review is indeed thorough; in one report, the bibliography of works consulted totals 50 pages (see Ralston, Anderson, and Colson, 1983); in others, it runs to 10-15 pages. Certainly the broad review of the literature is important to the research, especially given the scope and vagueness of the concept of decentralization. Because the issue of decentralization is of central concern to the literature on implementation, greater use of this literature is recommended. In fact, Martin Landau and Eva Eagle (1981) conclude that prior studies of decentralization offered "no standard or operation definition which governs either organizational theorists or management science" (p.7).

The Berkeley research effort also assesses specific case studies to present both generalizations (Leonard, 1982) and qualification on these generalizations (Ralston, Anderson, and Colson,

1983). The case studies are usefully comparative, including a review of the decentralization problems encountered in the poverty programs in the U.S. War on Poverty (Marshall, 1982). Cases tend to come disproportionately from Africa; more Latin American and Asian cases would add balance to the research because states in these two regions tend "typically" to be stronger than the "typical" case in Africa. More important, authors of individual reports give insufficient attention to describing why they have chosen the cases they have. More attention to case selection would also alert authors to limitations on the inferences and conclusions they draw. In the general research output, mission-related consulting experiences are used as specific examples; by and large, however, they are not central to the overall direction of the basic research output.

#### 6. Programmatic Implications

The result of these methodological approaches is a series of "actionable hypotheses" that, according to the Berkeley team, need to be tested through considerable applied research. Nevertheless, Leonard (1981:6) indicates that these hypotheses can also be taken as sufficiently reliable to be adopted by field practitioners to guide their efforts at decentralization, assuming they are adopted within a critical and adaptive framework. The most important propositions are described in the previous sections. The decentralization research therefore spanned efforts to define the concept, to explore its variability, to make propositions about where, when, and how it might be implemented, and to suggest specific applied research efforts. The

findings themselves have been developed to a useful level of generalization that potentially provides field practitioners with general tools to analyze specific situations and indicates specific guidelines for making choices among a variety of options.

From a social scientific perspective, the research output of the Berkeley project is useful: it is critical; it questions received wisdom; it is based on a thorough knowledge of relevant literature; it makes good use of case studies; it proposes a series of propositions based upon conceptual analysis of the subject matter and conditions in third world countries. This positive assessment, however, must be tempered with a concern over the project's major research shortcoming, its failure to present the research output in a format that is useful to field practitioners. In short, while most of the research output is field applicable, it is not field accessible. Project reports address an academic audience in scope, tone, and style. In one case, only an academic audience would find the material understandable because of its use of abstractions, theory, and jargon (Landau and Eagle, 1981). In addition, each research report is lengthy, ranging from 50 pages to close to 230 pages in published form, far too much to expect field practitioners to "wade through."

It should be noted that, according to discussions with individuals involved in the Berkeley decentralization project, the strategy of the research team was to reach practitioners indirectly, through a more knowledgeable and sensitized academic community. That is, the project directors anticipated that its research would be most

relevant to academics but that these academics would in turn improve the potential of decentralization schemes in rural development through the influence of their teaching, research, and consultancy work. It is difficult to assess this kind of programmatic impact, and the termination of the Berkeley project in 1982 clearly helped limit more direct impact at the field level.

Thus, the research output of the Berkeley project remains significant for its quality and its effort to influence the academic community at the same time that its direct field relevance remains truncated. A newsletter like the Rural Development Participation Review or the kind of field handbook or field guide that has become a specialty of DAI could also have been produced at Berkeley, and was in fact planned by the project. On the basis of its published output, however, the Berkeley experience shows well the potential tension between an academic or scholarly concern with conceptual clarity, critical analysis, and skepticism about conventional wisdom, and the field practitioner's concern with getting something to work effectively as soon as possible.

A related point concerns the subject of the cooperative agreement itself. As one researcher stated, "decentralization is potentially everything; therefore it is effectively nothing." As a result, the research topic itself is susceptible to well-known weaknesses of academic research: overconcern with conceptual purity, reluctance to "take a stand" on an issue, and rumination into successive layers of complexity. Given the fact that decentralization



is not an end in itself but rather a strategy used to achieve other primary goals, it may be that it is not a suitable vehicle for combining basic research, applied research, and technical assistance. It is also a concept that necessarily overlaps with participation and therefore cannot be clearly differentiated from the research at Cornell on this topic. This indicates that careful thought should be given to the topics pursued through the cooperative agreements in terms of their concreteness and capacity to generate useful guidelines for field practitioners. While decentralization remains a popular idea, it may not be a particularly researchable one when it is isolated from the particular activity to be decentralized. Alternatively, the skills and insights generated by the Berkeley experience might usefully have been filtered through other cooperative agreements to aid those concerned about integrated rural development, participation, and performance management in the practical application of their research efforts. Thus, there may be a place for cooperative agreements that provides consulting experience and service to the consultants.

#### 7. Gaps in Knowledge

In terms of future research, the cooperative agreement on managing decentralization presents a useful and well-conceived series of working hypotheses that should now be the subject of field research. The analysis of the concept itself has probably gone as far as it usefully can without this kind of experience. It can be strongly recommended that other research efforts incorporate the

findings and hypotheses of the Berkeley project because of its value in exploring where, when, and how decentralization strategies might add to project success. For example, applied research on integrated rural development, participation, performance management, regional planning, and alternative rural development strategies should incorporate the insights and generalizations of the Berkeley projects in testable form. This would be the most useful way of taking advantage of the important research output of this project.

In addition to field research, further conceptual specification should also be considered. A useful place to begin is with the concern with structures of inequality that affect decentralization initiatives. The propositions generated through the Berkeley research focus on an important first-cut at the issue of inequality, the relationship between local elites and the poor. The next step should be to disaggregate "the poor" more effectively into units sharing relevant characteristics—gender, relationship to productive resources, age structure, dependency relationships. This would permit the generation of more specific propositions about how such groups are affected by various decentralization strategies. Even where local elites are responsive to the interests of the rural poor in general, for example, a decentralization strategy may reinforce existing inequalities within the ranks of the poor, such as those that might exist between men and women or between marginal landowners and the landless. A focus on further conceptual analysis would have clear implications for the kinds of methodologies selected to study the

appropriateness of local contexts for decentralization strategies and the programmatic implications for their selection. This second-order hypotheses building effort would propel the study of decentralization toward areas of new knowledge-building at the same time that it would generate useful insights for field practitioners.

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REVIEW HLAND TENURE

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1. Overview of the Cooperative Agreement

In May 1962, the University of Wisconsin entered into a contract with AID to conduct applied research on land tenure issues. This initial contract, which with amendments was to run until 1969, authorized the University to create a Land Tenure Center (LTC) that would concentrate on research and training. The LTC was given relatively free rein to undertake multidisciplinary research on land tenure problems, work with third world government agencies and universities, publish manuscripts and bibliographies on land tenure issues, and build a specialized library. However, its principal task was to monitor agrarian reforms in Latin America that were supported by the newly signed Alliance for Progress. When the second contract with AID was signed in 1966, the LTC was beginning to establish a broader focus on land tenure and reform throughout the third world. That contract, which ran from 1969 to 1979, was funded under section 211 (d) of the Foreign Assistance Act of 1961. It allowed the LTC greater leeway to take a broad geographic and multidisciplinary focus.

The current cooperative agreement was signed in August of 1969. The initial grant for the first three years of operation was \$1 million. This has been augmented by an additional \$2.1 million in

supplementary funds and mission add-ons. The cooperative agreement calls for the LTC to provide direct assistance to missions and host governments. Toward this end, the LTC is required to serve at least five missions or host governments per year over the life of the project, to produce five state-of-the-art papers, to develop a network of consultants available for work on land tenure issues, and to disseminate to specialists and applied practitioners information related to the cooperative agreement's focus.

The cooperative agreement has given the LTC more opportunities for field work than it had in the past and has required that the Center improve its capacity to undertake short-term assignments of immediate interest to AID and host governments. To date, the LTC's core staff and consultants have carried out four long-term efforts in Botswana, Nicaragua, Ecuador, and Mauritania. At the same time they have provided short-term services to ten missions in Latin America, three in Asia, and three in Africa. One state-of-the-art paper has been produced on land markets and three more are in progress on the topics of group farming, pastoralism, and lessons learned from past land reform efforts. Finally, four research papers based on applied consulting work have been written and issued through the LTC publication series.

## 2. Central Assumptions

It is not easy to separate the present assumptions underlying the LTC cooperative agreement from those that have become associated with the organization since it first began to make its presence felt

in rural development. A review of recent LTC publications suggests two clusters of assumptions. The first relates to the place of land tenure in rural development:

- a. patterns of land tenure cannot be separated from their larger ecological and societal setting;
- b. planned and unplanned changes in land tenure patterns have major impact on the development processes of a country;
- c. equitable distribution of the benefits of rural development should be a major object of land tenure systems;
- d. land tenure reform does not always have positive effects for production or equitable benefits;
- e. evaluation of land reform options should focus on political stability implications as well as production and equity.
- f. there is no preferred land tenure system, however, the small family farm based on freehold tenure has the most promising potential for growth and equity;
- g. gradual reforms are more likely to be successful than sweeping reforms that introduce entirely new land tenure patterns; and
- h. proposals to reform land tenure patterns should be closely accompanied with recommendations for promoting local institutional capacity to regulate land-related functions and train local leaders to manage such institutions.

The second set of assumptions relates to the role of the Center in forging applied policy recommendations and building host government institutional capacity:

- i. research carried out by LTC staff should seek a balance between conceptual development, policy analysis, and applied recommendation;



- j. micro data is more useful than macro data for analyzing land tenure issues;
- k. research publications should combine original field work with a good review of the secondary data and be multidisciplinary in inquiry;
- l. research undertaken by the Center should be published and, if possible, made available in the language of the country studied;
- m. general principles should be sought and applied in research but it must be recognized that land tenure patterns and their affects are highly variable and idiosyncratic;
- n. studies published by the Center should be pragmatic, with care taken to ensure that they apply to the political realities of the task environment and the options that it permits;
- o. analysis in the Center's studies should be candid and willing to criticize government or donor policies when necessary;
- p. research undertaken by the Center should be closely connected to the problem solving needs of policy makers rather than be merely abstract or over-detailed contributions to academic literature; and
- q. research by expatriate specialists should be closely connected with efforts to train local specialists and build local research institutions.

### 3. Theoretical Framework and Central Concepts

No established social science theory guides the research of the LTC. However, the overall approach of the work product clearly emphasizes structural analysis. Land tenure is defined as the rules, institutions, and patterns governing the ownership and use of land. The structural patterns that mark land tenure systems are considered to play a major role in controlling access to productive opportunities on the land, shaping rural patterns of income and employment, and

determining the distribution of wealth, status, and authority in both the urban center and the rural periphery. Theoretical concerns are focused on the ways in which these patterns of landholding create opportunities and constraints for equitable economic growth and on the kinds of reforms that can overcome constraints and capitalize on opportunities. The analysis is set in the conceptual tradition of institutional economics and legal analysis. As such, it is guided by economic theory and legal structuralism. The lack of a clear theoretical framework has some positive benefits. Specifically, it prevents the analysis undertaken by the Center from becoming ideologically charged and allows for objectivity in a difficult and controversial area.

Since the cooperative agreement began, the focus of the LTC has been decidedly more micro and increasingly multidisciplinary. Emphasis has been on producing detailed case studies that seek to be useful to donor and host government policy or programmatic concerns. As yet, no overarching manuscripts have been prepared that have the scope of Dorner's earlier book on land tenure (Dorner, 1972). Nor have middle range theories been developed that have the utility of the analysis produced by Dorner and Kanel on productivity and employment creation advantages of small family farms (Dorner and Kanel, 1971). Hence, there is no published record from which to assess if the LTC's core staff is guided by or building a coherent theoretical perspective that can serve to organize data and generalizations about land tenure, identify principal problems related to land tenure, guide research on

them, facilitate analysis of the research product, or evaluate the utility of the resulting analysis.

#### 4. Research Findings and Programmatic Implications

The LTC has made substantial contributions to knowledge about land tenure since its establishment in 1962. For a summary of that contribution, see the recent evaluation carried out by AID consultants (Montgomery, Powelson, and Schuh, 1982). This review will only focus on research findings and applied recommendations generated since the cooperative agreement began.

Throughout the cooperative agreement, the LTC has been confined to highly specific research topics that are determined by missions and host governments. As a result, the findings are situation-specific and heterogeneous. If there is a common thread, it is the Center's concern with seeking: (1) to identify and understand village-based or corporate land tenure-related development problems; (2) to analyze alternative local organization forms and institutional arrangements for addressing such problems; and (3) to review issues of land-based resource ownership and management. The reports that have been published in the LTC's research series and those that have remained consulting memoranda have both the detail demanded by academics and the operational thrust needed by development practitioners.

The only published overview paper is on land markets to benefit the rural poor (Dorner and Saliba, 1981). The paper provides advice to governments that seek to promote greater access to land by

the poor but are unable to carry out reforms that expropriate or redistribute land. Towards this end it sets forth strategies for improving the functioning of land markets on the assumption that freeing up markets for private property will improve access for the poor. Among the strategies discussed are land taxation, land registration and titling, improved instruments of credit financing, and several means of direct state acquisition and disposition. The strategies discussed are illustrated by case studies drawn from Costa Rica, Bolivia, Chile, Ecuador, Jamaica, Japan, Australia, Canada, and the United States.

Briefly, among the important points made by the paper are:

- a. Policies to make land markets dynamic are not substitutes for redistributive land reforms but assist the poor in interim periods and complement reforms when they do occur.
- b. Strategies reviewed are most effectively applied in situations where redistribution via expropriation and tenure reform has already proceeded, where a relatively egalitarian landholding structure already exists, or where the land market can be made to work more efficiently in the interests of both productivity and equity.
- c. Two or more of these strategies must be implemented to have any probability of reaching the desired results.
- d. Strategies presented generally require a system in which private property in land prevails and a market for land is potentially feasible.
- e. Land taxation policies can create developmentally favorable incentives within the agricultural sector while simultaneously increasing that sector's contribution to public revenue.
- f. With strong political will and sound land tax designs, land ownership patterns and income distri-

bution can be moved in the direction of greater equity, as well as used to promote more nearly optimal use of land, water, and other resources by affecting the intensity of resource use and output mix.

- g. Among the choices that policy makers can make in designing land tax policies are severity and gradation of the tax rate, basis and method of assessment, exemptions, special penalties, and different types of payment.
- h. Methods of land registration and titling define and facilitate the transfer of possession and use rights over land, and are particularly important in early states of development when land is usually the most important productive resource and capital is scarce.
- i. Land sale guarantee programs and programs that provide direct guarantee by government of loans by local credit agencies lending directly to farmer-buyers are attractive strategies for inducing lenders to provide land purchase funds to large numbers of small farmers whose credit-worthiness and managerial capacities are not well-known.
- j. State acquisition of land programs should be coordinated with land tax policies that improve administration and increase revenues because effective tax policies can improve the functioning of the land market and increase the state's leverage for acquiring land.
- k. Most third world countries have too little capital and too little taxing power to buy a great deal of land and hold it to protect farmland from urban encroachment or to farm, but they can follow strategies such as land sale guarantee programs based on bonds.
- l. If a government land acquisition and sale program is implemented, an appropriate agency of the government probably should be given first priority for the purchase of such lands when the agency is working in close collaboration with landless farm workers, renters, or farmers owning very small units.

The land market paper also reviews the form of organization to be established on lands acquired by the government. Here, after

briefly summarizing joint operations, joint farming, cooperative farming, collective farming, state farms, and communes, the authors conclude by pointing out: (1) how difficult it is to generalize about which form of farm system is more effective or appropriate; (2) how difficult it is for outsiders to impose a particular form on a farming community; and (3) how important it is for the people most directly affected by the selection of a form to participate in that decision. This section of the paper is less detailed than that dealing with the actual strategies. As such, it merits further work, particularly in elaborating criteria that can assist policy makers seeking to reach decisions as to what form of farm system should be promoted.

Clearly, the poor need financial support and institutional mechanisms through which to obtain land in task environments where government land grants or major redistribution reforms are not possible or slow in implementation. Such conditions are found in a number of countries. Hence, the ideas presented in this paper can prove useful to governments and donors seeking to find ways around such structural binds. However, as the authors point out, the paper has the greatest relevance for countries of Latin America and the Caribbean.

Four of the five published papers are on Africa. The first, on land tenure issues in West African livestock and range development projects, focuses on the role of land rights in African development projects dealing with livestock and range management (Riddell, 1982). It does this by reviewing a variety of recent projects that attempt to

get collectivities of herders to use existing but decreasing land resources in new ways. As such, it reviews strategies that seek to restructure range use, create new water resources, or improve the quality of the herd for more intensive use of pastures. The central research question that underlies these kinds of strategies is "who has how much right, and when, to those resources." After reviewing projects in Mauritania, Senegal, Niger, Cameroon, and Mali, the paper offers a number of insights useful to understanding that question and relating it to project design issues.

The paper is analytically dense and contains a number of important observations that enhance awareness of land tenure patterns in pastoral areas and that begin a needed process of stimulating more careful research aimed at building a better paradigm for organizing tenure patterns in arid areas. Some of the more interesting of these observations are that:

- a. Africa is characterized by increasingly transitional modes that make it difficult to make sharp distinctions between "farmers" and "pastoralists" or "sedentary" or "nomadic" peoples when discussing the management of land resources.
- b. Present assumptions about the contribution of domestic livestock to the diet may be an artifact of our culture and not represent a universal value worth pursuing through livestock development projects.
- c. Livestock and range development today uses a basic paradigm that did not work in the colonial period and in all probability will not work any better today (e.g., stimulating animal production on already over-taxed pasture resources through improved animal health (veterinary medicine), water point development to extend the range, and preserving the pasture through increased offtake to meet the existing demand for meat.

- d. Livestock development programs and projects are backed by a very good scientific literature but marked by minimal accomplishments.
- e. Livestock development interventions can easily be undermined by drought and any long-term program or project in the livestock sector must include drought-related strategies as an important element of their design.
- f. Tenure revisions are needed to give farmers seeking to begin cultivation and build villages the capacity to develop land in pastoral areas, but such revisions should preserve the residual rights of herds that predate the arrival of cultivators.
- g. New land tenure rules introduced by livestock development intervention must contain provisions that allow herders to revert to drought strategies when rains fail.
- h. Residual rights should form the basis of land tenure policy that recognizes the nature of the resource base and the territorial extent of economic and resource management relationships and they should not be viewed as a nuisance to be written out of existence through the use of appropriate legal machinery.
- i. The trend to establish unified land tenure codes does not make sense for Africa given the diversity of production strategies followed by different ethnic groups in varying ecological niches.
- j. Land tenure policy should be formed around resources like water that have an exchange value in the existing economic system.
- k. Accumulated evidence suggests that land tenure legislation that draws lines and provides separate tenure systems on one side for cultivators and on the other side for herders will not work.
- l. Any tenure arrangement that makes cultivation and pastoralism incompatible is bound to be ignored in practice.
- m. Land tenure policies should avoid making assumptions that equality exists among herders, as this is frequently not the case.



- n. There is a need to develop a new paradigm or method of analyzing the land tenures of Africa's livestock managers and for developing land tenures for arid regions that are flexible, dynamic, and adjustable through negotiations as seasonal variations are encountered.
- o. It is a mistake to assume that the open range is a blank slate upon which rules of access, allocation, and duration can be written with the assumption that that which is owned by all is owned by no one.
- p. Land tenure provisions for arid areas must be sensitive to the types of landed resources needed for survival (grass, water, camp sites game, etc.) and the seasonality of use.
- q. Analysis of land tenure systems and development of policy options should be sensitive to the fact that there are rules of resource use that are based on custom, precede formal law, and take precedence over it (law-in-action).
- r. Until the patterns of access and exclusion are understood, it is a mistake to promote modern range management strategies as they are based on the need for exclusion as a prerequisite for predictability in the management and rehabilitation of the range.
- s. Rights to seasonal access, reversion to other groups when crops are harvested, crisis access in times of drought, and other residual rights plague efforts to design livestock development projects and need to be carefully examined in the pre-implementation phase.
- t. Land policy should view tenure for livestock managers in arid areas of Africa as a bundle of rights being exercised simultaneously along three dimensions of transiency, inclusivity, and intensity.

Clearly, this detailed paper raises a number of interesting generalizations that can be used to guide future research on the management of natural resources and to promote intervention designs that are sensitive to existing patterns and compatible with local interests and needs.

A related paper on Botswana (Lawry, 1983) reviews recent ideas for promoting the smallholder livestock sector through the management of communal grazing resources. After providing a history of the evolution of present day government livestock and grazing policy, the author concludes that "most conventional approaches to local institutional development do not account for the extent to which the potential authority of local institutions for regulating resource use has been irretrievably undermined by changes in the structure of the rural economy" (p.iv). Similarly, he demonstrates how "the changing role of livestock in household income strategies also militates against many forms of local level action" and how "decreasing reliance on livestock as a source of current income contributes...to increased resource degradation" (p.iv). In establishing this format, the paper points out how useful historical analysis can be and how important it is to understand changing household decision patterns. As such, the study provides a model for future studies that will undoubtedly be undertaken in many other areas of Africa where livestock play a role in the local economy. Beyond this, the paper illustrates the difficulty of addressing management of the commons issues in periods of rapid social and economic change, particularly when local institutions and structures are being undermined by other government policies. This is an important warning to those who will be undertaking similar studies elsewhere. Finally, the paper provides an interesting alternative approach that seeks to define tenure rules in terms of individual rights to common property. In elaborating that

approach, the author provides useful distinctions between communal grazing land management based on a "public lands management perspective" vs. "range management perspective." The discussion on these distinctions will prove useful to others faced with similar policy-relevant research questions.

While technically not produced for AID, a paper on Zambia (Bruce and Dorner, 1982) describes the land tenure system that has been established in that country since independence. It provides a useful critique of the Zambian system that can be drawn upon by the government and donors. Briefly, it describes a system that assigns all land to the president to hold in perpetuity. Land is leased to people at no cost and leases may be transferred with the approval of the government. For purposes of financing and taxation, the land is assumed to be "without value," a concept that distorts production and distribution patterns. The report analyzes the existing tenure system and offers suggestions for dealing with the constraints it creates. These suggestions range from general observations to specific recommendations, for example: (1) land tenure measures are not likely to be effective stimulants of agricultural productivity unless accompanied by appropriate price policies, adequate and timely delivery of production inputs, and limitations on the amount of state market intervention in the regulation of the economy; (2) while land tenure has a long-term impact on the development of agriculture, it will not be a useful tool for getting rapid, short-term increases in food production; (3) adopting a "land without value" concept in order

to promote equity and maintain communal property principles is not in the national interest as it promotes unintensive land use and allows a few farmers cost-free access to limited and valuable land; (4) rather than implementing across-the-board reform of intestacy rules to deal with the negative impact of customary tenure's inability to designate heirs to land, clearly it might make sense to introduce limited freedom of testation; and (5) current lands and deeds legislation is unnecessarily complicated given the importance of leaseholds to Zambian economic productivity, and officials should consider revising it along lines of several recent laws drafted elsewhere in Africa.

In essence, the Zambia study illustrates how customary tenure systems can be adjusted to the needs of a developing and modernizing agriculture. Since a number of African governments are faced with the need to balance the attractiveness of maintaining valuable aspects of traditional tenure, promoting production, and ensuring social equity in access to land, the paper has utility to other countries, donors, and development specialists. Perhaps most important, the paper illustrates the willingness of LTC researchers to address sensitive issues of land tenure and to criticize cherished government policies for regulating land use.

The final published paper in Africa examines customary law allocations of residential land in villages (Bruce, 1981). It illustrates that traditional customary rights are not necessarily an impediment to mortgages and that reforms toward freehold systems in order to allow mortgages can lead to substantial social inequities.

Briefly, the study seeks to ascertain if the tenure system can be sufficiently improved to facilitate the emergence of a mortgage market. This is an important question, for the general argument in Africa is that communal land held on a use basis should be converted to freehold in order to allow holders to obtain improvement money through mortgage instruments. The paper points out that customary tenure is sufficiently secure and permits leasing and the sale of allocations. Other factors prevent the emergence of a mortgage market: namely, the fact that land in the residential areas of villages has artificially low market values, that mortgages of customary residential allocations have vague legal status, and that land boards hamper efforts to evolve customary rules to permit mortgage of such land. On the basis of these facts, the paper offers several options: (1) conversion to freehold; (2) government legislation to promote the use of mortgages within the customary law system; and (3) development of leases with legality as mortgagable interest. The first is criticized for being costly and likely to lead to inequality, while the second is rejected as being too complicated. Hence, the study recommends the third, largely because it is simple and likely to be effective. What is important about this paper is that it suggests how highly focused research can overcome the general simplistic view that customary tenures should be converted to freehold. In particular, its detail and style suggest a format for carefully researching and evaluating a much wider range of options than those usually considered.

A number of consulting reports illustrate the range of experience and data that has been generated through the LTC project:

(1) In Botswana an LTC consultant prepared a report that sets forth an inventory of village institutions, explains why government-created Village Development Committees and other government-sponsored organizations are not functioning well, and presents a rationale for avoiding the creation of center-imposed organizations at the local level. (2) In Indonesia, a paper was prepared to assist the mission to design a project aimed at helping the government develop efficient and effective methods for improving land mapping, registration, titling, record keeping, and administration. (3) In Nicaragua, a workshop was prepared that outlined proceedings for presenting information on comparative experiences in land reform to members of the Sandinista-led government. In addition, a paper was produced that develops a program for participant training for provisional staff members of the Agrarian Reform Agency. (4) In Ecuador, reports were prepared that aim at developing a strategy to strengthen the capacity of the Ecuadoran Institute for Agrarian Reform and train its professional staff in research methodologies, agrarian structure policies, and rural development strategies. (5) In Mauritania, studies were undertaken on traditional land tenure patterns with the purpose of recommending legislative reforms for the government to promote in its next development plan. The micro focus of present LTC reports raises the issue of the need to integrate them in ways that contribute to filling the knowledge gap about land tenure and its reform.

## 5. Methodology

Present research by LTC core staff is field-based and focused on producing primary data. This micro perspective generates a great deal of fine grained information, primarily through the analysis of government legislation and documents, compilation of statistics from government records, interviews with officials and rural people, intense field observation, and extensive use of secondary published and fugitive research publications. Questionnaires and surveys are rarely used to gather micro level data (Stanfield, 1979 is an exception). No innovative methodological approaches are used in the publications reviewed here. The methodological approaches to data collection described above appear to have been carried out without significant bias.

The eclectic methodology of the project for gathering data is carried out with an inter-disciplinary perspective. While researchers are grounded in a particular field, this approach requires them to disregard conventional boundaries and seek descriptive and explanatory information from anthropological, sociological, political, economic, and geographical perspectives.

## 6. Gaps in Knowledge

The work product of the cooperative agreement phase of the LTC's contribution to knowledge is so micro-oriented that it would take a specialist to determine what gaps exist in each of the country-related papers. It is also dispersed across many countries and themes, so that it is hard to determine the aggregate effect of the

studies. Hence, there is a clear need for consolidating comparative research to tie these emerging studies to the larger literature.

To date, only one generalized paper has been produced, a study of interventions in land markets to benefit the rural poor (Dorner and Saliba, 1981). It is not specifically based on the field studies undertaken as part of the cooperative agreement. Additional studies are being drafted on pastoralism, group farming, and lessons learned from recent land tenure reform experience. It is assumed that the first and third of these topics will be based in part on recent LTC field studies. All three studies would be useful. However, what is not clear is the criteria by which such general topics are selected or how the LTC sets priorities for them. One useful activity the LTC could undertake would be to review the state-of-the-art of knowledge about land tenure, identify major middle range gaps in comparative knowledge, and develop a strategy for filling them with general studies.

Among the topics that merit consolidating studies and that are linked to current work are: (1) legal issues surrounding community management of natural resources; (2) land tenure patterns and reform issues in Africa; (3) land tenure reform and its effect on and need for local organizations; (4) the role of PVOs in land tenure reform; (5) problems of building institutional capacity to do research on land tenure issues and to implement technical aspects of reform in a task environment where reform is unlikely; (6) reevaluation of land tenure issues in the light of longer time perspectives on the impact of the



green revolution; and (7) the changing status, income, and power of small, medium, and large farmers and the landless in consequence of reforms. Among these topics, papers on community management of natural resources and land tenure in Africa are the most closely linked to ongoing work.

Two additional comprehensive research publications are needed, both of which the LTC is uniquely qualified to write. First, there is a pressing need for a major comparative study of the topic of land tenure. No comprehensive and theoretically informed work has appeared since the publication in 1974 of the work of Hung-Chao Tai. Yet, much has happened as a result of revolutions over the past decade, the impact of the green revolution, and increasing structural transformation in agriculture. The LTC has the resources, the experience and the contacts to prepare such a study. If properly done, it would have significant utility to governments facing reform issues, to donors, and to the academic community. Second, the LTC has always attempted to match research with institution building and training. As such it has learned a great deal about how to build land tenure research institutions and to assist in training their professional staff. A consolidation of that experience into a set of guidelines for building institutional professional capacity would prove useful to host governments and donors interested in funding the development or reinforcement of such institutions.

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REVIEW IREGIONAL PLANNING AND AREA DEVELOPMENT

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1. Overview of the Cooperative Agreement

A cooperative agreement between AID and the University of Wisconsin was undertaken in 1978 to explore the potential of regional planning and area development strategies to increase the impact of rural development interventions in third world countries. As the project evolved between 1978 and 1981, it focused on three related issues—planning, area development, and decentralization—as modes of action that would lead to more effective regional development. The research output of this project frequently overlapped with the concerns of Berkeley's decentralization project, Cornell's participation research, and DAI's focus on integrated rural development as there were common themes linking these projects, such as developing more responsive administrative systems and designing integrated planning and service delivery mechanisms. In the Wisconsin regional planning and area development project, however, the concern with planning as a concept, methodology, and practice was most fully developed.

The cooperative agreement contract with the Department of Regional Planning at Wisconsin committed the project to producing a state-of-the-art paper and to field testing major hypotheses and pursuing in-depth research in four countries. Ultimately, the project

was only able to gain access to one country, Tunisia. In that country, the project carried out research and assisted in the implementation of a regional decentralization plan. The project produced consulting reports, three state-of-the-art papers, a comparative study of regional planning experience in East Africa, and a general paper derived from the experience in Tunisia.

## 2. Assumptions

The Wisconsin research undertaking was committed to the idea that planning is an effective instrument to increase the potential of development programs and projects. Within this broad perspective, regional planning is affirmed to be a means to achieve greater equity and economic benefits from rural development interventions. Thus, the concern of the research is with "the notion of planning as an approach to development and regional planning as an approach to regional development" (Castillo and Hoffman, 1981:1). Jakobson (1981:5) notes that there is no agreed-upon definition of planning, region, or regional planning among scholars or practitioners. In practice, however, there is general agreement in the Wisconsin research output that planning "is concerned with analyzing information to propose solutions to societal problems, evaluating those solutions, and finding ways to carry them out" (Castillo and Hoffman, 1981:3; see also Jakobson, 1981:11-12). Regional planning is considered to be a subset of planning practice that is specifically concerned with the development of peripheral regions, defined as areas that lag behind national averages of socio-economic development (Castillo and Hoffman,

1981:1). Regional planning, and the planning effort more generally, must address both the technical and methodological aspects of planning and the institutional context that largely determines the success or failure of planning initiatives. While the research output is concerned with both, it is clear that the institutional or contextual factors that affect planning initiatives are assumed to be most important in determining planning outcomes. Jakobson (1981) acknowledges the importance of these factors but argues that concern with them easily results in overlooking the need to pay extensive attention to the technical aspects of how to formulate and implement a regional planning effort.

### 3. Major Hypotheses and Findings

In general, a considerable portion of the research output of the Wisconsin project is concerned with developing the concepts and perspectives appropriate to the pursuit of regional planning in practice (Castillo and Hoffman, 1981; Jakobson, 1981; Sen, 1981; Morgan, 1981). Thus, much of the effort evident in documents produced considers the assumptions and concepts underlying previous approaches to regional planning and attempts to provide "a new paradigm for regional planning" (Hoffman and Castillo, 1981:3). As explained in a research document concerned with regional planning doctrine, generally accepted assumptions underlying this approach are that: (1) regional planning is an effective means to encourage socioeconomic development; (2) economic growth within a region will rebound to the benefit of the region and its population; (3) such growth will improve regional

social conditions, and (4) the organization of space is critical to the direction of socioeconomic development (Castillo and Hoffman, 1981:14-15).

The authors of this report question a number of these assumptions and argue that while regional planning and development have great potential, they need to be reconceptualized to overcome the erroneous assumptions of much current and prior thinking, particularly those that identify growth with development and those that identify area with poverty. In particular, they must face two central dilemmas of regional planning: (1) the identification of the causes of regional underdevelopment; and (2) the recognition that regions generally incorporate a nonhomogeneous population. Theories of regional development, such as those of Rondinelli and Ruddle (1978) and Friedmann and Weaver (1979) disagree over whether regional underdevelopment is caused by a lack of effective linkages and the lack of spatial integration between the region and broader economic and social structures or whether it is caused by insufficient autonomy from the (possibly) exploitive larger structures (Castillo and Hoffman, 1981:7-13). In addition, much prior work, by assuming that regional economic growth was linked to socioeconomic development, failed to recognize that economic growth would be shared unequally at the regional level and could even lead to greater levels of inequity within a region. Thus, it is argued that the concentration on area found in much regional planning should be replaced by concentrating on target groups within regions. In terms of planning, however, the

region continues to be considered a viable unit for bringing together complex problems and agendas and integrating them into a workable plan for action (Castillo and Hoffman, 1981:32).

Consideration of decentralization as a strategy for administering area development plans is also proposed in project documents (Morgan, 1981; Rondinelli, n.d.). Similarly, the assumptions underlying most rationales for decentralization are found to be faulty (see especially Morgan, 1981). That is, decentralization is probably not an effective means to bring about sociopolitical reform through greater participation and responsiveness, as is argued by many. Rondinelli (n.d.) suggests that lack of political commitment to sharing power effectively is a major constraint on plans and efforts to decentralize. When attention is directed to the role of participation in decentralization, Morgan (1981:38) argues that it "will serve as a means of implementing, rather than influencing, the decisions of those occupying major positions in the political apparatus....Decentralization and popular participation are not realistic means for achieving a major restructuring of society." Instead, decentralization should be viewed as an administrative technique for increasing organizational performance and adaptation to a variable tasks environment. When reconceptualized in this fashion, it offers a viable mode of action for increasing the potential impact of regional development plans. According to Morgan (1981), the perspectives of organization theory are most useful for accomplishing this.



Commitment to the utility of new conceptualizations or regional planning and decentralization is linked to a series of propositions found in the research documents. These propositions are general in nature and are not specifically tested through field research. Instead, they represent a series of hypotheses for improving the pursuit of regional planning and decentralization initiatives. Jakobson (1981) presents a methodology for planning that, he argues, would allow practitioners to capture many of the characteristics essential to successful regional planning and pro- development. Planning can be pursued in "complex, uncertain, and unstable situations" through his "sketch plan concept" that promises integrated, coordinated, and interactive activity even on the basis of limited and often faulty information (Jakobson, 1981:44). The "sketch plan concept" is based on the project's experience in Tunisia.

More broadly, Castillo and Hoffman (1981) propose that planning for regional development must meet a series of criteria if it is to be implemented successfully. The planning process must incorporate an analysis that is historical, dynamic, continuous, complex, and holistic, as these are conceived by the project team (pp. 132-34). Second, it is proposed that effective and implementable plans must be multisectoral, interactive, coordinated, innovative, and autonomous as these criteria are defined in project documents (pp. 35-37). In a paper on the "project cycle," Sen (1981) argues that a related series of characteristics is necessary for successful projects. Thus, project planning, design, and implementation efforts must be under-

taken in a way that is circular rather than linear, that is interactive in terms of design and implementation processes, and that allows for learning during the project cycle itself. Sen argues that effective project development "requires that the appraisal, implementation, and evaluation activities occur throughout the project cycle rather than only once in an arbitrary sequence" (1981:11). He indicates that the information necessary for the project cycle should be reduced to a relevant minimum level, that implementation activities require special attention including concern for institution building and budget processes, that continuous monitoring is essential, and that flexible methodologies for project design and evaluation are essential. In both cases, regional planning and project design and implementation are political activities in that they are continuously influenced by the context in which they are pursued.

Rondinelli, (n.d.) also proposes a series of characteristics of decentralization efforts and their context that would insure more effective efforts. Thus, he argues, strong political commitment to decentralization is essential for success as is the willingness of leaders to share power with organizations and groups not under their direct control. Bureaucratic support and administrative capacity are important at both central and lower levels of government. The resistance of local elites must be dealt with and rural inhabitants encouraged to participate effectively in decentralized interventions. Clear but flexible laws and administrative arrangements for allocating responsibilities are as critical as the development of supportive

attitudes among officials and citizens and for decentralization efforts to be successful. In contrast, he indicates that empirical efforts to pursue decentralization in the Sudan, Kenya, and Tanzania suffered from lack of political commitment to decentralization, inadequate planning, low administrative capacity, and resources, and generally unsupportive environment. Thus, the contextual factors surrounding efforts to decentralize were far more important than the characteristics of the decentralization plans themselves in the limited success these African governments encountered.

#### 4. Methodology

It is clear from this review of the propositions and findings that most of the research output of the regional planning and area development effort deals with conceptual issues and relies upon literature specifically identified with regional planning, decentralization, and organization theory (Castillo and Hoffman, 1981; Sen, 1981; Morgan, 1981). There is little attempt to draw broadly on social science literature beyond these fields or to apply concepts to specific case studies. An exception is Rondinelli (n.d.), who bases a discussion of decentralization on an evaluation of the experience of the Sudan, Tanzania, and Kenya, utilizing literature on the course of those governments' efforts at decentralization and (apparently) discussions with knowledgeable persons. In addition, Jakobson (1981) presents a planning model based on extensive experience in Tunisia and a number of other countries.

In the research output as a whole there is scant attention to issues of methodology. Concepts tend to remain vague and poorly developed and are not linked to solid recommendations about data collection and field testing. Rondinelli (n.d.) can be taken to task for presenting expansive and largely untestable conclusions on the basis of slender evidence apparently unsupported by direct observation. Jakobson's (1981) advocacy of the "sketch plan concept" could be the basis for clear field application, but it needs to be presented in a more accessible format. In general, the research output suggests that conceptual clarity, a clear research agenda, and methodological rigor were not central to the project's design or implementation.

#### 5. Programmatic Implications

Because of the conceptual nature of much of the research output and the high level of generality of most of the major propositions, specific programmatic implications of the Wisconsin effort are difficult to identify. Propositions such as "regional planning efforts are greatly influenced by the environmental context in which they are pursued," or "political commitment is essential to make decentralization efforts work effective? /," do not lend themselves easily to operational guidelines. In particular, the discussions of the characteristics of effective planning and the conditions conducive to plan implementation and decentralization are likely to be considered unrealistic by field practitioners as so much of the economic, social, and political reality encountered in third world countries is directly counter to the supportive or facilitating conditions posited

as essential to success. There are, within the project documents, a large number of prescriptions about what ought to be in terms of the aspects that planning should incorporate, the characteristics that regional planning should conform to, or the environment that should surround efforts to decentralize. More specific guidance about how these conditions might be created is needed. An exception to this tendency is the series of activities meriting AID assistance that Rondinelli (n.d.) derives from his evaluation of decentralization efforts in three African states. Similarly, Sen (1981) provides a series of general guidelines for project cycle success that relate to the importance of planning, simplicity in administrative processes, budgeting, organization, and the utilization of scarce personnel. These can be considered general rules of thumb to guide practitioners in the design and implementation of rural development projects. They are not injunctions that are likely to take practitioners by surprise however. Thus, a clear direction for future effort in regional planning and area development is to utilize conceptual frameworks and broad propositions to explore with greater depth, specificity, and parsimony the characteristics of plans and their environment that can enhance successful efforts. These in turn should be the basis for deriving field accessible guidance about where, when, and how regional planning and area development can be undertaken.

#### 6. Gaps in Knowledge

The research output of the Wisconsin project provides a first step toward translating generally agreed upon "goods"—regional

planning, area development, and decentralization--into more effective practice. The reviews of previous efforts have allowed researchers to identify the problems with many regional planning efforts and to define concepts in a way that makes it more possible to include a broad range of socioeconomic variables in regional planning analysis. However, the two dilemmas pointed to by Hoffman and Castillo (1981)--the need to identify the causes of regional underdevelopment and the need to address an area that incorporates economically and socially diverse populations--remain dilemmas for researchers and practitioners in the present.

The divergent perspectives of Rondinelli and Ruddle (1978) on the one hand and Friedmann and Weaver (1979) on the other remain a topic for much extended research. That is, area development initiatives should begin from a basic analysis of the causal factors involved in regional underdevelopment; the methodologies proposed by the Wisconsin team may aid in this effort, but much empirical and theoretical work remains to be done in order to determine whether linkages to broader economic, political, and social structures are likely to be conducive to or destructive of plans for regional development. As with most such controversies, the answers to the questions it poses are likely to be complex and variable by location, target group, and political and economic context. Thus, prior to efforts at effective regional planning, a fuller appreciation of the nature of the problem is required.

Similarly, it is valuable to note, as the project team does, that economic growth is not the same as insuring greater equity within a region. This statement must become the basis for more detailed research on how regional planning and area development schemes can go beyond a spatial orientation to address the complex issues of intra-regional diversity, differentiation, and variability of economic and political power. Within the effort, attention should be directed to the dynamic aspects of regional change; migration, infrastructure development, population growth, industrialization, agricultural production patterns, and other processes of change are profoundly altering the reality of rural regions throughout the third world. Can regional planning and area development schemes respond to this changing context effectively enough to lead development efforts in positive directions?

A third aspect of the research undertaking that needs additional attention relates to both of the above issues. There is a consensus in the project output that the context of regional planning, area development, and decentralization is critical to the success of efforts to achieve these objectives. This context should be the focus of considerable field research to develop more specific understanding of what contextual factors seem most critical, how they vary across time and location, and how more positive development contexts might be created. This is a large research undertaking and as such it suggests that much greater effort needs to be given to focusing future research tasks on researchable questions and important hypotheses.

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## SECTION III

FOCUSED GUIDELINES FOR IMPROVING THE UTILITY  
OF COOPERATIVE AGREEMENT RESEARCH:  
THREE SPECIALIZED REPORTS

In Section I, a series of recommendations is presented for improving the knowledge-building potential of AID-sponsored cooperative agreements. In this concluding section, additional guidelines are presented for: (1) improving the methodological foundations of cooperative agreement research; (2) increasing the policy relevance of cooperative agreement research; and (3) improving analysis of the role of households and women in rural development research. The analyses and resulting recommendations are developed in three papers especially commissioned for this section of the overall report:

A. Improving Research Methodology

Donald P. Warwick  
Harvard Institute for International Development

B. Improving Research Utility

John D. Montgomery  
John F. Kennedy School of Government  
Harvard University

C. Improving Research Contribution to Knowledge About Households and Women

Pauline E. Peters  
Harvard Institute for International Development

SPECIAL REPORT AIMPROVING RESEARCH METHODOLOGY

Donald P. Warwick

1. Introduction

A review of the research methodology seen in publications from eight cooperative agreements suggests two broad conclusions.<sup>1</sup> First, all of the agreements led to social scientific research of some kind. The specific projects ranged from carefully designed sample surveys to loosely structured bibliographic explorations. Second, this research followed no common standards for conceptualization, research design, data collection, analysis, and interpretation. Among the numerous studies using sample survey methods, most relied on some version of probability ("random") sampling but one adopted a primitive form of quota sampling. Several authors reporting case studies made a systematic effort to ensure that the cases chosen fairly represented the domains about which generalizations were being made. Others seemed to view cases as a vehicle for marshalling evidence or of promoting a

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<sup>1</sup>The cooperative agreements covered were those with the University of California at Berkeley; Cornell University; Development Alternatives, Incorporated; Michigan State University; the National Association of Schools of Public Affairs and Administration (reviewed with the material from the Development Project Management Center of the U.S. Department of Agriculture); Ohio State University; and the University of Wisconsin at Madison (Area Development and Regional Planning). I reviewed all of the documents from these cooperative agreements that are cited in the bibliographies of the individual reports in Section II. The generalizations in the following pages are based on that body of literature.

avored concept. And only rarely did authors provide enough information on sampling, data sources, and the like to permit an independent evaluation of methodological quality by outside observers.

This paper proposes that future AID cooperative agreements involving social science research give explicit attention to methodological standards. Toward that end the following discussion will review the key methodological issues raised by research conducted under the agreements at hand and suggest guidelines for dealing with those issues. The aim is not to construct a methodological monolith encompassing all projects regardless of their purpose, but to recommend ways of improving research within the opportunities and constraints available.

Some prefatory comments should be made on the likely constraints in research done under cooperative agreements. Probably the greatest single constraint is the need to balance the ideal requirements of research against the operational needs of AID. Working from the theoretical standards of sampling theory, a social scientist may wish to test a critical hypothesis with a sample of ten countries in four different geographic areas. But discussions with AID lead to the conclusion that for practical reasons, such as the absence of an AID mission or political obstacles to field research, studies can be carried out in only four countries, all in Africa and Asia. Researchers may also find they have to modify their study designs to accommodate the specific interests of AID or of collaborating host country nationals. Such constraints will be a fact of life in most cooperative agreements.

But even within the limits imposed by the AID environment and host country conditions there is no reason why social science research cannot follow the highest standards permitted by the situation. If samples of households or of individuals are to be drawn, they should, to the extent possible, be based on the procedures for probability sampling unless there are compelling reasons to the contrary. If the study is to use quantitative coding, commonly accepted standards for coding reliability should be adopted. Further, AID itself should recognize that there can be high costs to low quality research, especially if the results are used to develop new paradigms of development thinking or to reshape policies. In many cases a combination of AID insistence on methodological quality and a willingness to bend some of its own requirements could lead to improved research at little or no additional cost. And, if the present sample of cooperative agreements is any indication of the future, there will often be considerable latitude for quality research if the investigators are prepared to use it.<sup>2</sup> Some of the methodological difficulties to be noted did grow out of restrictions from AID, but more were the result of decisions under the control of the researchers themselves.

The following discussion will open by considering two issues with significant ramifications for methodological quality at many points: normative judgments and conceptual clarity. The paper will

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<sup>2</sup>Several studies of high methodological quality were carried out, for instance, under the cooperative agreements with Cornell University, Michigan State University, and Ohio State University.

then take up two broad questions of methodology and then move on to six specific areas in which standards are needed.

## 2. Normative Judgments and Conceptual Clarity

### a. Normative Judgments and Advocacy

In studies related to public policy it is easy for investigators to merge value preferences and empirical analysis in ways that are detrimental to sound research. The greatest danger is that a priori commitment to a particular concept, policy line, or social cause will undermine research objectivity at every point, from the conceptualization of the problem to the final presentation of results and recommendations.<sup>3</sup> Sample selection, questionnaire construction, the choice of cases, the design of codes, and other research procedures can all be slanted toward a preferred pattern of results. A further danger is that the investigators themselves become so involved in advocacy that they fail to distinguish between facts and values, between what they find as scientists and what they hope to find as advocates.

In the studies under review, most authors made a reasonable effort to maintain scholarly objectivity and, when normative judgments were made, to indicate the bases of those judgments. But there was also some confusion. At one extreme was an author who railed against value judgments about agricultural credit and in the process made several of his own. He wrote that "the arguments used to justify

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<sup>3</sup>This point is developed in Warwick and Pettigrew (1983).

cheap agricultural credit are unsound, based on value judgments, go counter to economic logic, and/or are not supported by empirical evidence" (Adams 1981:20). This statement makes the judgment that economic logic and empirical evidence are good and that value judgments as well as noneconomic arguments for cheap credit are bad. The ultimate irony is that the rejection of value judgments itself entails value judgment and thus can be dismissed on its own terms.

More serious problems arose in papers that fused empirical findings and normative judgments. In an essay on women and participation, one author adopted an unmistakable but also unstated value position—that women's interests should be promoted by all rural development programs (Staudt, 1979). Since this is an ethical premise rather than an empirical conclusion, it requires derivation from underlying principles and commentary on the strengths and limits of its claims. Given that other actors, notably men, can also make moral claims on development programs, and given that these claims can sometimes come into conflict, it is important to know on what they are premised and how they should be reconciled.

From the standpoint of research methodology the main difficulty with such advocacy is that it blurs the lines between what is preferred in principle and what is observed in research. At one point the author writes: "Credit programs ought to consider making loans available for other forms of collateral which give women access, and land reform programs ought to grant title to the tiller and to marital partners" (Staudt, 1979:40). Yet the paper does not indicate

precisely why the recommended action should be taken. More generally the interweaving of factual observation and value preference makes it difficult to know when the essay is summarizing empirical findings about women and when it is advocating ways of increasing women's participation in development programs. Similar difficulties were seen in other reports.

The question of values arises very sharply with such concepts as "participation" and "people-centered planning." Time and again papers from the Cornell project state or imply that participation is a moral good. But nowhere in this set of essays is there systematic discussion of why participation has value and what limits might be placed on that value by other personal and social goods. Concepts such as "people-centered planning" pose even greater difficulties, for they rely on what philosophers call persuasive definition (Carner and Korten, 1982). The very way in which the concept is stated induces readers to accept its value irrespective of any empirical evidence about its operation. Concepts such as "learning process" and "bureaucratic reorientation" likewise carry a freight of values in their phrasing (Korten and Uphoff, 1981). Who, after all, is going to be against planning that serves people, processes that promote learning, and the reshaping of (sluggish) bureaucracies?

The following guidelines may be helpful for research undertaken in future cooperative agreements. Their aim is not to purge values from social scientific research, for that is impossible, but to ensure a reasonable separation between normative judgments and empirical findings.

Guideline 1. Researchers should draw a clear distinction between the normative and empirical components of their studies. When a study introduces normative judgments or engages in advocacy, their moral bases should be explicitly stated and they should not be merged into or identified with empirical findings.

Guideline 2. The concepts used in social scientific research should not rely on persuasive definition or otherwise prejudice matters that are properly the subject of empirical investigation. When a value-laden concept is used, explicit attention should be given to the moral judgments behind it.

b. Conceptual Clarity

This review underscores the close association between the quality of conceptualization and quality of research methodology. A clear definition of concepts and a detailed specification of their major elements have three principal advantages for subsequent states of research. First, they help to prevent an overly aggregated study of the phenomena in question. As the Cornell project makes plain, any study that approaches the question of rural development participation without specifying its meaning and components would run the risk of excessive generality and arbitrariness in research design. The concept of participation is so vast in its reach and so multidimensional in its composition that it must be narrowed and disaggregated before meaningful investigation can take place. Second, specification of the major elements and dimensions of a concept sets the stage for the development of overall as well as specific indicators. Third, clear conceptual definition facilitates comparable research by different investigators. A common difficulty in social science



research is that different scholars define the same concept in different ways and this provides little or no basis for comparing their findings.

The present research likewise illustrates the drawbacks of diffuse and poorly defined concepts. In the paper on "people-centered planning," not only were other approaches damned by definition, but the precise meaning of the phrase itself remained unclear (Carner and Korten, 1982). In the case presented, an AID mission went through a series of intensive discussions leading to a revised Country Development Strategy Statement. The new version was called people-centered because it sought to target development assistance toward poor households. But since the brainstorming sessions were all held in the AID mission and involved no direct participation by the intended beneficiaries, the process could just as well have been labeled "donor-centered planning." Similar problems arise with the concept of "bureaucratic reorientation," which is more an agenda for action than a workable guide to research (see Korten and Uphoff, 1981). The practical difficulty with such an ill-specified concept is in knowing when a bureaucracy has been properly reoriented and when it has not.

Observance of two guidelines could help to improve the quality of research methodology:

Guideline 3. Studies should avoid highly aggregated or diffuse concepts whose operational meaning is subject to widely varying definition and interpretation. Concepts should be as precise as possible and be explicitly defined.

Guideline 4. When a key concept contains a number of separate elements or dimensions, these should be spelled out and ideally defined. In studies involving quantitative measurement, conceptual definitions should be linked directly to their corresponding empirical indicators.

### 3. Methodology and Research Design: General Issues

One of the most serious limitations of the studies conducted under the eight cooperative agreements was the paucity of information reported about data sources and research procedures. The following statement from one of the papers could be generalized to the entire set under review: "In presenting results, researchers generally have devoted little space to justifying the approaches they followed in collecting and analyzing survey data. But the choice of data collection and analysis procedures may importantly influence survey results (Eicher and Baker, 1982:76).

The problem of insufficient information was particularly acute with studies based on survey research, but arose with other data sources as well. In reports on surveys, authors almost never provided enough detail for independent judges to evaluate the quality of research design, sampling, questionnaire construction, field interviewing, and coding. The same was true with several studies using cases. Typically neither the specific rationale for case selection nor the quality of the data used to construct the case studies were given explicit attention. This experience suggests the following guideline for future studies:

Guideline 5. Research reports should provide specific information about both data sources and the quality of those sources. In studies based on sample surveys, information should be reported on sample selection, questionnaires or interview schedules, coding and analysis procedures, and, where relevant, the error of estimates. For research drawing on other data sources, detail should be supplied on the type and quality of the information used and on analysis procedures. In general, final reports should have enough information about methodology and data sources to permit an independent assessment of quality.

A second broad issue concerns the purpose of the research carried out through cooperative agreements. It was clear that different authors had different aims in mind, including: (1) conceptual clarification and specification, as in some of the Cornell project's work on participation; (2) arriving at empirical generalizations on the basis of literature reviews; (3) development of new methodologies, such as farm systems research, suited to the distinct needs of practitioners; (4) devising operational guidelines or "actionable hypotheses" for work on rural development; (5) investigating specific phenomena or testing hypotheses through field research; (6) building or testing models; (7) conceptual advocacy, as in the reports on "learning processes" and "bureaucratic reorientation"; and (8) policy advocacy, such as in some of the work on women's participation and rural credit.

Two problems are posed by this array of objectives. The most serious occurs when authors fail to indicate their purpose in conducting a given study. For example, in reading a paper on the learning process model (Korten and Uphoff, 1981), one might think that

its purpose was an objective analysis of the evidence for and against that approach. Closer analysis shows, however, that the apparent purpose was more advocacy than objective assessment. There would be no problem if the aim of advocacy were explicitly mentioned in the opening paragraphs, but when it is not, readers might be led to think that it is social science research in the more common sense. A second difficulty is that some authors mix different objectives in the same study without sufficient attention to their relative weight and the problem of trade-offs. Thus, to judge from the literature reviewed here, farm systems research hopes at one and the same time to be an empirically sound means of studying farmer needs and a vehicle for directly improving agricultural production (see Gilbert, Norman, and Winch, 1980:esp. 1-5). But there will often be situations in which the norms of correct methodology will clash with the desire for rapid action. Indeed, one paper on this subject notes that "ultimately, FSR will be judged less by the 'correctness' of its methodology than by how much it contributes to rural and agricultural development" (Gilbert, Norman, and Winch, 1980:84). But that statement begs the question of how methodological quality relates to the desired development. It could well be that "quick and dirty" surveys leading to an inaccurate picture of farmer needs and practices will do less for agricultural development than more carefully designed studies with less immediate but more adequately based findings.

The question of research objectives is complex, particularly in studies concerned with policy. However, some of the difficulties observed here could be reduced by following these principles:

Guideline 6. Writers should openly and explicitly state the purpose or purposes of their studies. Such disclosure is particularly important when the study involves some form of advocacy and/or when readers may otherwise misjudge its true intent.

Guideline 7. When a study is pursuing two or more incompatible objectives, the report should comment on how those incompatibilities have been handled. Such commentary is particularly necessary when sacrifices in methodological quality have been made or are being recommended in the interests of speed or policy relevance.

#### 4. Methodology and Research Design: Specific Issues

The present review uncovered several areas of methodology and research design in which the quality of future research could be improved. In every case there were both positive and negative examples, but with enough of the latter to justify explicit attention to standards.

##### a. The Choice of Methodology and Research Design

One of the most basic questions of all centers on the approach to be used in conducting a study. Should the research be quantitative, qualitative, or a mixture of the two? Should case studies be used and, if so, how many and what kind? Should the study design be tight and rigorous or loose and exploratory? Are new data needed, or can the questions be answered through a review of existing information?

In AID-sponsored research, answers to these questions will depend on several factors: contractual demands and donor desires; country conditions; the personal preferences and professional competence of the investigators; funding; time; and the purpose of the

study. However, it is certainly in AID's interest to ensure that the design and methodology chosen are the most appropriate for the objectives and constraints at hand. For example, if a project's objectives call for specific quantitative answers to questions about the extent of rural poverty, and if there are sufficient time and resources to mount a high-quality study on that subject, the researchers should not be permitted to conduct a diffuse exploratory study yielding limited data of unknown generalizability. Similarly, if the purpose of a research project is to assess the advantages and limitations of certain management strategies for rural development, AID will not be well-served by a study showing only advantages in one or two projects.

Research completed under the cooperative agreements further shows the benefits of combining different data sources and methodological approaches in the same study. A fine example of methodological integration is a study carried out under the Cornell project on the rural water sector in Botswana (see Roe and Fortmann, 1982). The major data source was the Water Points Survey, which combined a survey of 458 rural households, in-depth field observation of water usage, and interviews with as well as observations of water management groups. It was evident throughout the research report, however, that the authors were drawing not only on those sources, but also on their own extensive experience in the country, related literature, and conversations with many persons outside the formal survey. A limited but growing literature on methodological integration provides

additional illustrations of the benefits of merging complementary data sources.<sup>4</sup>

Two guidelines can be proposed in this area:

Guideline 8. The choice of methodology and research design should be guided above all by the objectives and specific requirements of the study in question. Both the strategies for gathering information and the degree of rigor in study design should be adapted to the particular research questions involved. When the study will attempt to assign causality, the design chosen should be powerful enough to permit valid causal attribution.

Guideline 9. When possible and appropriate, the research design should include the collection of data by two or more complementary methodologies. Within such designs special efforts should be made to obtain independent assessments of key phenomena through different data sources.

b. Sampling

Every methodology and data source, from household surveys to library research, involves sampling. The essence of sampling is choosing some part of a larger body to represent the whole. While commonly identified with survey research, sampling also occurs when a researcher selects a given body of literature to review, picks a village for intensive observation, decides on one or more illustrative cases, chooses materials to be used in constructing case studies, or singles out certain historical periods and events for careful scrutiny. The key question posed by sampling in any sphere is whether the part chosen fairly represents the whole about which generaliza-

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<sup>4</sup>See, for instance, Blumer and Warwick (1983: Chapters 22 and 23).

tions will be made. Are the households, individuals, literature, cases, case materials, events, historical periods, or other units an adequate sample for the purposes of the research, or are they biased in important ways? If they are biased, the generalizations drawn should be suitably qualified so that they are not misleading.

Here the most general difficulty with sampling was failure to provide adequate information on the procedures used and on their rationale. Although the cooperative agreements gave rise to numerous sample surveys, in almost none of these studies was it possible to assess the technical quality of the samples used. The problems were even more serious with studies using cases, for there the question of sampling was typically ignored or passed over very lightly. Yet the rationale for choosing cases is every bit as much a question of sampling as the rationale for selecting households or individual respondents in a survey.

To judge from the limited evidence available, most researchers using surveys of households made some effort to apply accepted standards of probability sampling. However, in several cases they relaxed those standards when faced with difficulties of access in the field. It is hard to know whether those departures were really necessary and how they finally affected the representativeness of the samples actually used. In only one instance were totally unacceptable sampling procedures used in household surveys (Goldsmith and Blustain, 1980). In that case the researchers drove the enumerators to a specific district and instructed them to interview any four farmers.



They imposed no categories on the respondents to be approached; the interviewers could select small or large, young or old, male or female farmers. If this had been an exploratory study the costs would have been smaller, but it was not. The authors went on to use the resulting data to show differences in the two areas covered by the study. Given the low quality of the sampling procedures used, such comparisons had no foundation whatever in inferential statistics. The differences observed between the two areas may have been as much the result of varying predilections among the enumerators as of the conditions in those regions.

Research under future cooperative agreements could benefit from attention to these guidelines on sampling:

Guideline 10. Whatever the specific data source, the report should address the question of sampling. It should first indicate how the sample was chosen and the rationale for that procedure. Second, the report should discuss how the sampling strategy used affects the generalizability of the findings to the larger body about which conclusions will be drawn.

Guideline 11. To the extent possible, sample surveys should follow accepted norms of probability sampling at all stages. When practical obstacles make it difficult to apply these norms, the researchers should consult a sampling specialist about ways of working in that situation. In studies aiming to estimate population characteristics or to compare different groups from the same population, quota sampling should be avoided.

#### c. Use of Case Studies

Many of the publications under review drew on case studies to test hypotheses, illustrate key points, advocate an approach, or show a problem. At one extreme it was a study that gathered and system-

atically coded data on 150 local organizations in developing countries (Esman and Uphoff, 1982). The opposite end was anchored by an essay using just one case to document the advantages of a particular approach to planning. Between these extremes were studies that introduced several cases to serve the purposes of the research. The use of cases in social science research raises three basic questions for future cooperative agreements.

First, how should cases be chosen? Is it scientifically justifiable to use a single case to make a point when there are other cases from the same domain that would create a different picture? And if several cases are to be selected, what procedures should be adopted? There is obviously no categorical answer to either question, for much depends on the objectives of the research. If the aim of a study is conceptual advocacy, and if that aim is openly stated, one or two cases may be quite acceptable. But even there the investigators are obligated not only to disclose their intentions, but to indicate the sampling biases arising from the choice of cases. It is not acceptable to present a single positive case as if it were representative of a population in which there are many negative cases. More generally, two guiding principles can be suggested for case selection:

Guideline 12: The procedures used in selecting cases should yield a fair representation of the broader universe about which generalizations will be made. While it is not necessary to use probability sampling, the selection criteria should ensure reasonable coverage of diversity and avoid biased choice.

Guideline 13: In writing research reports involving cases, authors should indicate their selection procedures and the likely bias produced by those procedures.

Guideline 14: In studies aiming to generalize to a larger domain, the use of single case studies should be avoided when possible. Single cases are acceptable when the purpose of the research is to explain those cases or to develop hypotheses, but not when it seeks to generalize to a range of cases.

Second, how adequate is the information used to construct case studies? Although this point is often overlooked in discussions of cases, the problems of evidence in case writing are the same as for other kinds of research. For example, one study reviewed here sought to study implementation issues in integrated rural development by reviewing the experience of twenty-one AID projects (Crawford, 1981). The research methodology involved the preparation and then analysis of twenty-one cases. One limitation of the data sources for the cases was that they were based heavily on evaluations done for AID, including at least one evaluation completed by the contractor itself. It is well-known that such evaluation studies vary greatly in quality, candor, and coverage. The study would have been stronger had the authors commented explicitly on the adequacy of the information, noting apparent weaknesses, differences in quality, and instances in which the original evaluations were challenged. The discussion leaves the impression that the evidence for the cases was generally solid and not subject to significant disagreement.<sup>5</sup> Such a situation would be

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<sup>5</sup>Among the 21 projects reviewed there was at least one in which the information presented as the basis for the discussion was

most unusual with cases of this kind. In other studies drawing on cases it was virtually impossible to judge the quality of the information used for the simple reason that the sources were not identified. Observance of the following guidelines might help to avoid such difficulties in the future:

Guideline 15. The source(s) of information used in constructing case studies should be clearly identified and the quality of those sources should be discussed. If there are ambiguities in the data or disagreements over significant facts and interpretations, these should be noted in the report.

Guideline 16. The portrayal given in case studies should fairly reflect the situation being described. Case descriptions should not be slanted toward the views advocated by the researchers nor should they omit significant details.

Third, what are the criteria of success and failure used in judging a given case? Very often in policy-related studies cases are introduced to show the success or failure of a given approach. The danger is that to make their points, researchers will adopt overly simple criteria for judging success and failure. One author spoke glowingly of a project in a developing country and used it throughout his article to illustrate the virtues of a particular approach to management (Korten, 1982). Yet at no point did he indicate in precisely what ways the program was successful, nor did he mention any areas of weakness or failure. Difficulties of this kind might be reduced by attention to this guideline:

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challenged. This was the evaluation conducted by Development Alternatives, Incorporated, on the AID-sponsored Abyei project in the Sudan. Both the process followed and the substance of the findings brought strong objections from the Harvard Institute for International Development, the contractor for the project. While this is not the place to review the debate as such, the presentation of the Abyei case would have been more balanced had it noted the disagreements generated by the evaluation.

Guideline 17. When judgments are made about the success or failure of a given case, the specific criteria of success and failure should be mentioned. If multiple criteria are involved, the case should present information on all relevant criteria rather than offer a single aggregated assessment of success or failure.

d. Data Collection and Measurement

The entire area of data collection and measurement would normally be a rich topic for commentary in a review of research methodology. In this case, little can be said owing to the scant detail provided on those matters. Although it is often hard to know, from the indications available most researchers seemed to follow sound methods in gathering data and in developing key indicators of concepts. One study, in fact, went to commendable lengths in applying quantitative coding to qualitative materials on 150 organizations (Esman and Uphoff, 1982). The methodology would have been stronger, however, if the researchers had made two changes. First, steps should have been taken to control for the likely tendency of raters to form global judgment about organizations and then apply that judgment to the coding of specific variables. The high correlations reported between the coded "operational variables" and the code for total performance suggests the presence of this tendency. Second, given the importance of operational indicators of performance, the study should have developed some measure of performance that was independent of the other coding. If that was impossible, the authors could have mentioned that they tried to develop such a measure but that it was not feasible. This experience suggests the need for the following guideline:

Guideline 18. When the analysis relies heavily on a key criterion variable, such as a major indicator of success, failure, or effectiveness, the study should develop alternate measures based on different methods. Systematic discussion should also be provided of differences in the patterns of association between predictor variables and the different measures of the criterion variable.

Other measurement problems included a questionnaire relying too heavily on a "agree-disagree" format likely to produce response bias; a set of aggregate indicators whose links to the concepts in question should have been examined more closely; and a sample survey that involved a facile translation of complex concepts into empirical measures based on questionnaire data. None of these problems was frequent or serious enough to merit a separate guideline.

#### e. Analysis and Interpretation

In general, whether their information sources were quantitative or qualitative, bibliographic or observational, most authors were judicious in drawing conclusions, introduced appropriate qualifications, and adhered to accepted standards of scholarly inference. Some, in fact, were exceptionally careful in showing the limits of their data, and the implications of those limits for the interpretations that could be made. Nonetheless, this review brings out three problems in analysis and interpretation.

The first and most common is overgeneralization. Consider the following statements:

...the rural poor prefer amenities and do not perceive production as a high priority for group activity (Peterson, 1981:21).

American agronomists, however, have absolute faith in technology. They think they can teach modern methods to every cultivator, big or small, owner or sharecropper (Khan, 1978:29).

Government officials in the districts still maintain an air of superiority in dealing with rural people. They usually avoid social interaction with their "clientele" and do not participate in time-honored village activities and rituals (Rondinelli, n.d.:70). The ineffectiveness of central administration in East Africa can be attributed in part to the legacy of colonial domination and in part to deeply ingrained cultural traditions (Rondinelli, n.d.:74).

In the first case the author presents no evidence to support such a blanket generalization about the preferences of the rural poor. The second statement is apparently based on casual observation of American agronomists rather than on any systematic research. The source of information for the third statement was not identified, but apparently was a combination of library research and hearsay rather than first-hand observation. And in the discussion containing the final statement the author neither established that central administrations in East Africa were ineffective nor presented any specific evidence on the putative causes of ineffectiveness. Such generalizations suggest the need for this guideline:

Guideline 19. Generalizations made on the basis of research should not extend beyond the limits of the information available and should respect the nuances and complexities of that information. Particular care should be taken in generalizing about a population from data based on a sample.

A second problem is oversimplified portrayals of complex situations. Two examples can be cited from the present body of

research. The first is a paper attempting to show the presumably universal value of a "learning process" model (Korten, 1982). The oversimplification begins with the concept itself, which is never precisely defined, and continues through the presentation of case material and conclusions. The evidence in support of the approach derives mainly from the experience of an irrigation agency in an Asian country. To judge from the case material this agency has been an unqualified success, and much of the success was brought about by the application of a "learning process" model. Yet the author never indicates in exactly what areas this agency has been successful nor does he show the specific ways in which learning processes contributed to that success. Interestingly, when I asked knowledgeable officials in the World Bank about that irrigation agency they replied that it was generally successful but did have some problem areas. Further, they felt that the primary reason for success was the presence of a strong leader who knew how to navigate in the complex bureaucratic environment of that country. A balanced presentation on the "learning process" model would have noted some of its drawbacks as well as strengths and cited cases in which attempts to apply it produced mixed or negative results. If there were no such cases, discussion would be in order on the limits of the positive case(s), including other conditions, such as strong leadership, that may have contributed to success.

The second example of oversimplification comes from an otherwise careful study of water use in an African country (Roe and



Fortmann, 1982). In a chapter exploring the views of public officials in that country the authors present a picture that is considerably less nuanced than earlier depictions of farmers. Though they speak of "widely held perceptions" among these officials, they present no solid evidence to show the existence of such perceptions. The result is a characterization that borders on the stereotypical. This example illustrates the need not only for nuanced analysis in general but for even-handedness in the portrayal of different groups in the same study. Future studies might avoid these difficulties by observing this guideline:

Guideline 20. The characterizations made of individuals, groups, programs, organizations, situations, or events should be faithful to the variations and complexities involved. Special efforts should be made to present a differentiated picture of complex phenomena and to avoid condensations or simplifications that may be misleading.

A third problem in this field is a confusion between marshalling and weighing evidence. While most authors seemed committed to presenting a fair picture of the evidence at hand, a few saw their task as winning readers to a particular point of view. The result was presentations that accentuated the positive and often omitted the negative. The clearest cases were the papers advocating "learning process models" and "people-centered planning," but there were also others. The guidelines below address this problem:

Guideline 21. Authors should normally present a balanced picture of the evidence available on a given point. Evidence in support of the researcher's favored hypotheses should not be overstated nor should negative evidence be omitted.

Guideline 22. If a writer chooses to engage in advocacy, that intention should be openly disclosed so that readers may make their own assessments of its effects. Even when the author's purpose is advocacy, the state of the evidence should not be misrepresented in the interests of persuasion.

f. Issuing Policy Recommendations

Many of the studies covered here came out with policy recommendations or other practical advice. Two questions can be raised about this process. First, are the recommendations supported by the research evidence at hand? For example, one of the papers reviewed dealt with new approaches to training. Citing "experience with training institutes and training activities in Africa, Asia, and Latin America," the authors listed six common weaknesses of training programs and recommended an alternative based on "action orientation" and an "enhancement approach" (Honadle and Hannah, 1981).

Generalizations were put forth with no specific reference to geographic regions, the content of training, its objectives, or different institutional settings. In this case the empirical basis for the recommendations seemed shaky, at best. Second, to what extent do the recommendations or policy advice derive from normative principles? Time and again in these papers authors used words such as "should" and "ought" without indicating their referents. In many instances it was clear that the "should" or "ought" referred to a priori moral judgments rather than to the implications of empirical data. The significant point is that authors should clearly indicate how and why they reached their policy recommendations. In particular,

readers should not be misled into thinking that a given recommendation flows from empirical data when, in fact, it is mainly a reflection of the author's own value preferences. Empirical data can never show that a certain line of action should be taken. The most that they can do is indicate that by some criterion there is a pattern, a problem, a shared desire, or an opportunity. Recommendations to move ahead in any area always involve a value judgment, including the judgment that some action is needed.

Future studies could reduce the confusion over policy recommendations by applying these principles:

Guideline 23. When research reports include policy recommendations they should lay out the empirical bases for those recommendations. The discussion should indicate how, specifically, the research findings relate to the recommendations.

Guideline 24. Authors should clearly indicate the normative bases of their recommendations. They should be explicit about the values and principles behind the recommendations and offer some commentary of why those values and principles apply to the situation in question.

## 5. Concluding Comments

This review suggests two other ways in which AID might improve the quality of social science research in cooperative agreements. One is to establish a panel of methodological experts to advise on questions of research quality at the time a cooperative agreement is being worked out and later as necessary. The panel could include specialists on survey research, econometrics, participant observation, case writing, and other relevant approaches. Through some careful

questioning before a cooperative agreement is launched and some selective monitoring later, these specialists might, at a relatively low cost, help to prevent some of the more common methodological lapses and errors.

Second, through the panel suggested or some other means, AID could take action to increase the familiarity of scholars who use survey research with the literature in that field. In the studies reviewed here many specific projects turned to the sample survey either as the sole source of empirical data or as a major research component. Yet it was clear that many users were not familiar with the basic works in the field. The net result was typically not disaster, but it was research of lower quality than necessary. One of the clearest signs of unfamiliarity with the literature is the meager detail provided on sampling, questionnaire construction, coding, and other aspects of survey methodology. Even authors who wrote with insight and competence on the problems of survey research cited none of the main works in the field. As a minimum, AID might recommend that future users of surveys in cooperative agreements become familiar with one or more of the basic texts in that field. These include:

E. Babbie, Survey Research Methods (Wadsworth Publishing Company, 1973).

C.A. Moser and G. Kalton, Survey Methods in Social Investigation (Heinemann, 1971).

D. Warwick and C. Lininger, The Sample Survey: Theory and Practice (McGraw-Hill, 1975).

Survey research practitioners could also profit from consulting works on sampling, such as L. Kish, Survey Sampling (Wiley, 1965), and various books dealing with questionnaire writing, especially S. Payne's small classic, The Art of Asking Questions (Princeton University Press, 1951).

SUMMARY**GUIDELINES FOR IMPROVING RESEARCH METHODOLOGY  
IN AID COOPERATIVE AGREEMENTS****A. Normative Judgments and Conceptual Clarity**

1. Researchers should draw a clear distinction between the normative and empirical components of their studies. When a study introduces normative judgments or engages in advocacy, their moral bases should be explicitly stated and they should not be merged into or identified with empirical findings.
2. The concepts used in social scientific research should not rely on persuasive definition or otherwise prejudge matters that are properly the subject of empirical investigation. When a value-laden concept is used, explicit attention should be given to the moral judgments behind it.
3. Studies should avoid highly aggregated or diffuse concepts whose operational meaning is subject to widely varying definition and interpretation. Concepts should be as precise as possible and be explicitly defined.
4. When a key concept contains a number of separate elements or dimensions, they should be spelled out and ideally defined. In studies involving quantitative measurement, conceptual definitions should be linked directly to their corresponding empirical indicators.

**B. Methodology and Research Design: General Issues**

5. Research reports should provide specific information about both data sources and the quality of those sources. In studies based on sample surveys, information should be reported on sample selection, questionnaires or interview schedules, coding and analysis procedures, and, where relevant, the error of estimates. For research drawing on other data sources, detail should be supplied on the type and quality of the information used and on analysis procedures. In general, final reports should have enough information about methodology and data sources to permit an independent assessment of quality.
6. Writers should openly and explicitly state the purpose or purposes of their studies. Such disclosure is particularly

important when the study involves some form of advocacy and/or when readers may otherwise misjudge its true intent.

7. When a study is pursuing two or more incompatible objectives, the report should comment on how those incompatibilities have been handled. Such commentary is particularly necessary when sacrifices in methodological quality have been made or are being recommended in the interests of speed or policy relevance.

C. Methodology and Research Design: Specific Issues

8. The choice of methodology and research design should be guided above all by the objectives and specific requirements of the study in question. Both the strategies for gathering information and the degree of rigor in study design should be adapted to the particular research questions involved. When the study will attempt to assign causality, the design chosen should be powerful enough to permit valid causal attribution.
9. When possible and appropriate, the research design should include the collection of data by two or more complementary methodologies. Within such designs special efforts should be made to obtain independent assessments of key phenomena through different data sources.
10. Whatever the specific data source, the report should address the question of sampling. It should first indicate how the sample was chosen and the rationale for that procedure. Second, the report should discuss how the sampling strategy used affects the generalizability of the findings to the larger body about which conclusions will be drawn.
11. To the extent possible, sample surveys should follow accepted norms of probability sampling at all stages. When practical obstacles make it difficult to apply these norms, the researchers should consult a sampling specialist about ways of working in that situation. In studies aiming to estimate population characteristics or to compare different groups from the same population, quota sampling should be avoided.
12. The procedures used in selecting cases should yield a fair representation of the broader universe about which generalizations will be made. While it is not necessary to use probability sampling, the selection criteria should ensure reasonable coverage of diversity and avoid biased choice.
13. In writing research reports involving cases, authors should indicate their selection procedures and the likely bias produced by those procedures.

14. In studies aiming to generalize to a larger domain, the use of single case studies should be avoided when possible. Single cases are acceptable when the purpose of the research is to explain those cases or to develop hypotheses, but not when it seeks to generalize to a range of cases.
15. The source(s) of information used in constructing case studies should be clearly identified and the quality of those sources should be discussed. If there are ambiguities in the data or disagreements over significant facts and interpretations, these should be noted in the report.
16. The portrayal given in case studies should fairly reflect the situation being described. Case descriptions should not be slanted toward the views advocated by the researchers nor should they omit significant details.
17. When judgments are made about the success or failure of a given case, the specific criteria of success and failure should be mentioned. If multiple criteria are involved, the case should present information on all relevant criteria rather than offer a single aggregated assessment of success or failure.
18. When the analysis relies heavily on a key criterion variable, such as a major indicator of success, failure, or effectiveness, the study should develop alternate measures based on different methods. Systematic discussion should also be provided of differences in the patterns of association between predictor variables and the different measures of the criterion variable.
19. Generalizations made on the basis of research should not extend beyond the limits of the information available and should respect the nuances and complexities of that information. Particular care should be taken in generalizing about a population from data based on a sample.
20. The characterizations made of individuals, groups, programs, organizations, situations, or events should be faithful to the variations and complexities involved. Special efforts should be made to present a differentiated picture of complex phenomena and to avoid condensations or simplifications that may be misleading.
21. Authors should normally present a balanced picture of the evidence available on a given point. Evidence in support of the researcher's favored hypotheses should not be overstated nor should negative evidence be omitted.



22. If a writer chooses to engage in advocacy, that intention should be openly disclosed so that readers may make their own assessments of its effects. Even when the author's purpose is advocacy, the state of the evidence should not be misrepresented in the interests of persuasion.
23. When research reports include policy recommendations, they should lay out the empirical bases for those recommendations. The discussion should indicate how, specifically, the research findings relate to the recommendations.
24. Authors should clearly indicate the normative bases of their recommendations. They should be explicit about the values and principles behind the recommendations and offer some commentary on why those values and principles apply to the situation in question.

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SPECIAL REPORT BIMPROVING THE UTILITY OF AID-SPONSORED RESEARCH

John D. Montgomery

1. Developing a Test for Research Utility

So much has been written about the distinction between pure (basic) and applied (operational) research that the distinction itself has become a meaningless exercise in definition. Thus, I believe it to be pointless to distinguish between "pure" and "applied" research, and impractical to discover which research is used and which is not. As a more useful and practical step, I suggest applying a "decision overlay" to research products to see if they are potentially useable. The "overlay" is not self-administering, but I believe its results would be replicable if a few simple rules are applied.

First, the questions to be asked are as follows: Does a given element of knowledge or new insight contribute to improved policy? More precisely, what are the potential uses of a given research output in the specific contexts in which AID operates? How would the knowledge produced by a research contract (1) change a preference or style of operation of an individual or group whose behavior is relevant to AID's mission? or (2) reaffirm a doubtful or challenged preference or style of operation for such decision makers?

Second, the actors to whom AID-sponsored research is expected to be useful are to be defined as follows:

- I. AID Washington (AID/W), which is presumed to have responsibility for making allocations among countries and sectors;
- II. an AID field mission (USAID), which is presumed to have responsibility for making allocations among programs and projects, for offering technical advice, and for exercising some influence on policies to be pursued by the country in which it is located;
- III. national governments receiving U.S. aid (HG), which have allocative responsibilities but which also bear operating responsibilities for the management of projects and the implementation of programs; and
- IV. project managers and their staff of administrators and technicians (Proj), who are responsible for micro level decisions affecting their jurisdictions.

## 2. Applying the "Decision Overlay"

I screened a sample of 32 products of AID cooperative agreements in a first application of the proposed test of policy relevance. The judgments rendered in each case are, of course, my own.

Judgments about policy usefulness are subject to challenge and also to misinterpretation. A policy finding that one potential user might consider a radical departure from his practice, another might consider a confirmation; what is useful background to a new employee at any level of decision making might be insultingly obvious to an old hand. Moreover, different readers might find different policy implications from these published reports, since they are rarely stated as directly as the summary in Part III implies. The reports themselves use a wildly different base of factual data, ranging from an individual's experience as a scholar or consultant to quantitative

analysis of scores of closely observed project operations. If the authors of each report had been asked to state the policy findings as they applied to an identified individual occupying each of the four levels of decision making, and then to a different individual decision maker, also designated by name, and the operation had continued until a sufficient sample had been taken, we could be sure of the results.

But for present purposes the summary is adequate. I am probably not much better or worse informed than a reasonably experienced administrator about the somewhat esoteric subjects considered in these research reports. The reports summarized here were chosen as major outputs of AID's sponsored research in rural development and development administration in the past half dozen years or so, and thus give a current cross section of the work of one important office in the Agency. They range in length from 7 to 315 pages; some are the work of a single author and a product of a few days' time, sometimes summarizing years of field experience; but others are heavily documented and show signs of many months spent in a library or directly gathering data in the field. The proportion of pages devoted to stating and explaining policy findings is greater for short than for long reports, but the credibility of those pages is probably the reverse. The range of the policy findings is equally varied, from advice on how to structure decentralized operations and what kinds of price and interest policies to set for rural credit services, to how to conduct training courses and motivate field staff members. Very few of the policy findings are self-evident; they have

been violated in practice, usually with relatively poor results, as the research itself demonstrates. Distributing these reports, or finding even more effective means of disseminating their contents, would therefore improve development practice in some instances (though not all "poor" policy is avoidable).

Most of the reports offer advice that seems appropriate to "governments" (31 of the 32 apply to decision makers in HG); the decision making level next most frequently addressed is that of the AID mission (19 of the 32); project managers are likely to be interested in only 8 of the reports; and AID Washington only 7, except indirectly because of its participation in negotiations with field missions and host governments.

### 3. Findings

There were eight contractors whose work was sampled for this evaluation:

- a. University of California, Berkeley: Project on Managing Decentralization (seven reports);
- b. Development Alternatives, Inc.: Project on Organization and Administration of Integrated Rural Development (four reports);
- c. Michigan State University: Off-Farm Employment Project (two reports);
- d. Ohio State University: Agricultural Finance Program (two reports);
- e. University of Wisconsin: Regional Planning and Area Development (two reports);
- f. Cornell University: Project on Rural Development Participation (three reports);



- g. National Association of Schools of Public Affairs and Administration and Development Project Management Center: Cooperative Agreement on Social Development Management (twelve reports).

The policy findings from these reports are summarized below with the potential decision maker designated in Roman numerals as defined in the first part of this report.

a. The Berkeley Reports

- 1. Stephen S. Cohen, John W. Dyckman, Erica Schoenberger, and Charles R. Downs, Decentralization: A Framework for Policy Analysis (1981).

A "state-of-the-art" paper defining the functions and roles of decentralization and citing current literature describing experience with it. (150 pages)

Policy Finding No. 1: When governments decentralize decision making to local organizations in order to relieve administrative overload, they should insure that the decisions to be transferred to local authorities would otherwise have to be made at central levels. (p. 11) User: HG with some implications for Proj.

Policy Finding No. 2: When governments decentralize the functions of allocating services, they should prepare to deal with new clients and therefore to encounter difficulties in consistency of implementation, coordination, and control, and thus in the long run add to the burdens of central bureaucracy. (pp. 12-15) User: HG.

- 2. David K. Leonard and Dale Rogers Marshall, eds., Institutions of Rural Development for the Poor (Berkeley: University of California, Institute of International Studies, 1982).

Chapter 1 (by Leonard), pp. 1-39.

The bulk of the chapter is devoted to analytical taxonomies to be applied in diagnosing situational requirements.

Policy Finding: Programs intended to reach the rural poor should be designed to reduce their vulnerability to capture by elites including national agencies with variable interests. The recommended strategy: to distribute functions between national and local organizations. (19 pages, 8-27, most of which are devoted to explaining how these analyses should be carried out by designers, with examples and illustrations of suggested procedures.) Users: AID/W; USAID; HG.

3. Same, Chapter 2 (D.R. Marshall), pp. 40-72.

Most of this chapter illustrates from the U.S. poverty programs the categories of linkages defined in Chapter 1.

Policy Finding: In decentralizing programs to local organizations, governments should transfer resources subject to retention of control, including shared responsibility, by central agencies. (3 pages, approximately, passim.) Users: AID/W; USAID; HG.

4. Same, Chapter 3 (Peterson), pp. 73-124.

Like the other chapters in this book, much descriptive material drawn from different sources illustrates the operational implications of these guidelines.

Policy Finding No. 1: States that introduce cooperative mechanisms as a means of aiding the poor should structure them so that their internal administrative leaders are responsive to farmer members. (Approximately 3 pages, 79-81) Users: HG, Proj.

Policy Finding No. 2: Marketing cooperatives should not be used for food products if they are intended to benefit the small scale rural sector. (2 pages, 84-85) Users: HG, Proj.

Policy Finding No. 3: The issuance of credit through cooperatives should be structured to encourage existing informal or market-based sources rather than as a substitute for them; subsidies that do not have this effect should be avoided if the purpose is to target small farmers. (2 pages, 93-94) Users: HG, Proj.

Policy Finding No. 4: Production cooperatives should be small (fewer than 100 families at most) and subject only to indirect state controls. (5 pages, 99-111, passim.) Users: HG, Proj.

5. Same, Chapter 4 (Peterson), pp. 125-150.

Policy Finding No. 1: Cooperative organizations should have a concrete goal, a single task, depend relatively little on nonfarmer skills, focus on tasks that cannot be carried out by individuals, produce goods or services for which a demand exists, stand in isolation from other groups, and follow informal organizational procedures. (2 pages, 126-127) Users: HG, Proj.

Policy Finding No. 2: Governments establishing cooperatives to benefit the poor should concentrate their resources on single-purpose organizations where an uncongenial local political environment exists. (3 pages, 134-137) Users: HG, Proj.

Policy Finding No. 3: The effort to provide services to the rural poor ought to permit redundancy among organizations so that farmers can choose among them and induce competition for their support. (2 pages, 141-143) Users: HG, Proj.

Policy Finding No. 4: Government support to productivity-oriented local organizations should provide funds at the first stage of developing effectiveness, personnel at the second stage of improving effectiveness, and add management and staff at the third and fourth stages of expansion and termination. (3 pages, 144-146) Users: HG, Proj.

6. Same, Chapter 5 (Steinbo), pp. 151-192.

The chapter contains descriptions of many different models and approaches to para-medical operations.

Policy Finding No. 1: Governments should provide for sustained linkages to the communities involved in public health programs. (2 pages, 158-159) User: HG.

Policy Finding No. 2: Governments should use para-professionals drawn from local communities as peripheral supplements to normal medical care. (2 pages, 170-171) User: HG.

7. Same, Chapter 6 (Leonard), pp. 193-224.

Policy Finding No. 1: Governments using administrative instruments to serve the rural poor should also encourage the development of local organizations with parallel functions and purposes as a corrective to pathologies of bureaucratic interventions. (2 pages, 196-198) User: HG.

Policy Finding No. 2: Governments and donors providing capital assistance to local organizations should incorporate plans for generating recurrent incomes at the design stage in terms of a planned withdrawal of operating assistance. (2 pages, 202-204) Users: AID/W; USAID; HG.

b. The DAI Reports

1. George W. Honadle, Fishing for Sustainability: The Role of Capacity Building in Development June, 1981. (96 pages)

Examples of organization experiences making use of village capacity occupy 33 pages, 55-88.

Policy Finding No. 1: Governments should build the capacity of existing institutions to serve developmental objectives before undertaking to organize new institutions. (2 pages, 6-8) Users: USAID; HG.

Policy Finding No. 2: Governments engaged in administrative reform to improve the capacity of public organizations should design incentives to induce each important element of the system to accept the change. (2 pages, 27-28) User: HG.

Policy Finding No. 3: Administrative reform should start simple, focus on structural constraints, progress incrementally, respond to new demands as they occur, and make use of projects and learning laboratories. (1 page, 44) User: HG.

2. George W. Honadle and John P. Hannah, Management Performance for Rural Development (April, 1982). (25 pages)

Policy Finding: Governments should use training resources as instruments for organizational development wherever possible, involving staff members at all levels in the development of solutions toward existing problems on site, as opposed to existing prepackaged, single-level training programs. (2 pages, with detailed descriptions of an illustration in Jamaica) Users: USAID; HG.

3. Paul R. Crawford, Implementation Issues in Integrated Rural Development: A Review of Twenty-One USAID Projects (Development Alternatives, Inc., May 4, 1981). (133 pages)

An interim report aimed at identifying organizational problems in IRD projects and alternative solutions. The analysis is based on 21 AID-supported projects in Asia, Africa, and Latin America, with documentation supplied from AID reports. Each project is discussed in terms of administrative difficulties reported in earlier evaluations, with some attention to possible remedies. A chapter is devoted to summarizing the political, economic, and environmental constraints of the projects.

Most of the administrative case studies are taken from evaluation and other reports drawn up for different purposes than the generation of general policy guidelines. Consequently the issues tackled are unique to each project and probably reflect the idiosyncratic views of the original evaluators. Thus the information is both project-specific and of somewhat doubtful generalizability. The report is essentially, therefore, a source book rather than either operational or policy research. It is a prelude to research rather than the actual performance of it.

Policy Finding No. 1: Governments should structure IRD projects to maximize participation by the intended beneficiaries; should make use of existing organizations where possible; should keep the tasks assigned to participating organizations simple and closely related to the interests of the members, and should design incentives to encourage staff attention

to beneficiaries' needs and responses. (About 3 or 4 pages, passim, the rest being devoted to descriptions and summaries of findings) Users: USAID; HG.

Policy Finding No. 2: International donors should avoid contracting with universities for projects in IRD because of their lack of experience, inappropriate incentive structure, low staff motivation, and structural features of project management. (3 pages, 100-104, passim) Users: AID/W, USAID.

Policy Finding No. 3: Training offices should direct major efforts toward on-the-spot training derived from identified needs of the trainees, allowing for revision of training materials and subjects and the introduction of materials drawn from actual on-the-job experiences. (1 page, 111) Users: USAID; HG; Proj.

Policy Finding No. 4: Activities requiring investments or commitments (especially nonfinancial) by beneficiaries in IRD projects should be introduced wherever projects are expected to be self-continuing. (1 page, around 122-123) Users: HG, Proj.

4. Development Alternatives, Inc., Making Rural Development Self-Sustaining: A Guide for Project Planners and Managers (November, 1982). (174 pages)

The paper is intended to be a brief topic-by-topic summary of existing knowledge, drawn mostly from DAI field studies, about IRD. Its policy recommendations are intended to be factual and descriptive (e.g., "political support is necessary for a project") rather than specific and operational. It contains warnings ("the need for quick, visible results, for example, may lead to the introduction of expensive service delivery systems or technologies that cannot be maintained..."). (Both quotations are from page 8.) It is explanatory and illustrative rather than normative. Most of the recommendations are given in the form of requirements, stated rather abstractly. ("The first requirement is that the sustainability be considered throughout project design, implementation, and evaluation process.") (page 11).

Each subsection (sustainability, information gathering, adjusting to external constraints,

adjusting to financial constraints, and developing organizational capacity) contains a set of guidelines or key questions to be considered by designers and managers. Most of the specific guidelines are of the "how to" variety: how to use written records for information, how to conduct the Delphi method of consensus building, etc. There are many lists of advantages and disadvantages, questions and assumptions, and formal alternatives to be considered by operating managers.

The clear intent is to provide the fruits of basic or operational research in handy form rather than to justify or define specific policy preferences.

#### c. The MSU Reports

1. Enyinna Chuta and Carl Liedholm, Non-Farm Employment: Review of the State of the Art, (1979). (85 pages)

The first twenty pages of the report describe the dimensions of the problem; the next twenty pages describe the linkages between off-farm employment and other elements of the economy. The next ten pages discuss the economic consequences of increased off-farm employment. Fourteen pages are devoted to a description of projects, forms of financial aid, technical assistance, and managerial services that could be provided to small scale informal industry by governments. (14 pages, 64-78)

Policy Finding No. 1: Governments should support informal industrial and artisan enterprises that are capable of absorbing surplus rural labor. (2 pages, 20-21) Users: USAID; HG.

Policy Finding No. 2: Governments should establish interest rates, tariffs, foreign exchange, and tax incentives to equalize the disparity between costs to large formal and those to small or informal employers. (6 pages, 54-60) Users: USAID; HG.

Policy Finding No. 3: Governments should supply rural infrastructure services through the use of small scale units and maintain appropriate prices in order to accommodate the needs of the informal rural sector. (1 page, 60) Users: USAID; HG.

Policy Finding No. 4: Industrial policies, including licensing, standardization, and employment conditions should incorporate special provisions favoring the informal sector where the intention is to encourage off-farm employment through the informal sector. (1 page, 61) User: HG.

2. E.H. Gilbert, D.W. Norman, and F.E. Winch, Farming Systems Research, A Critical Appraisal, (1980). (122 pages)

The first twenty pages is essentially a description of farming systems research and a taxonomy of elements contributing to its subsystems and operating activities. The next 24 pages describe national, regional, and international research institutions and their functions. The following 38 pages describe research methodologies used at different stages of this research, the linkages among the institutions engaged in research, and training programs. There is a 3-page conclusion (82-84) but it contains no policy recommendations or obvious implications for action.

#### d. The OSU Reports

1. Dale W. Adams and Douglas H. Graham, Critique of Traditional Agricultural Credit Projects and Policies, Occasional Paper, #621; reprinted in Journal of Development Economics Vol. 8, (1981). (33 pages)

Most of the manuscript is a review of the dynamics of subsidized loans or credit programs aimed at small farmers, showing the distortions that arise in rural finance markets as a result of concessional loans. Consequences such as capture by larger prosperous landlords, inadequate repayment rates, and increased borrowing costs to small operators, are shown to result from such policies. Indirect results include distorted innovations, inadequate savings rates because of the comparative advantage of relying upon credit, and an undesirable drying up of existing informal sources of credit for small operators.

Policy Finding No. 1: In setting interest rates in credit programs for small farmers, governments should meet actual transaction costs, thus using market



responses to measure the effectiveness of subsidies versus improved access. (3 pages, 13-16) Users: USAID; HG.

Policy Finding No. 2: Governments should maintain flexible interest rates for rural credit programs in order to accommodate inflationary changes and approach a stable real cost. (1 page, 18) Users: USAID; HG.

Nine pages are devoted to explaining why governments persist in traditional rural credit programs in spite of repeated failures. (9 pages, 21-29) Users: AID/W; USAID; HG.

2. Dale W. Adams, Policy Issues and Rural Finance and Development, (June 15, 1977). (45 pages)

The first ten pages are devoted to a discussion of the history of western practices in rural credit and a translation of the assumptions underlying them to low-income country situations.

The next twenty pages discuss the economics of credit, followed by a ten-page description of informal lenders and the efforts of government to develop new institutions to adjust what appear to be inequities resulting from traditional operations.

Policy Finding: Governments should let the market establish real interest rates even for small borrowers and encourage the mobilization of voluntary financial savings and informal credit activities. (2 pages, 40-42) User: HG.

e. The University of Wisconsin Reports

1. Kashav C. Sen, Project Cycle, Regional Planning and Area Development Project, a State-of-the-Art Paper #1 (May, 1981). (44 pages, plus appendices)

There are no policy recommendations or implications in this paper, which is essentially a description of the "phases" of a "project" defined out of the author's experience and by citations drawn from discussions of international development. Appendices reproduce the conventional definitions and descriptions of special approaches like basic needs,

social impact evaluation, appropriate technology, project appraisal, participation, and social soundness analysis.

2. Concepcion Del Castillo and Michael L. Hoffman, Regional Planning Doctrine: Assumptions and Implications for Use in Underdeveloped Areas, Occasional Paper #4 (July 1981). (38 pages)

This paper consists of an interpretation of recent trends in the planning profession as applied to regional development. There are no policy implications defined here, nor is experience with planning incorporated in the discussion of the profession as practiced by AID or developing countries. Like its predecessor, the paper is neither pure nor applied research. It is a set of reflections about the profession and responds to the general description of the "state-of-the-art" paper.

#### f. The Cornell Reports

1. Milton J. Esman and Norman T. Uphoff, Local Organizations: Intermediaries in Rural Development, Rural Development Committee (May 9, 1983). Chapters 1 and 2 (50 pages)

These two chapters, from a book scheduled for publication in 1984, discuss the historical importance of local organizations in the west, the belated recognition of their importance in the literature of international development, and the political context in which government policies toward local organizations might emerge. But no policy recommendations such as appear in these chapters.

2. Norman T. Uphoff, John M. Cohen, Arthur A. Goldsmith, Feasibility and Application of Rural Development Participation: A State-of-the-Art Paper (January, 1979). (285 pages)

Policy Finding No. 1: In organizing for rural development, governments should concentrate on productive activities that will provide economic benefit to the rural populations, allowing the targets and objectives to emerge from the collaboration of the farmers. (1 page, 26)  
Users: HG, Proj.

Policy Finding No. 2: Such efforts should be integrated with other rural development activities rather than treated as a separate program or sector. (1 page, 28) User: HG.

Descriptions of organizations that illustrate these principles occupy ten pages, 38-48.

Policy Finding No. 3: Governments making use of cooperative organizations for rural development should concentrate on marketing, credit, input purchase, and other auxiliary functions rather than production except in cases where the factors of production have been socialized. (3 pages, 52-55) Users: USAID; HG.

Summaries of analyses of cooperative experience showing preconditions for success appear in two more pages, 55-57.

Policy Finding No. 4: Governments concerned with problems of landlessness should undertake programs beyond productivity credit and access to education to achieve institutional reform, especially of land ownership. Where supplies of land are severely limited, "marginal" distributions of family garden plots should be undertaken. (About 5 pages, between 112-117) User: HG.

Policy Finding No. 5: Agricultural research intended to benefit the rural poor should concentrate on immediate and direct advantages and involve the individual groups to be affected. They should be integrated into the community's educational activities over time. (One page, 165, followed by three pages of illustrations drawn from project experience; approximately five pages follow, giving guidelines for general application.) User: HG.

Policy Finding No. 6: Guidelines for farmer participation in productive activities should include coordination of incentives between collective and private activities and adjust to crops and organizational variables. Users: HG, Proj.

3. John M. Cohen, Norman T. Uphoff, Rural Development Participation: Concepts and Measures for Project Design, Implementation, and Evaluation, Cornell University, Rural Development Committee, Monograph #2 (January, 1977). (315 pages)

This essay is not intended to convey policy directions as such, but rather to explore the concept of participation in light of the existing discussions and analyses, working toward the production of information that would be useful for project design, implementation, and evaluation.

g. The NASPAA-DPMC Reports .

1. Coralie Bryant, Louise G. White, Elizabeth Shields, and Therese Borden, Research in Development Management: Learning About the Effectiveness of Management Interventions, (May, 1983). (31 pages, plus notes)

The paper presents a taxonomy of indicators of outputs, impacts, and consequences that might be applied in evaluating different types of projects. There are no policy findings or detailed discussions of government interventions in a specific context. There are suggestions for managers concerned with styles of project appraisal.

2. Morris J. Solomon, Flemming Heegaard, and Kenneth Kornher, An Action-Training Strategy for Project Management, (1977). (20 pages)

This paper presents useful guidelines for training purposes that go beyond pre-fabricated courses and involve sustained management participation in order to maximize feedback into operations.

Policy Finding No. 1: Governments should evaluate training by examining changes in effectiveness of trainee-alumni in planning and executing projects. (1 page, 3-4) Users: USAID; HG.

Policy Finding No. 2: Governments should build sustained training capacity by maintaining in-country teams with mixed skills to develop specific courses. (1 page, 5-6) Users: USAID; HG.

Policy Finding No. 3: Training should include project level courses developed with the collaboration of management. (1 page, 10-11) User: HG.

3. Morris J. Solomon, Merlyn H. Kettering, Pierrette J. Countryman, and Marcus D. Ingle, Promising

Approaches to Project Management Improvement (1981).  
(7 pages, plus appendix describing training projects in Jamaica, Tanzania, and Indonesia).

This report of a workshop seeks to explain the "success" of training programs designed with the concurrence of operating management ("action training"). It offers guidelines for training rather than policy findings as such.

4. Guidance System Improvement: An Emerging Approach for Managing Agricultural and Rural Development (April, 1983). (12 pages)

This paper describes a strategy by which key actors come together to define major tasks of project management in order to improve the group's consensus and confidence for subsequent administrative cooperation.

5. Marvin B. Mandell and Barry Bozeman, with Steven Lovelace, Toward Guidelines for Conducting R&D on the Guidance System Improvement Approach (May, 1983). (85 pages)

Proposed steps for an appraisal procedure that could be applied to evaluating the "guidance system improvement approach" described in item #4. There are no specific recommendations for policy purposes nor is a research design incorporated in the discussion of possibilities and pitfalls.

6. Merlyn Kettering, Improved Financial and Program Management (1982). (12 pages)

A description of steps taken to improve financial accountability in the Sahel Development Program in response to serious shortcomings reported by program audits. The effort includes consultation and training and includes as well changes in the organizational procedures and behavior.

7. Marcus G. Ingle, Merlyn Kettering, Pierrette J. Countryman, Organizational and Conceptual Approach of Development Project Management Center (1981). (19 pages)

A description of DTMC's approach to technical assistance and training and management. There are no specific policy findings.

8. J. Robert Herr, Project Analysis: Toward an Integrated Methodology (no date). (63 pages, plus appendix)

Description of theoretical approaches to integrated project analysis incorporating social as well as economic variables.

9. David C. Korten and Norman T. Uphoff, Bureaucratic Reorientation for Participatory Rural Development, (November, 1981). (24 pages)

Policy Finding: Governments using bureaucratic instruments to address problems of the poor should reorient administrators by adopting the "learning process" approach by which working groups from the agencies involved collaborate with research institutions.

There are approximately four pages devoted to discussion of elements in this learning process.

10. David C. Korten, The Working Group as a Mechanism for Managing Bureaucratic Reorientation: Experience From the Philippines (May, 1982). (33 pages)

Description of a practical experience of a working group in the national irrigation administration of the Philippines which brought action agency staff members with consultants under international funding to study interactions between agency activities and the beneficiary population. There is no direct policy finding except for the indication that similar approaches should be undertaken by other agencies, with international support.

11. George Carner and David C. Korten, People-Centered Planning: The USAID Philippines Experience (March, 1982). (21 pages, plus appendix)

Describes a procedure using the Philippines to analyze "survival strategies" taken by groups of poor households in the Philippines, as a means of assisting the AID mission in developing innovative approaches to the poor. (Appendix, 5 pages, describes survival strategies of landless agricultural workers.)

Policy Finding No.: AID missions should undertake empirical studies of the behavior and needs of target populations for whom development programs are under consideration. (1 page, 18-19)

12. David F. Pyle, From Project to Program: Structural Constraints Associated with Expansion (June, 1982) (9 pages)

Summary of the administrative changes introduced by transition from small-scale pilot project to more substantial interventions, explaining failure of the project to enter successfully into the expanded operations it envisaged. No immediate policy findings.

These findings are summarized in Table 1.

#### 4. Findings and Recommendations

a. There is no necessary correlation—even a negative one—between good research and policy-relevant research. Some of the best scholarship contained information that would change or reinforce policies and behavior. Some of the most plausible recommendations and apparently operational advice arose out of intuition, experience, judgment, or even ideology rather than a careful appraisal of well-documented facts.

Implication for action: Sponsors desiring to develop a research base for policy should incorporate a decision making framework (including the identification of decision makers whom the information is supposed to help) in the research assignment.

b. There is no strong correlation between the nature of the research institution and the quality of the research or the policy relevance. Consulting firms like Development Alternatives, Inc. have made important contributions to knowledge through their documentation

TABLE I

Potential Utility of Research Reports from 8 Sources

Research Product *	Total Pages	Pages of Policy findings	Pages of Policy Relevance	Potential Users**	Comment
a. 1	150	3%	10%	III, IV	Preliminary state-of-the-art paper
2	39	8	85	II, III	Guidelines for project designers
3	32	4	25	I, II, III	Background material from U.S. poverty programs
4	51	23	10	III, IV	Generally accepted doctrine
5	25	40	40	III, IV	Adds to conventional wisdom
6	41	9	20	III	Well established but not generally followed doctrine
7	31	13	20	I, II, III	Not easily implemented
b. 1	96	5	35	II, III, IV	Examples especially useful
2	25	8	45	II, III	Good advice on training (rarely followed)
3	133	7	75	I, II, III, IV	Extremely controversial
4	174	1	5	II, III	Intended as handbook or manual of instructions on assorted management techniques
c. 1	85	12	50	II, III	Emerging doctrine
2	122	0	55	III	Background information
d. 1	33	12	27	I, II, III	What governments should do--and why they don't--about farm credit
2	45	5	20	III	Largely overlap with d.1.
e. 1	44	0	0	II, III	Taxonomic
2	38	0	0	III	Reflections about the planning profession

\* NOTE: Letters and numbers refer to reports designated on pp

\*\* NOTE: 'I' refers to AID/W; 'II' to USAID; 'III' to the Host Government; 'IV' to Project Managers.



TABLE 1

**Potential Utility of Research Reports from 8 Sources**  
(Continued)

Research Product	Total Pages	Pages of Policy Findings	Pages of Policy Relevance	Potential Users	Comment
f. 1	50	0%	10	II, III	Background
2	285	10	10	III, IV	Now the standard reference on participation
3	315	0	10	III	Precursor to g.2
g. 1	31	0	2	I,II,III,IV	Management indicators
2	20	15	10	II, III	Current training doctrine
3	7	0	50	III	Workshop report. Overlaps (g.2)
4	12	0	50	II, III	Management technique
5	85	0	0	III	Procedure for g.4
6	12	0	50	III	Management correctives
7	19	0	0	I	Prospectus of services and approaches
8	63	0	0	I, II, III	Variables in project analysis
9	24	0	15	II, III	Administrative behavioral reform
10	33	0	85	II, III	Application of h.1
11	21	5	85	II, III, IV	Consciousness-raising technique
12	9	0	20	II, III	Administrative analysis

of project experience; universities like Berkeley have produced useful and nonintuitive guidelines for action by investigating institutional behavior carefully.

Implication for action: Research for policy purposes should not be assigned on the basis of an anticipated preference for action on the part of consulting firms or for theory on the part of universities. Other considerations like capacity to retain and deploy high quality personnel and the clarity of the assigned task or the monitoring of performance are more likely to produce desirable policy outputs.

c. It is possible to compare the policy relevance of research conducted by different institutions in different fields by imposing an overlay of potential decision makers on the published output and structuring the findings, or even the raw data, in terms of their potential utility. Such an exercise should be conducted independently of a methodological evaluation, however, since some of the clearest policy recommendations may be unsound.

Implication for action: If a sponsor wishes to improve the policy relevance of research in its programs, an interim check using some form of "decision overlay" as both a test and a learning device can serve as a tool of management for that purpose. A further implication: sponsors should always apply such a "decision overlay" with a parallel "methodological overlay" so that the researchers are aware that standards of both utility and soundness are to be applied to their work.

d. The crude measure of utility provided by the "decision overlay" does not capture the full value of research to an operating agency. While some of the research results seemed to have no utility or policy relevance even as background information—the University of

Wisconsin's regional reports, for example--they may be merely a warmup exercise intended to position the researchers to perform more useful functions.

Implication for action: The sponsor should be patient with institutions embarking on research assignments whose relevance to operations is unclear, or in fields that have not yet attained a level of consensus necessary for accumulative data gathering and interpretation. But the sponsor's responsibility goes beyond patience (see preceding paragraph).

e. The combining of research with consultancy assignments is probably beneficial to both activities. Apart from the obvious advantages of "access" that is so often difficult to achieve in the social sciences, there are important cognitive assets to be gained when researchers participate in an operational situation. This exercise suggests more about the value to researchers than it does about the potential advantages gained by operators when the longer-term and conceptual aspects of their work are being reviewed.

Implication for action: AID should not try to separate the research from the consulting functions contractors can provide; it probably should not always insist upon merging them in the same institution, if the resources in personnel, library facilities, and management are not available, however. AID should prefer achieving a balance between the two functions within the same contracting organization if it is receptive to that combination of efforts, but should not expect good performance in both of them if the personnel involved are reluctant or apathetic about either the research or the consulting services.

f. Communicating the results of research to the users most likely to benefit from them is a task AID can undertake better than

the researchers themselves. The quality of the writing of these reports varied from publishable professional style to jargon-ridden statements, with a certain amount of popular bureaucratese in between. The reports themselves are not standardized or presented with the specific decision maker in mind, and the policy relevant findings sometimes had to be teased out of the descriptions, analyses, and taxonomies and were not immediately obvious in some cases.

Implication for action: AID should establish a separate unit (perhaps in the Bureau of Science and Technology) to disseminate research findings and current doctrine about subjects where the best judgment is approaching consensus as to policy outcomes. Such reports need not be presented normatively nor as official statements of the U.S. government, but should be considered a service to decision makers at all four levels described in the overlay developed here. An alternative mechanism for this purpose would be an independent clearing house founded by the Agency for purposes of disseminating the policy implications of sponsored research.

SPECIAL REPORT CPROMOTING RESEARCH CONTRIBUTIONS  
TO KNOWLEDGE ABOUT HOUSEHOLDS AND WOMEN

Pauline E. Peters

1. Introduction

A wide range of papers, articles, and monographs published under nine cooperative agreements were reviewed to determine the degree to which research and applied consulting contributed to professional understanding of the role of households and women in the rural development process. This report analyzes that contribution and, further, seeks to demonstrate the way in which more systematic attention to rural households and gender can contribute to a fuller understanding of rural development itself. To this end, the report also suggests guidelines for promoting more applied research on the roles of households and women in rural development.

Analyses of rural development have increasingly been taking households as units of analysis because of the central role played by households in shaping and mediating the decisions of rural inhabitants in response to the further integration of rural areas into wider economic and social structures, and an accelerating process of livelihood and occupational diversification. Critical to understanding this central role is the development of more refined conceptual and methodological means of investigating the relations

within and between rural households. Key to this task is a more sophisticated understanding of gender as a principle of differentiation and of the ways in which it interacts with other critical variables. Rural populations are not homogeneous and it is necessary for both research and policy purposes to develop the most precise means of distinguishing the significant subgroups or subcategories in any population being investigated or assisted. Perhaps more than any other cross-cutting topic, conscious attention to households and gender issues can enhance the relevance of research emanating from most cooperative agreements.

At present, the topics of households and women generally receive separate treatment by donor organizations, consulting agencies, and individual researchers. Thus, at AID and elsewhere, women are ensconced in a specially defined field of "women in development" whereas households appear as units of analysis and social units addressed within substantive fields such as rural credit, agricultural development, off-farm employment, nutrition, and so forth. One of the unfortunate consequences of this analytical and institutional separation has been a failure to integrate information on these two topics sufficiently well either in research or policy recommendations. This paper suggests points at which such an analytical and methodological linkage would be fruitful. In addition, it points out ways to rethink "households" and "women" as both analytical categories and as targets for research and policy. Central to the perspective proposed here is that "rural households" and "women," whether as categories of analysis

or as prospective participants in rural development projects, must be considered in relation to each other.

The cooperative agreement research output indicates that research could be improved through: (1) greater precision in the selection and use of units of analysis and analytical categories; (2) greater care in the formation of generalizations; (3) more attention to the distinction between advocacy and research; (4) the critical need to go beyond current stereotypes, in particular to expand the present focus on women as a disadvantaged category and its corollary rationale of considering a focus on women to be merely an equity issue; (5) a more systematic analysis of household processes that develops more rigorous theories and methods for investigating both intra-household and inter- or supra-household organization; and (6) more precise distinction between gender as a key variable in differentiating a research population or program recipient group and women as social actors within specific social units or groups. The aim in this proposed redirection is to avoid the present ad hoc manner of considering gender and households and to achieve a more rigorous integration of these as analytical categories in our understanding of the critical social and economic processes in rural development.

## 2. Guiding Assumptions and Major Findings

There are two basic organizing assumptions about women in the publications reviewed. First, consideration of women in rural development is assumed to be an issue of equity, in that women are seen as constituting a disadvantaged category of rural poor (Eicher

and Baker, 1982; Uphoff, 1979; Esman and Uphoff, 1982:3,29; Cohen and Uphoff, 1977:222; ICRW publications). A common theme is the pervasive structured patterns of differential access of women and men to extension advice and services (Geller, et al., 1980; Staudt, 1979; Whyte, 1983; Eicher and Baker, 1982; Honadle, et al., 1980), credit (Charlick, et al., 1983; ICRW), resource control (Charlick, et al., 1983; Staudt, 1979; ICRW), and land rights (Cohen and Uphoff, 1977). Uphoff, et al (1979) go on to state that to prevent or preempt such bias against women is an issue not only of equity but also of releasing or not repressing the talents and energies of a large proportion of a country's population.

The other and related assumption is that extra-domestic production aspects of women's activities should take first priority in research and policy analysis. The emphasis on women's productive activities and the assumption that women are a disadvantaged category of producers are reflected in the major conclusions of the studies:

- a. labor statistics underestimate women's employment especially in the informal sector and women face particular structural barriers and hindrances to their fuller employment;
- b. definitions of gainful employment in national labor statistics underestimate and undervalue women's work in both domestic and extra-domestic activities;
- c. women's access to new forms of technology and to credit sources is more restricted than that of men's, a circumstance based not necessarily in purposive discrimination as in preexisting structures of resource control, authority, and information which either generate or exacerbate women's relative disadvantage;



- d. as a result of these, women heads-of-households, whether "the women left behind" by male labor migrants, urban migrants themselves, or "independent" farmers and traders with dependents, fall disproportionately into the poorest categories in a range of countries;
- e. legislation designed to protect women's employment has frequently been based on misguided stereotypes of women's roles and hence has served paradoxically to hinder rather than help women;
- f. capital-intensive developments in both agricultural production and processing off-farm activities have often displaced women's labor;
- g. there are benefits and costs to women of their participation in animation rurale or as paraprofessionals;
- h. the range, variety, and strengths of women's organizations may be usefully seen as "analogies" rather than blueprints for project development;
- i. current understanding of labor migration underrepresents the degree to which women migrate independently of spouse or family.

Within these organizing assumptions the extent of coverage varies from detailed syntheses of evidence and recommendations on female-headed households, women's formal employment, and their role in migration to a few paragraphs on certain aspects of women's disadvantage relative to men's.

The findings point to numerous research questions and issues for policy action and analysis, quite apart from the specific recommendations that have been made by authors. Even where there are certain reservations to be made about particular formulations or frameworks of interpretation, the sheer raising of the issues and their discussion help to fill the notable gap in prior rural development literature on issues of women and gender.

Insofar as rural households are addressed at all in the cooperative agreements, it is in an indirect manner rather than as part of a systematic discussion. Authors take the household as a unit of analysis and/or as a presumed unit of production and consumption without either (1) defining what comprises the unit, or (2) examining whether the household is the appropriate unit of analysis for the particular questions being addressed. A few authors are even less precise and use the term household only as a convenient but unjustified alternative for farmer or individual or farm. For example, one report on rural credit includes "Farm Household" in its title, yet the survey results appear to refer in practice to replies by individual respondents. Because the use of the term household is unexamined, the reader does not know precisely which is the referent, nor whether the patterns of credit use would appear different if households as opposed to individuals were sampled.

### 3. Issues of Conceptualization

#### a. Units of Analysis, Analytical Categories, and Conceptual Clarity

The need to be precise both in the choice of units of analysis and in the justification for that choice as well as in the analytical categories that are used cannot be overemphasized. Much research time and resources are lost through lack of precision, since conclusions remain unconvincing or questionable. If conclusions are low in explanatory and predictive value then they also generate poor guidelines for policy.

Household Structure and Socioeconomic Change. Although the household is frequently taken to be a key unit of analysis, recent research has demonstrated the need to take account of both intra-household relations and inter-household links as part of the research strategy. Unless we understand household dynamics and the wider networks in which households are embedded, we cannot fully comprehend the effects on production and welfare of major processes of rural development.

Labor migration, both within rural areas and between these and urban centers, is of great significance for many regions. A number of the papers reviewed addressed the issue of the effects of migration on household viability as productive and consumption units. Problems of conceptualization arise with reference both to the appropriate units of analysis and of the key terms used as explanatory concepts.

Thus, conclusions that labor migration by adult members of households inevitably leads to breakdown of extended families or to "the weakening of traditional family structure" (ICRW, 1979) can be questioned on both grounds. First, certain cases suggest that far from labor migration eroding or destroying "extended" family groups, these may be supported or even generated by migratory processes since labor allocation and patterns of investment are distributed across households (Lewis, 1981; Peters, 1983a). The relative autonomy of "nuclear" or conjugal units within these larger groupings have then to be carefully specified. More precise understanding of the variable

effects of labor migration on different categories of rural households or on different categories of persons (by age, sex, class, etc.) is achieved by more precise specification of the units of analysis.

Similarly, conclusions on the weakening or breakdown of family structures that are derived from data on changes in patterns of authority between spouses or between generations are questionable. This is because the definition of the key concepts is far too loose; they remain evocative rather than specified. When is a change in domestic organization or a shift in the behavioral patterns of spouses a "change" and when a "weakening?" How "traditional" are family types that historical evidence often shows to be products of colonial or post-colonial policies on location or settlement? "Family" is particularly slippery as a term and is often taken to refer, without specification, to a nuclear unit of husband, wife, and children, units which also include other relatives and non-relatives, extended kin groups and networks, resource-owning groups, and so on.

Women's Marginality, Women's Autonomy. A similar loss in analytical power from the use of conceptually fuzzy categories is seen in the use of "marginality" and "autonomy" to describe the efforts of socio-economic change on women and on households. These concepts tend to remain very general and imprecise in use, with more emotive than analytic force and hence preempt conclusion rather than facilitate analysis to reach a conclusion based on demonstrated evidence.

For example, an interesting and important discussion of some of the negative effects on women of development processes remains

opaque because the major explanatory concept used, "marginalization," appears to combine three separable sets of circumstances (Staudt, 1979). These are the structural marginality of women in formal labor markets insofar as numbers employed, wage levels, conditions of entry, promotion prospects, and so forth are concerned; the devaluing of domestic work as householdss are incorporated into cash economies based increasingly on wage labor; and the contradictory processes of women being forced on the one hand into the labor market by declining levels of agricultural production and on the other being withdrawn from extra-domestic work as income and/or rank increase. Had the discussion not only distinguished these processes rather than collapsing them into the one concept of "marginalization," but also specified more carefully regional and class differences, the points could have been made more precisely and forcefully.

It is clearly important to learn whether and in what manner particular processes of change undermine existing strategies of livelihood or relations of authority, or to what degree the constraints on the production, consumption, and investment activities of different categories of rural people are tightened or not. Moreover, much empirical evidence has demonstrated that certain categories of women are particularly likely to suffer the more negative characteristics of change. Nevertheless, the current uses of marginality and autonomy tend to result in circular reasoning or sterile debate and require much greater precision and refinement. A topology of rural households based on an assessment of the relative autonomy of wives

vis a vis husbands (Staudt, 1979), for example, fails to provide categories of households that are discrete and hence appropriate units for analysis or subjects of policy or program. Similarly, the important point that women who migrate do so as wives depends upon whether the women are identified as autonomous or dependent (ICRW, 1979). Greater conceptual precision and thereby greater force to the conclusions could have been achieved by distinguishing more carefully between a migrant's motives and his/her structural position within a domestic or family group, and among different dimensions of dependence on or independence from different categories of persons.

b. Overgeneralized conclusions

A problem that is particularly apparent in the more synthetic papers, which attempt the important task of drawing together large bodies of research reports or government statistics, is to generalize too quickly from a limited set of data. The limitation involved may be that of regional coverage, time span, validity of data deriving from the manner of collection, the types of inference drawn, and so on. For example, the broad statement that: "migration leads to the dissolution of patrilocal, patrilineal families and to the emergence of mixed or nuclear families," is supported by evidence from only one country (ICRW, 1979:123). Similarly, general statements that a particular form of social organization (such as a family) has broken down are often made with reference to a synchronic data base even though the statement is essentially claiming a temporal base. That is, perceptions of 'strain' or 'conflict' within households or

families are taken to be products of the particular changes posited as the breakdown of former structures. Yet without evidence of the presence and quality of strain and conflict before said changes took place, one is hard pressed to prove that the latter produce the former.

c. Advocacy and Research

A final problem of conceptualization is that at times, authors insufficiently distinguish between statements based on advocacy and those purporting to be statements of research findings. This tends to be associated with the use of underspecified concepts and overgeneral conclusions. For example, evidence of certain negative consequences of development processes for particular categories of women or of households is taken to justify statements about general disadvantage. There is substantial evidence that describes the strain on the labor time of women whose husbands and close male relatives migrate leaving them with a "double burden" of work, and the tension between a woman's having to exert managerial decisions in her husband's absence and maintaining the authority relation between spouses. In seeking to place such evidence before those responsible for policy formation and implementation, the temptation to overdraw the negative aspects and ignore or underplay the beneficial or neutral is often strong. Yet clearly, not all women nor all "female-headed households" are severely disadvantaged. Where the discussion more carefully links general statements with evidence from particular areas/cases and provides a more nuanced and compre-

hensive treatment, the conclusions and policy recommendations gain in conviction. An advocacy stance per se does not entail faulty conceptualization or questionable inferences from unspecified data. Careful formulation and generalization can only aid advocacy in that the issues are first more rigorously delineated and explained, and secondly can lead to more appropriate policy project formation.

#### 4. Gaps in Knowledge and New Directions

##### a. The Household as Unit of Analysis

It was pointed out in Section I that a number of authors in the cooperative agreements have taken the household to be the basic production and consumption unit in rural areas, a convention that has been long established in related fields of peasant studies, social economic history, and rural sociology. The focus on the household by rural development analysts is not arbitrary but is a considered response to an earlier focus in agricultural research and economic analysis of rural development on the individual farmer. Investigation and knowledge of the constraints within which rural people gain their livelihood have been greatly advanced by studies of decisions and allocations (see, for example, the review by Eicher and Baker, 1982). The particularly critical leap in recent analysis and methodology has been from considering one activity or crop per household production/consumption unit to attempting to understand the multiple activities and strategies of household units. Farming systems research approaches are the clearest example, although advances in understanding and, hopefully, policy implications, are being achieved with



regard to rural financial markets (Bouman, 1981; Ohio State, 1981) and migratory patterns (Roe and Fortmann, 1982).

The next critical leap is to recognize that the household is not a black box nor a totally bounded unit; that the relations within the household and between households require analysis in order to understand the activities of the household per se. Just as assuming that an individual farmer is the primary unit of analysis has proven in numerous ways to distort or mask the processes to be explained, so may an unqualified assumption of the farming household or family as the sole unit of analysis. The dimensions of this qualification are intra-household and inter- or supra-household relations.

Intra-Household Analysis. Insofar as intra-household relations are concerned, a major problem in studies taking the household as the primary unit of analysis has been to assume that it is a joint decision unit, and that, hence, internal transfers are either irrelevant or assumed to be automatic and frictionless. One study of a rural area in Jamaica, for example, claims to be based on a "household" survey. Examining the methodology section, however, the reader learns that the data were elicited from "farmers" interviewed in an "accidental" sample and that the respondents, who were in most cases heads of households or heads' spouses, knew "the affairs of the entire household" (Goldsmith and Blustain, 1980). Such a statement requires substantiating evidence. Much evidence already available in the literature suggests that information from one member of the household is rarely equivalent to valid information on "the entire household."

Recent research indicates that members of households frequently have different preferences or strategies. The particular patterns of activities attributed to households are in fact the outcomes of prior adjustments among these different sets of preferences. There are implications of these findings for both theory and method. As far as data collection and analysis are concerned, information must be collected for individuals as well as for social aggregates such as households (cf. Rosenberg and Rosenberg, 1979).

A major criterion for investigating the critical differences among household members' sources of income, expenditure patterns, labor allocation profiles, and so forth is gender. Thus, one report argues that to ignore the strategies of individual women and men within households is to misconstrue the reasons for and effects of migratory moves (ICRW, 1979).

In most of the papers reviewed, however, there is no explicit awareness of the importance of intra-household relations to many issues of rural development. Even when authors caution against assuming the household to be a homogeneous, undifferentiated unit or the information from the household head or any single member to be equivalent to information from all members (Uphoff, et al., 1979:94, 122), such comments remain ad hoc and unintegrated into the analytical framework. The authors of an important paper on developing farming systems research, for example, take the farm household as the key unit of analysis. They suggest possible complications when they refer twice to the possible presence of "multiple decision makers" within

households and once to the significant links between different subgroups of households. Yet in both instances these important qualifications to a central assumption are relegated to footnotes and are not discussed in the text (Gilbert, et al., 1980:6n1; 13n1; 47n2).

Over the past decade, a large amount of empirical evidence on intra-household differences in patterns of labor allocation, sources of income and preferred expenditures and investments has been produced. Some of this is reflected in the cooperative agreements (especially ICRW; Cornell; Eicher and Baker, 1982). There have also been substantial advances in the theoretical and conceptual frameworks for understanding domestic organization and its links with wider social, economic, and political structures. The development of bargaining models by economists in response to the work of the "new household economics" and other social scientific models of bargaining, decision making, negotiation, and social exchange are examples of attempts to devise concepts and methods to deal systematically and rigorously with both intra-household relations and transfers between domestic units.

It is to be hoped that the "intimations" of these issues in the cooperative agreements reviewed will be followed up with more systematic and focused work that will build on the theoretical advances being made.

Supra-household Analysis. The same care required in approaching the internal organization of households has to be exercised in relation to their boundaries, that is, to inter- or

supra-household linkages. Roe and Fortmann (1982), for example, provide a very useful discussion of the problems that may face researchers when they assume that production and consumption, or all major decisions and transfers, take place within one discrete domestic unit or household. Using evidence from Botswana where these specific issues have been most effectively engaged, they explain how, in a system of agricultural production that is based on seasonal mobility and on a combination of crop production, husbandry, and off-farm employment, "the" household may be divided into two, three, or more units at certain times or for certain functions. While the particular seasonal and other movements in Botswana have in a sense forced this comprehension of the permeability of household boundaries, it is to be hoped that this well-documented example\* will help analysts and researchers in other areas of the third world to look extremely carefully at the units of analysis that, only too often, either are taken for granted, or seem "obvious" from a particular perspective.

One paper (Eicher and Baker, 1982) notes some parallel work being done by Franco-phone researchers on the assumption that there is one "exploitation agricole" (approximately equivalent to rural household). These researchers have been developing a more sophisticated methodology that seeks to distinguish for any particular

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\*There is a large body of research data available on these issues for Botswana (see, for example, Alverson, 1979; Cooper, 1979; Kerven, 1979; Mahoney, 1977; Peters, 1983a, 1983b), as well as for other parts of southern Africa (see especially Murray, 1981; Spiegel, 1980).

rural area the most significant units of production, of consumption, and of investment, units whose boundaries do not necessarily coincide.

In sum, then, just as taking the household as the site of the "joint" preferences or utilities of household members can be very misleading, so can the failure to set any particular domestic unit within larger networks of interaction. The MSU literature on farming systems research stresses the importance of placing the farming household within its social, economic, and political context in order to grasp the dynamics of any "farming system." Authors also recognize the problem inherent in any systems model of setting boundaries to the analysis. By their nature, systems models tend to become octopuses. Nevertheless, there are certain interconnections or linkages that are critical to understanding farming systems. One set is those between domestic units or farming households. This may be quickly demonstrated. A key task of FSR is to establish "recommendation domains," that is, those categories of farming households that share sufficient characteristics to constitute a relatively discrete subgroup for extension advice, technological innovation, etc. Thus the perception of significant links between subgroups should not be relegated to a footnote but incorporated into the analysis.

If the promise of farming systems research (and its untitled correlate, rural systems research) is to be realized, these appended insights must be rendered an integral part of the conceptual and methodological apparatus of enquiry. For example, in a plough agricultural system, such as those in parts of Africa, households

without oxen and those with, say, more than ten oxen constitute two separable "recommendation domains." Yet the links between them, in terms of exchanges of labor against plough services, or labor against a share of the harvest, will be critical to understanding how the system operates, and thus, the significant parameters of individual farmers' activities. Since determinants of labor and other resource allocation also derive from the composition and organization of domestic units, the methods for investigating such a dynamic must include inquiry into inter-household transfers as well as intra-household allocative decisions.

b. Going Beyond the Conventional Stereotypes

The policy relevance of household and gender issues would be enhanced by redirecting research in the following ways:

- 1) To explore the significance of gender for certain rural development processes without equating "women" with disadvantage and poverty;
- 2) To expand the focus in any enquiry into the effects of rural development on different categories of women or households beyond production per se;
- 3) To distinguish more precisely between gender as a key variable in differentiating a research population or program recipient group and women as social actors within specific social units or groups.

1) Beyond Marginalization. First, attention to gender issues should be expanded beyond women as disadvantaged and poor.

While much of the research and policy action in rural development have been concerned with the rural poor, male and female, it is striking

how the category "women" in the literature on development is taken ipso facto to mean poor and disadvantaged. It has almost become a litany or, less eloquently, a knee jerk reaction. This is not to deny that much useful work has been produced from this perspective; the ICRW corpus and some of the Cornell publications are examples. Nevertheless, the present emphasis on "disadvantaged women" is too narrowly defined and the obsession with "female-headed households" and typologies of households has resulted in diminishing returns to that model of inquiry (cf. Peters, 1938b). Moreover, a research or policy focus on women only as an equity issue encourages a tendency to "add on" consideration of distinctions based on gender rather than to incorporate them into the analytical framework for understanding socio-economic change. In turn, this lack of incorporation or of a systematic analysis of gender entails a loss in explanatory power and policy relevance.

To suggest that research now needs to consider other categories of women than the "needy" is not to say that poor women, who face more numerous and less tractable constraints than poor men, should cease to be a focus of research and policy. It is to suggest, rather, that to retain such an undifferentiated category of women is to obscure many of the questions that rural development specialists are asking. For example, not all wives of migrant laborers in all countries are "left behind" with only increasing burdens. Some use the remittances to improve their crop production, invest in cattle or tree crops, or start a small business. These categories of women need

to be investigated not only for analyzing patterns of income transfer, investment, or credit exchanges. In addition, information on the better-off categories of women and households is needed to answer questions about the poorer categories. This is because it is the relationships among social categories and social groups that are the keys to understanding the dynamics of rural production and social differentiation. As stated above, patterns of labor allocation or of income flows are determined both by household organization and by relations between households. The processes of the generation, maintenance, and transformation of certain categories of needy women or of certain types of asset-poor households will be discerned not by a myopic focus on one category of women or one type of household but by an understanding of the wider system within which these are encapsulated.

Such a shift in focus would help avoid some of the overgeneralized statements discussed earlier, such as the proposition that new technologies in combination with "cultural constraints" have undermined women's economic autonomy and have left them increasingly "powerless" (Honadle, et al.:122). There are certainly examples of women's labor being displaced by introduced technology and more seriously, their rights to land being jeopardized or lost. And our understanding of these consequences is far from adequate. Nevertheless, the current reality is neither as unremittingly grim as "increasing powerlessness" suggests, nor does it appear to be following in as unilinear a trajectory. An excellent paper on women's



associations (March and Taqqu, 1982) indicates a much greater variety of experience as well as a far more nuanced analysis that is, in fact, more typical of research on women and gender outside the development literature. Practitioners and researchers in development have models from which to learn.

Similarly, to infer from the negative effects of various changes on certain categories of women that women are going to miss out en masse in the process of institutionalization (Gellar, et al., 1980; Staudt, 1979) is overly simple and based on an expectation of neat, black/white outcomes rather than the more usual shades of grey. For example, data by Fortmann (1980) and other data provided by Gellar, et al. demonstrate that one should probably expect both gains and losses, instances of conflict and cooperation from any change, directed or otherwise. Both dimensions need analysis. Not enough attention has been given to the uneven, often contradictory effects of various events (the introduction of a new processing machine or a credit system) and processes (migration, commercialization of livestock) on the lives of women and men and on the composition and organization of domestic and other social units.

ii) Domestic and Extra-Domestic Production. Some of the problems noted in current interpretations of "women in development" result from limiting the relevance of differences of sex to the sexual division of labor and of defining the latter very narrowly. For example, analytic attention to gender is often justified by

stating that women play important roles in the production and marketing of food. Apart from the fact that this applies only to some areas of the third world, attention to the sexual division of labor is required not only because of women's specific responsibilities such as being food producers. It is also required in order to comprehend the dynamics of household allocation of labor and other resources and hence the cyclical/seasonal changes in the demand and supply of labor, and in patterns of income, consumption, and investment.

In general, there is an overemphasis on the production aspect of women's activities to the detriment of other dimensions of women's experience in the development process. This emphasis represents a needed response to the overconcentration on women as reproducers of children and as home maintenance workers. Nevertheless, it is important that one not allow a further imbalance to be created. Thus, the jural or legal aspects of the particular circumstances of categories of women require closer attention, often per se, often in association with questions about production. Claims on property and various resources, rights and obligations of conjugal arrangements, the disposition of income, inheritance, and succession rules and practices are examples. That closer attention is warranted to women's associations is seen in the paper by March and Taqqu (1982) and in the questions raised by writers on rural credit systems (Ohio State) and farming systems research (Michigan State).

More work is also needed on the relation of ideological and cultural models to various aspects of the development process as this relates to women and gender. For example, a number of the papers reviewed refer to "ideology" with respect to authority relations within the household, to gender relations in different societies, to preconceptions of project designers, and so on (e.g., Uphoff, et al., 1979; Staudt, 1979; ICRW; Whyte, 1982) but with very little systematic discussion of this dimension of change and development. With respect to these issues, the development-oriented research could benefit enormously from two bodies of literature: the cross-disciplinary feminist debate on the constitution and ideology of the household and the anthropological literature on domestic social organization, kinship and gender roles.\* These not only address the issues of the household but are also engaged in a sophisticated discussion of ideology and systems of meaning. Two particular areas in the analysis of sex roles and women in reference to the development process, which demand both careful empirical investigation and nuanced analysis of ideological and cultural categories, are the system of national accounts and the definition and measurements of gainful employment (Eicher and Baker, 1982; ICRW, 1980; cf. Beneria, 1981) and a related topic, the analysis of the "domestic" work and roles of women and the relation between social reproduction and production.

\*Both literatures are now so large that it is rather invidious to mention only a few; nevertheless, examples are Stolcke (1981); Morris (1981); Ortner and Whitehead (1981); and Guyer (1981).

iii) Gender as a Critical Variable. A more productive research focus on women and households with the aim of providing policy-relevant findings depends on a clearer and more precise distinction between gender as a principle of differentiation and women holding specific roles in particular social units. Research and project reports in the cooperative agreements and elsewhere demonstrate that the organization of key social units, such as households, labor groups, credit associations, residential groups, and so on, is an important component of analysis. They also demonstrate that as a category, female farmers, female heads of households, women traders, and so forth, often face specifiable sets of conditions that differ significantly from those of male farmers/heads/traders. It is necessary, then, that the analytical frameworks to investigate and guide rural development incorporate gender: (1) as one of the critical variables differentiating rural populations, and (2) as one axis in investigating household organization.

These points may be briefly illustrated with examples from the cooperative agreements reviewed:

a) Agricultural development: Stavis (1979) notes that technology requiring high labor units per land area benefits "smaller farmers." But clearly, this is only if the farmer can mobilize sufficient labor, a condition that does not apply to all small farmers. In particular, empirical evidence from various parts of the world suggests that certain categories of small women farmers face significantly different labor profiles than male farmers. Elsewhere

in his report Stavis cites Staudt's research finding that women farmers frequently receive less extension aid than males do. Yet because he cites this ad hoc and does not systematically incorporate gender as a critical variable, other conclusions, like that cited here, remain open to question.

As a second example, papers on farming systems research illustrate the need to distinguish between "gender" and "women" as well as the ways in which they are related in research. The determination of whether single trait or packages of practices ought to be employed in agricultural innovation (Gilbert, et al., 1980:53) needs to take account of gender since often tasks in a crop cycle or in livestock are sex-linked, and thus the allocation of labor may be significantly different for each sex (as well as for different age groups or different levels of asset ownership).

It is particularly important to stress that gender as a variable that differentiates a research or program population must always be seen in relation to the particular social units (households, family, kin group, residential group, etc.) that mediate women's access to resources or authority. Thus, in farming systems, researchers might discern that women farmers as a category face particular sets of opportunities and constraints that differ from those of men. These women farmers or subgroups among them might thus constitute a particular "recommendation domain." But the significance of gender as a principle of differentiation must be assessed by investigating the particular roles played by women within specified

social units such as the household. The results would then be recommendation domains comprising women farmers within households with a specific set of characteristics (based on number of cattle, crop mix, income level, etc.), or households at different levels of assets and with different ratios of adult male to female labor.

b) Rural Credit: No attention is given in any of the papers on rural financial markets to the particular significance of RFM to and for rural women. Yet Bouman's paper, which presents empirical studies of credit associations, documents the central role that women play in informal credit systems. While all the authors writing on RFM recognize and discuss the ways in which smaller or poorer farmers have faced greater costs than bigger or richer farmers with respect to formal credit institutions, no similar attention is paid to the likelihood of a similar pattern for women. The ICRW paper on credit provides evidence to suggest this is so. The ICRW team also recommends that special institutions and procedures are needed to improve women's access to sources of credit and savings, but that this must be done within the context of promoting viable financial markets. Thus, while the arguments of the "new direction" in thinking about rural credit are plausible, the particular ways in which one would seek (1) further understanding of RFM, and (2) a shift in emphasis from specific interventions in the form of credit programs are not spelled out. It would seem, however, to require: (1) greater attention to inter-household credit-debt relations and the ways in which households and inter-household networks are tied into supra-

local systems of exchange; (2) investigation, as yet undone, of the intra-household differences in credit-debt relations, which will provide information not only on household dynamics in this regard but also on significant differences between men and women as savers and borrowers; (3) consideration that some categories of producers such as women traders or farmers might still require targeted programs.

### 5. Conclusion

There are two major modes of analysis in rural development where women and households are significant categories. One is where there is policy interest in how certain categories of women or of household are faring in particular projects or policy interventions or in general processes of economic development. Second is where the interest in analyzing specific processes or patterns of relations (as in credit exchanges, labor allocation, income transfers, expenditure patterns) necessitates (a) taking gender as a key variable in differentiating a population (along with age, marital status, asset level, etc.) and/or (b) investigating relations both within and between household units.

In neither of these modes (each of which has an enormous range of possible applications) can "women" or "households" be taken as undifferentiated categories or units. Equally, this formulation underlines the fact that these two "topics" (women, households) are better seen as dimensions and that their proper investigation requires an integrated framework of analysis. Questions about the flows of labor or of income, or about information or credit networks, can

rarely be answered by aggregate figures alone but require inquiry into the links between key social units including households and key social categories including women.

Equally, it is particularly important not to confuse these two dimensions of an inquiry into the production, consumption, and investment processes of rural development. Many of the shortcomings in research and policy statements about women and households—the unsubstantiated overgeneralized conclusions, imprecise explanatory concepts, or undetermined units of analysis—derive from an insufficiently precise distinction made between analytical categories and social groups or categories, and a persistent slippage from one to the other. Particular findings about specific groups or categories of populations are taken to demonstrate something about the general parameters that distinguish populations. Many of the general statements about "women" (concerning disadvantage, marginalization, autonomy, and so forth) are not tenable without qualification precisely because data on particular categories of women and households are taken to say something about "women" in general. Often, discussion moves back and forth between references to particular categories of women and those to "women" so that it is not clear which is the referent and what is the substantiating evidence required.

Papers in the cooperative agreements reviewed have provided more "hints" than systematic analysis in this aim to incorporate gender and household organization into analysis. Nevertheless, the points discussed in this report suggest that there are bridges being



built to some of the literature on these issues that exists outside specifically rural development analysis. In fact, this report suggests that there is a new understanding of rural development that implies greater concern with more careful specification of and investigation into household and inter-household processes. This understanding may be stated in a brief word: it is systemic. The frequent recourse to speaking of systems--farming systems research, rural credit/financial systems, labor migration systems--is not arbitrary. The shifts from basing analysis on the individual farmer to one on rural households and from single to multiple activities and strategies that have already been noted, are part of a more general approach that seeks to understand the activities of farmers within a total context that includes the domestic organization of farming, local organization, and national and international policy frames. Eicher and Baker's excellent summary (1982) of significant work in rural development is a good example of the attempt to break away from sterile and doctrinaire theoretical confrontations and move towards a fruitful synthesis of political economic analysis and micro-economic analysis of individual behavior.

**RECOMMENDED GUIDELINES**

- 1) Greater conceptual clarity is required to improve research methodology and to enable more precise conclusions and actionable findings to be drawn.
- 2) Particular care has to be exercised in generalizing from one case or data from one area, from a single or an undifferentiated category of population, and from inappropriate data bases.
- 3) A more rigorous distinction is needed between reporting research findings on gender-related issues and making a statement of advocacy. Care should be taken that the pressures of advocacy not lead to highly selective presentation of evidence such that only the negative effects of change are reported, neglecting the positive, neutral, or contradictory effects.
- 4) Imprecise specification of the units of analysis undermines the force of many conclusions presented. Particular care is required with "household" because it is used as a "lay" as well as a "technical" term.
- 5) An assumption that the household is necessarily a site of joint preferences or utilities has been seriously questioned in recent

research; hence, the issue should be carefully treated in future research.

- 6) Farming systems research and other research frameworks for investigating the production, consumption, and investment activities of rural populations need to integrate information on multiple decision makers within households and on inter-household networks into their analytical and methodological techniques.
- 7) Particular productive regimes where multiple strategies of livelihood are the rule have led to insightful discussion of the permeability of household boundaries. Now, this type of understanding needs to be systematized and applied to other cases.
- 8) While poor or needy women and households must continue to be foci of research and action, researchers and practitioners should not equate women with disadvantage. Different categories of women at all socio-economic levels need investigating if a fuller analysis of development is to be achieved.
- 9) Similarly, the focus of any inquiry into the effects of rural development on different categories of women or households must be expanded beyond production.

- 10) Research on rural development will benefit from a more precise distinction between gender as a key principle of differentiation and women as social actors within specific social units.
- 11) Emphasis is placed on the incorporation of gender and of intra- and inter-household relations into analysis. Both the integration of gender as a significant variable and the more precise assessment of the key units of production, consumption, and investment are best seen not as separate "topics" in the analysis of rural development processes and policy, but as essential components in an integrated, structural framework of analysis.

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