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## GROUP 11. RESEARCH METHODS

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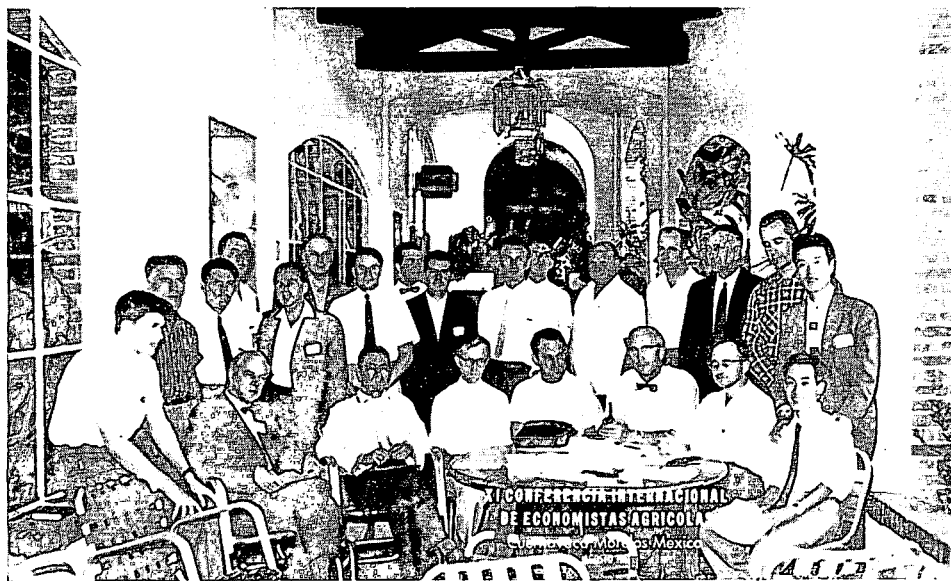
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It was agreed that the philosophic position underlying an approach to research in the social sciences influences the way that researchers define and solve their problems. Hence it would seem that some understanding of basic philosophic positions with respect to processes of inquiry is a necessary background for methodological discussion.

Another point of view claimed that emphasis on philosophic differences may not be constructive, and that the economist should concentrate on the difference between fundamental and applied research. He should define his fundamental inquiry in terms of finding invariant relationships. This, it was asserted, is what science is, and this approach minimizes concern with philosophical issues. It was stated too that the essential difference between fundamental and applied research is that the former involves theorizing with a view to developing new or more powerful tools which will help understand the real world; the latter applies theoretical tools. It was generally agreed that all research had a normative aspect, either implicitly or explicitly, and that it was desirable to recognize and state the normative implications of any particular piece of research. Some agreed that the normative aspect of research was concerned with 'good' or 'bad' in a philosophical context and should not be confused with 'good' or 'bad' in the context of statistical significance of results. Others argued that the two were fundamentally the same.



GROUP 11. RESEARCH METHODS

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*Fundamental v. applied, or problem-solving research.* Fundamental research is sometimes defined as tool-building, e.g. research to develop and refine the linear-programming technique. Applied research is tool-using, e.g. the application of linear programming to a particular problem. Though fundamental research may require more long-term investment, returns may be great. Applied research may require large, short-run expenditure.

The connexion between fundamental and applied research was viewed as two-way. Fact-finding, or positivistic research, was viewed as practical by some, but was later classed as fundamental. Problem-solving, or applied research, was viewed as at least partly normative. Some participants preferred to define positivistic or fact-finding work as 'research' or 'science', and problem-solving work as 'investigation'. Others wanted to define both as research. Some were willing to consider some normative work scientific and were unwilling to regard all fact-finding as research or scientific. It was argued in one instance that the methods of normative inquiry are not fundamentally different from those of factual (not necessarily positivistic) inquiry. A wide range of reactions to philosophic concepts was evident.

*Inter-disciplinary research.* Real world problems are generally inter-disciplinary, and the economist must generally have knowledge of other disciplines if he is to be competent. Given the inter-disciplinary complexity of many problems, how do we go about establishing a necessarily simple, but powerful apparatus? Perhaps at this stage of our knowledge, the best we can do is to *fit* the relevant economic model loosely into the particular relationships from other disciplines affecting the problem. The relevant relationships from the other disciplines can then indicate at least qualitative modifications in results which might otherwise be indicated by a pure economic model.

The team approach to research on problems of agricultural economics is extremely useful, so the economist should have a working knowledge of other disciplines, e.g. nutrition, soils, psychology, though he need not have professional competence in them.

*Roles of creativity and invention (physical, institutional, and normative).* Creativeness is an essential aspect of both applied and fundamental research. Training in abstract thought and methodology may depress creativity by over-developing the desire to use such abstractions and methods, and by conditioning one's thoughts. As an

example, some participants felt that a focus on linear programming might depress ability to conceive of and create new farm organizations. Similarly, concern with static competitive theory might preclude conception of non-competitive institutional arrangements as solutions to problems.

*Requirements for a theory.* Elements required for a theory were stated to be: (a) a set of undefined, or primitive, terms, (b) rules for using these terms, (c) defined terms and statements derived from the terms and rules, and (d) identification of terms and statements with experience. Opinions differed, but it was agreed that a theory to be potentially useful must show internal consistency among all its terms, rules and statements.

*Model construction.* Models were viewed mainly as parts of theories adapted by both abstraction and amplification to the requirements of a specific situation or problem. Both theories and models abstract from the complexities of reality by assuming certain variables through assigning their 'impacts' to probability distributions to be disregarded in theory and averaged out in empirical work. Still other variables are included in a theory or model as endogenous or exogenous. Both models and theories have the requirements listed in the preceding section. The difference is to be found mainly in the more detailed applicability of a model to a specific problem. This is obtained by assuming factors fixed in accordance with the specific problem, and also by regarding as random mainly unimportant variables which are also independent of the independent variables in the problematic situation.

A discussion of empirical work mainly involved sources of data including case studies, tours, observations on operating systems as in farm management, soil science, astronomy, and much of psychology, and experiments (problems in design). Much attention was given to the problems and advantages of co-operating with biologists (nutritionists, agronomists and animal scientists) in designing experiments to permit economic analysis. One of the problems involved in such inter-disciplinary work is the relative shortage of theoretical models in the related biological sciences. Other problems include excessive variability, uniqueness of universes investigated and the limited size of observation and/or lack of replication.

Farm accounts were attacked as failing to solve problems, but defended as providing facts in a positivistic, non-problem-solving way of great value to the profession. They were also attacked as unlikely

to produce representative estimates, and defended as not required to produce such results. It was held that they could be used to solve specific problems, and to keep extension workers in touch with reality.

Quality of data is closely related to the complexity of problems encountered in such fields as the study of national growth, human behaviour and responses to fertilizers. Inability to fix factors, limitations on numbers of observations, and non-randomness often make it difficult to avoid the influence of the large number of uncontrolled and unstudied variables. Problems of identification arise in supply-and-demand response studies, in growth studies, and in studies using behavioural data to infer normative characteristics of managers.

*Criteria for evaluation of research.* Knowledge produced, or to be produced, in a research effort must be judged by its clarity, consistency (internal and with observation or experience) and workability. Research efforts can also be evaluated according to efficiency criteria. It is necessary to consider the possible gains from (a) improved allocation of research resources (e.g. between fundamental and applied research; between types of research methods on given projects; between projects, between regions, &c.); (b) improved specification of the research horizon; and (c) more total resources being allocated to research with consequent gains from scale of effort within the total framework of a country's need for investment.