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Potential Roles for Social Scientists Interested in Natural Resources Issues in the Land Grant System

Discussant
Ted L. Napier*

I would like to preface my remarks by stating that much of the information regarding resource economics research presented at the November, 1984 seminar sponsored by the NCR-111 committee is relevant to practically all social scientists interested in natural resources development and probably to most physical scientists as well. Therefore, I refer to social science research rather than resource economics. I firmly believe that the issues addressed by the seminar participants are as applicable to natural resources sociology as they are to resource economics.

I have attempted to incorporate the essence of the group discussions which followed each paper presentation in the formation of this paper. Such discussions frequently clarify and elaborate issues addressed in more formal settings and the discussions at this particular seminar were excellent. A portion of these remarks are also the product of reflection on the issues addressed at the meeting upon my return to my own institutional setting.

Potential Research Roles For Social Scientists in the Land Grant System

Researchers in the Land Grant System may assume various types of roles and the preceding papers have identified several

*Ted L. Napier is Professor of Rural Sociology, Ohio State University, Columbus, Ohio.

which social scientists can embrace in the selection and implementation of a research agenda. The following is an attempt on my part to synthesize the various roles which social scientists may assume in the Land Grant System.

One of the first choices a researcher in the Land Grant System must make is to decide if he/she will engage in basic or applied research. Applied researchers primarily conduct research to address problems while researchers committed to basic research tend to be much less concerned about immediate application of the research output. Participant observation of numerous social scientists in the Land Grant System over several years suggests to me that applied research is perceived to be the most appropriate type of research. I suspect the reason for this commitment is the historical concern expressed in agricultural colleges for needs of client groups.

The papers by Biere and by Leitch tend to imply that basic and applied research approaches are not compatible but, in fact, both authors were commissioned by the NCR-111 committee to advance specific perspectives. Both authors articulate their respective position quite well but both are quite aware that applied and basic research efforts are not mutually exclusive. Data collected for the development of theoretical models could and probably should have considerable utility in terms of addressing applied problems. In my opinion, the apparent gap between applied and basic research is more myth than reality. I believe that when research is said to be "applied without any basic components" the researcher is not well trained in the methods and theories of his/her discipline to understand the

potential contributions of the research output to knowledge. I also believe that when research is said to be "basic without any applied components" the researcher is probably detached from reality and does not know how the findings can be used for problem solving. Researchers in the Land Grant System would be well advised to blend the two types of research approaches.

Once the researcher has made a decision regarding applied and basic research, he/she must make many other decisions which affect the type of research agenda which will emerge. The researcher can assume many roles in this process and several of the options are presented below.

One of the roles a Land Grant researcher can choose to enact is being a reactor to emerging problems. Reactors monitor change in the society and evaluate the actual or expected impacts of policy or processes of social change on affected groups. Such a role implies that social scientists will always be examining current issues. Such an orientation also suggests that reactors tend to be applied because they are problem oriented.

I personally doubt that many Land Grant researchers have the luxury of changing research areas quickly, given the human capital invested in specific content areas. Model building often requires longitudinal data bases to elaborate and validate past efforts. Extensive reallocation of staff and research resources to emerging problems and issues could be counter productive to research programs which require long-term investigation.

Another factor that retards change in research topics is the security professionals receive once they have developed national

and international reputations in specialized fields. Researchers continue conducting studies in their areas of expertise even when the topics are no longer defined as being important. It is difficult for established research staff to expend energies in developing new knowledge bases when they are secure in the type of research they have done in the past (historical inertia).

Another type of role that researchers in the Land Grant System can assume is being a reactor to funding sources. Researchers adopting this role permit the "research establishment" to determine their research agenda. Observations made during the past few years strongly suggest this approach has been widely adopted in the Land Grant System and that it is encouraged by administrators interested in securing external resources. I suspect that pressures to secure external funds for research-teaching-extension programs will probably increase given the decline in financial support from traditional sources. Such a situation suggests that the power of funding agencies to influence future research in the Land Grant System will increase. The role of social scientists in the establishment of research agenda could become one of seeking out the most lucrative "requests for proposals" rather than placing emphasis on the relevance of the issue to be studied.

Researchers in the Land Grant System can assume the role of being a research traditionalist. A traditionalist is one who has learned to do something well and will continue to do so because such efforts have been rewarded in the past and will probably be rewarded in the future. This type of professional values security so much that he/she does not wish to have any gaps in

records of accomplishments due to investment of time and effort in areas which may not produce desired outcomes on a short-term basis.

It is highly likely that the existing reward systems in the university setting perpetuate the traditionalist-type role player model. Junior faculty are frequently counseled to let senior researchers assume the risks associated with emerging issues and to conduct research that is "main stream" and has a high probability of being published. Unfortunately, when faculty reach the senior level, they have learned that the best means of achieving professional goals is to continue conducting research that has brought them recognition and promotions in the past. Subsequently, senior staff continue doing the same type of studies they have always done. Junior staff quickly conform to the expectations of the organizations to secure the promotions they desire so much. Such a system reduces the probability that "risky" research will be initiated by anyone. It is highly unlikely that researchers operating in such a system will initiate a research agenda that will open new areas for investigation.

I submit the traditionalist problem is not a function of conservative college or university administrators but rather is faculty-based. Research faculty are basically unwilling to assume risk in the choice of their research agenda. I suspect that Deans and Directors in the Land Grant System are much more willing to run risk capital than commonly thought.

Researchers may elect to assume the role of follower of discipline leaders in terms of establishing research agenda. This type of approach is based on the assumption that the recognized scholars should be "blazing the trails" for younger researchers to follow. It also assumes the "stars" of the disciplines are extremely relevant in terms of the topics chosen for investigation. Both assumptions may be in error because established scholars may have been influenced by historical inertia.

Researchers may elect to do their own thing in terms of developing research agenda. Research output may or may not be relevant but that is not perceived to be an important issue, since the person is self-actualized by his/her own endeavors. While I tend to favor professional exchange of ideas and frequent interaction with others who are engaged in similar research, I also recognize that many professionals perform extremely well as "loners" and should be permitted to operate in such a mode, if their research is relevant and meets professional standards of excellence.

Researchers may assume the role of being proactive to policy makers and funding agencies. Such an approach means that Land Grant researchers should "anticipate" the emergence of research problems and be prepared to provide counsel to decision-makers when the need arises for information. Such an approach also implies that the researchers in the Land Grant System must assume some responsibility for affecting policies and the establishment of research priorities of funding sources.

While I believe that social scientists are becoming more active in this type of role, I am not optimistic that Land Grant researchers will devote adequate time to influence natural resources policies or research priorities. I am even less optimistic that researchers will conduct research and theory development that does not have an identified client group because a demand for the output of future-oriented research may never emerge. In the event demand is not forthcoming for the research results, the researcher will have invested considerable time and effort without receiving professional rewards in the present incentive system. In essence, successful proactive professionals are those who are able to correctly predict the future or their efforts will never be appreciated or valued.

Social scientists in the Land Grant System can assume the role of advocate of a personal philosophical position or the perspective of a particular client group. While this type of research role could produce useful information, the scientist could become so strongly influenced by his/her biases or client interests that validity and reliability of the findings would be adversely affected. Generalizability of the findings to other settings could be sacrificed as well.

I suspect the trend toward greater reliance on external funding sources to finance research will enhance the probability that Land Grant researchers will become more client-oriented and more narrow in terms of research focus. It is possible that research output produced in this type of environment will become much more applied and be applicable to fewer situations. It is quite possible that broad policy issues will not receive much

attention because special interest groups will not be interested in funding such research.

Social scientists in the Land Grant System can elect to play the role of synthesizer of existing research. Such a role would require the professional to integrate existing knowledge and to interpret the materials in the context of public policy. The product of this type of effort would also be very useful for classroom instruction and extension programs. Individuals assuming this type of role must possess excellent skills in research methods-statistics and theoretical modeling because contemporary research is conducted using sophisticated tools of the social sciences. A person cannot play the synthesizer role unless he/she understands the materials produced and is able to correctly evaluate the strengths and weaknesses of the research output. The synthesizer role is extremely important in the Land Grant System because these people constitute the communication link between the generator of knowledge and the consumer. If the synthesizer cannot understand the information being produced, the information cannot be correctly communicated to potential users. I suspect that a number of synthesizer-type professionals do not possess the research-theory skills necessary to effectively perform the role.

Social scientists in the Land Grant System have traditionally been able to assume the role of being a producer of information not supported by the private sector. This type of role may become more difficult to assume in the future due to the decline in research support from traditional sources which were

willing to fund research that was exclusively in the public domain. It is also possible this type of role may be primarily assumed by federal agencies in the future because they are building extremely competent research capabilities and are able to provide researchers the time and support resources to investigate such complex issues. Public agencies are also better organized to expend resources on research topics which have high degrees of risk attached to them. Criteria used to evaluate performance in the agency situation are quite different from those used in academia and do not penalize the researcher if the research efforts do not produce immediate results or even fail to produce useful output. The researcher does not have to internalize the costs of high-risk research which suggests that scientists will be more willing to undertake such research efforts. Miranowksi's discussion of the Natural Resources Economics Division of the Economic Research Service is a good example of a federal agency that may assume such a role in the future. I firmly believe that the Land Grand System should be aggressively engaged in research that will not be funded or perhaps even politically supported by the private sector. This is especially true for controversial topics of national concern.

Lastly, social scientists in the Land Grant System can choose to play the role of the provincialist in terms of the focus of the research. Research may be conducted on the local level or on various levels of aggregation. My observations to date suggest that too many social scientists in the Land Grant System tend to be localistic in the choice of research topics. Such a situation is not necessarily inappropriate, if the

findings have generalizability to other areas of the state, region, nation or the world. Unfortunately, too many of the micro-oriented studies have relatively little applicability beyond the bounds of the study population. I suspect that provincialism is a product of Land Grant researchers responding to local client needs rather than selecting research topics which are relevant to larger publics.

Selection of Research Topics

Social science researchers in the Land Grant System not only must choose an appropriate role to play in the establishment of research agenda, they must also choose the type of approach they will employ in the selection of topics. Discussions at this seminar suggest there are at least two approaches for selecting research topics. The two approaches are the conservative approach and the risk-taking approach.

Conservative Approach

The major components of the conservative approach to the establishment of a research agenda are as follows:

1. Do research that is professionally and socially accepted.
2. Do research that will attract research money.
3. Do research that is high on the priority list of university administrators.
4. Do research that is easiest to do in terms of your present research and theory skills.
5. Do research that will give you recognition in the media and your discipline quickly.

6. Place the highest priority on the relevance of your research to client groups.

7. Do research that has the highest probability of being accepted in the major journals.

8. Do research that is certain to produce rapid results. Do not be concerned about building research bases for theory development and model building.

9. Do research which permits use of previously developed models. Multiple application of previously developed models (especially those developed by other researchers) increase the number of publications which can be produced.

Risk-Taking Approach

The major components of the risk-taking model for establishing a research agenda are as follows:

1. Be innovative in the selection of methods and theories used. Do not rely solely on existing skills. If the research demands new skills, then develop them.

2. Be willing to make significant shifts in terms of research topics and theoretical models to be examined. Investigate emerging issues and select relevant topics.

3. Anticipate topics that will be critical issues in the future. Be prepared to provide information in these emerging areas when the need arises.

4. Listen when people begin talking about "futuristic" ideas. Do not reject "wild" ideas too quickly because someone may have insight that could open new research horizons.

5. Be willing to commit personal resources to topics, methodologies, and theoretical modeling that have potential but have low probability of producing results in the short-run.

6. Advance research ideas to funding sources even when they do not have "Request for Proposals" posted which are relevant.

7. Be self-actualized in the selection of topics. Researchers should know when an idea has merit.

8. Be as concerned about the creation of new knowledge as you are about the needs of special client groups.

9. Be concerned about the long-run impacts of the research as well as the short-run effects.

10. Be concerned about building models for prediction purposes.

Predictions of the Future

Predictions are always tenuous at best and based on assumptions that recent trends will be maintained at least for a short period of time into the future. With these cautions, I will summarize what was discussed, implied and theorized by the program and seminar participants. I assume the sole responsibility for any misinterpretations of statements made by participants and readily admit that the positions stated in this paper and particularly in this conclusion section reflect my assessment of present trends.

Soft-monies will probably increase in importance for funding research endeavors in the Land Grant System at least during the next decade. During this time period, it is highly likely that

external resources will be sought with even greater intensity than in the recent past. Researchers will continue to be reactors to research priorities established by funding sources and to prepare research proposals for attracting external monies with relatively little regard to the content of the research topics. One of the greatest problems for researchers and administrators, particularly at the department level, will be directing research programs that by the nature of the funding sources will tend to become fragmented. It is also highly likely that research under these conditions will be less cumulative and probably more applied. Long-term planning to establish research agenda for departments will become more problematic because the availability of external grants will negate prior planning. It is highly likely that most academic departments will establish general objectives so that external resources can be secured from a broad spectrum of funding sources.

Professionals in the Land Grant System will continue to do some model building and do so very well. There will be some theory development which will press the bounds of knowledge. The greatest amount of research conducted by Land Grant researchers, however, will probably be focused on immediate problems which may or may not be relevant to a majority of publics we have been commissioned to serve. Needs of the less vocal and less powerful client groups will probably not be served because they cannot pay for the professional services nor do they have the political influence to direct the declining public resources to address their problems.

In essence, many researchers in the Land Grant System will become quasi-consultants in an academic setting. This will certainly be true for the best researchers who have research and theory skills needed by special interest groups. It is quite possible that the successful university consultants will transfer some of their loyalties from the "public" to the client groups that pay their fees and provide them grants.

An alternative system may emerge which consists of private consulting. Research faculty frequently can conduct research at much lower costs as private individuals than they can as university staff. Indirect costs make university-based consultants very expensive. If research staff are forced by circumstances to secure external resources to fund their university activities, they will begin to secure the grants for themselves rather than for the institution. If the university setting becomes more like a consulting firm, it is highly likely the research staff will seek alternative reward systems since consulting firms reward their professional staff much better. The attractiveness of the university as a institution for innovative and independent research may change drastically. Assured funding for creative thought without regard for special interest groups is probably a thing of the past in the Land Grant System.

I conclude from the printed materials and discussions presented at this seminar and from personal observations made during the last decade that the number of roles which social scientists (perhaps all scientists) is the Land Grant System can realistically select are becoming fewer over time. The evidence

suggests that many researchers are not particularly concerned about the constraints being placed on the number of choices made available to them and that they have basically defined the changes as being acceptable. The sources of the restrictions in the choice of roles made available to researchers are numerous. Loss of economic support has reduced the freedom administrators have to permit research faculty to engage in research that is on the frontiers of knowledge and is highly risky in terms of producing usable output. Reliance on special interest groups to support research has produced considerable pressure to do applied research that has limited applications and that makes relatively little contribution to theoretical modeling. Reward systems have placed emphasis on rapid research output and sustained publications in major journals which has tended to constrain the choice of topics and research approaches used. Attitudes of faculty toward involvement in the establishment of research agenda has permitted funding sources to determine research priorities. Lack of influence by numerous segments of the public has resulted in many research needs being ignored.

The identification and discussion of all roles that social scientists could play in the Land Grant System are beyond the scope of this paper but those provided are indicative of the potentials we have for consideration. The major obstacle to the establishment of well-integrated and relevant research agenda for Land Grant faculty appears to be the <u>willingness</u> of researchers to assume the responsibility for directing the destiny of social science research in the Land Grant System. I strongly suspect

that our Deans and Directors would applaud initiatives on the part of research staff to assume the responsibility for influencing research priorities of the federal and state agencies. I also suspect that many persons engaged in the establishment of research priorities within funding agencies would welcome constructive input from research faculty with empirical data to support specific research needs. Efforts expended by NCR-111 to date to influence policy and research priorities in the area of natural resources development is evidence that such an approach has potential for producing positive results.