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# POTENTIAL OF RESEARCH ON WORLD FOOD AND NUTRITION PROBLEMS

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August 2, 1977

## INTRODUCTION

This paper will draw on two years of experience with the World Food and Nutrition Study (WFNS) of the National Academy of Sciences. I will give you a bit of background about that study, but my comments will not be restricted to it. Primarily I will define a professional issue -- namely that we as a profession lack a meaningful and exciting research agenda for our role in tackling world food and nutrition problems. Moreover, we are failing effectively to communicate both our skills and our needs. I am not discussing a new topic, e.g., Farrell did a most scholarly paper on many of these issues in his AAEA presidential address. Certainly the paper given here by Maurice Kelso will be a classic in our literature. My contribution lies especially in the chance to discuss a case study which involved substantial input from our profession. I will be optimistic about our profession and about the potential quality of life for most peoples worldwide.

## THE WORLD FOOD AND NUTRITION STUDY

This study was originated by a request from President Ford in December 1974 following the World Food Conference in Rome. He asked the National Academy of Sciences to assist in a "major effort to lessen the grim prospect that future generations of peoples around the world will be confronted with chronic shortages of food and with the debilitating effects of malnutrition."

The issue specifically focused on how the United States scientific community could give leadership.

We interacted with about 1,500 of the outstanding food, nutrition and related scientists in the United States and throughout the world. The study was organized around: 12 teams of experts covering the various subject matter areas; two "blue sky" teams to look at the unusual possibilities (one, primarily of physical sciences emphasized supply and one primarily of social sciences emphasized consumption); one team to look at organizational changes; and one overall team to excercise some priority assessment of the research proposals of the other teams. The teams had both general and applied scientists. Two hundred and one people served directly on our study teams with 80 of those being social scientists, predominantly agricultural economists. The social scientists were diffused throughout the teams. Often they were limited to specific subject matter. To some extent, we may have taken them for granted and probably they were not given as much attention as were the others. This was particularly true with regard to defining their own specialized areas of research. A steering committee, several of whom were social scientists, gave overall supervision to the study. The dominant professional background of the staff was social science.

The team members were asked detailed versions of three questions. First, they were asked to delineate areas of research which their talents could address to alleviate constraints on availability of food and nutrition worldwide. The idea was to tackle both frontiers of science -- external and internal (Weisskopf, p. 406, 1977). They were to avoid giving us a shopping list, but rather selected priorities which were head and shoulders above others they might list. Second, they were asked to evaluate the effects

expected from this research, particularly in giving U.S. leadership to aid in solving the global problem (Arndt and Ruttan, 1955). Third, they were asked to lay out the institutional, financial and policy considerations relevant in getting the job done. They were not constrained by resource limitations, but they were expected to justify their choices. Most teams came up with a rather short, substantiated, and well defined list of priorites.

The study has resulted in two reports prepared by the steering committee -- an interim report (NAS, 1975b), [accompanied by a special NAS study on U.S. research priorities, (NAS, 1975a)] to President Ford and a recent final report to President Carter, (NAS, 1977). These are being followed by five more volumes containing the specific reports to the steering committee by individual teams, and including a social science volume. The two reports to the White House have been made public; the others will be soon.

Let me make two, probably unnecessary, caveats. These refer to an editorial technique that I will use, but would avoid if I knew how. That technique involves comparison of social scientists with their counterparts. First, no attempt is made to downgrade social scientists -- they compare favorably by traditional peer evaluation techniques regarding knowledge of subject matter and method. Second, no attempt is made to show disappointment with any social scientists who worked with us -- they represented their disciplines effectively and accurately. All comparisons are made to suggest tangible things we might observe and possibly emulate. Suggestions for improving our discipline do not deal primarily with professional stature; they deal mainly with the modus operendi by which we can more effectively help tackle the global problems of hunger and malnutrition. 1/

## THERE IS SOME URGENCY

If you sense some urgency in my comments, that is intended.

The age of complacency for our profession is gone. We talk about our professional direction at every AAEA meeting. For the most part we come out saying, "Tomorrow we must get organized!"[2/] That is not good for morale. Compared to other scientists in the WFNS, we seemed to lack in pride and scientific camaraderie; our frustration level was higher; and our satisfaction level lower.

Our potential emphasizes the urgency. The scientific community in general has been subjected to a catharsis. Scarcity, human rights, social justice and challenges to the scientific method itself have laid clean some broad opportunities for research skills. The problems are complicated. But we have a comparative advantage for many of them. We are dealing with inter-science, inter-politics and international issues, particularly involving resources, food, and policy making. Farrell (p.786) says "... it is increasingly difficult -- if not impossible -- to analyze and understand agriculture and related public policies except as an integral part of national and world economic, social, and political systems." Warley (p.820) indicates the point forcefully saying, "foreign economic policy is now the core of foreign policy."

A profound and effective paper by Emery N. Castle, "Renewable Resource Management: Meeting World Requirements for Food and Fiber," has done much to stimulate me -- both by its forthright evaluation of our past contributions and by its definition of an important new direction for us. I listen when Emery Castle concludes on the realities of adapting to technological change as follows: "Obviously, social adaptations are needed on a global

basis to these realities. Social adaptations and social control mechanisms are needed to cause individual behavior to be consistent with social objectives. These mechanisms have been modified indirectly by natural science through the widespread adaptation of technology and they have occurred as a result of social experimentation and learning, stemming from technical change. They have not been changed to any significant extent, I believe, because of knowledge advance in the social and behavioral sciences although Adam Smith, Karl Marx and J.M. Keynes might be cited to the contrary."

Such is his dire evaluation. But as usual Castle does not leave us there. He goes on to offer some new direction:

"Let us assume that the research succeeds in doing what the scientists say it holds the prospect for doing..... The question then becomes -- how will this be viewed by the millions around the world who make the crucial decisions on family size, education, human health, production, harvesting, and distribution? Will it be viewed as a permanent expansion of the opportunity for even greater populations and even greater consumption of material 'things'? Or, will the increased output carry with it appropriate incentives for future adjustments?

.....

A major re-orientation of research, primarily but not exclusively, social and behavioral in nature, is recommended. Such a re-orientation need not be highly expensive, as compared to natural science research efforts....

At the present time much of the social and behavioral science research and technical assistance is of a narrow scope on production and problems within the developing countries.....

..... wants, desires, and hence, consumption, are largely 'givens'

rather than variables to be explained in many social science investigations. This needs to change (Castle, 1977)." A sociologist, Philip Olson, recently gave a paper in which he surveyed recent sociology literature emphasizing these same ideas (Olson, 1977). Kelso also gave this emphasis.

## THE CURRENT SITUATION

Let me sketch some characteristics of the current situation of importance for our current issue. Some are findings of the WFNS; some are not.

Food and nutrition are much in the limelight. Energy is the spectre of weakness of the commercial world, but hunger is the spectre of the poor, and that is most of the world (International Fund Policy Research Institute, pp.1-4, 1977). U.S. policy has put food to the front starting especially with the China-Russian compromises. This policy is under severe test as we all know, but that is the policy. Terms of trade and population trends portend an emphasis on food. This is not so much to say that hunger and malnutrition are more urgent than in the past. The difference lies in the fact that for the first time in history, we have a good chance to do something about hunger and malnutrition and both the Third World and we know that. Warley (p.825) speaks of the "The success of the developing countries in shifting the subject of poverty from the periphrey of world affairs to the center." Also, the newer emphasis on the adverse effects of poor diets on health have added concern (U.S. Congress, pp28-46, 1973).

A cohesiveness of concern about this problem has evolved worldwide. We all know about the Rome World Food Conference of 1974 and the follow-up conferences. Most major countries have programs of research and assistance

(Cordaro, p.5, 1977). In the WFNS, we found sincere concern and evolving commitment worldwide to do something about this problem. This ebbs and tides, but it has solidified and has a permanence.

The basic conclusion of the WFNS was one of scientific and technological optimism. The concensus of the scientists was that they could meet the challenge laid out by President Ford if other members of society handled their part effectively. They particularly were concerned that the political and social problems be handled; otherwise they laid no bets on the chances of success. Wharton in his paper at the World Food Conference at Ames had held that this was his view also, (Wharton, 1976). Others echo the same thing.

Probably the most innovative and substantive contribution of our study was the couching of this problem in much broader terms than usual. The study discusses the political will of the poorer countries, joint programs with scientists worldwide, and social science research with special emphasis on poverty and the myriad of debilitating effects which poverty and hunger have on people. This is much more than the second-third-and fourth-generation problems of technical advance. This broader orientation does nothing to play down the importance of technology. It merely reorients it in a broader formulation. The press has emphazised this difference, and the reaction has been favorable. It is consistent with the current mood. President Carter in his special address worldwide on inauguration day highlighted hunger and poverty in his human rights pledge (Overseas Development Council, p.viii, 1977).

In the WFNS, the need for priority assessment in research came through clearly. The public is tired of hearing scientists merely asking for more money. They want convincing arguments from scientists as to what taxpayers can expect if resources are given to the scientists. This study probably consolidated the largest consensus judgment ever by scientists to justify their existence. But methodologically, the scientists demonstrated to us time and time again that unless you discipline them and force priorities, they simply will not make these choices.

A healthy respect was demonstrated for agricultural research by the general scientists and poicy makers. Also, a health optimism about the contributions to be made by social scientists was evident. This was optimism as contrasted to outright respect. Most general scientists did not doubt that social scientists could do the job. The applied co-workers (primarily from the USDA-Land Grant complex) of the social scientists were doubtful but mainly supportive. The attitude was basically one of "wait and see."

There will be increased scientific thrust, (National Science Board 1976). We recommended 20 percent increase in real terms immediately and 10 percent per year increase the next four years. Several factors add strength and momentum for food and nutrition research. General scientists can get research funds out of the broad public concern budgets, e.g., space, health, energy and environment. And they will do so, often by incorporating the food and hunger problem into their research proposals. The agricultural scientists have gained respect (e.g. for the upgrading of personnel and for the way scientific knowledge has helped farmers handle the corn blight problem, to adjust to recent weather changes, and to make an important impact on foreign trade and the level of food prices). Science maturity in this

country is recognized. An important scientific international network is evolving, from the international centers to a growing community of science.

The role of the universities in research may well increase. The Title XII Program formalizes the potential in the world food and nutrition areas, but the story is broader. The universities need the money, and research funds are probably their most attractive potential source for increases. Also, some evidence shows that the private sector emphasis on research may be declining; the public may need to pick up that slack.

Having said all this, I now must say that the case put forth for social science research in the research in the WFNS was not exciting to the other scientists, the staff, and the steering committee. (Table 1). The proposed research on national food policies and organizations gave the impression of a massive, and possibly an impossible, undertaking. The need and importance of it were accepted, but counterpart scientists expressed concern about its acceptance by the policy makers, and openly admitted failure to understand what was to be done. They understood the trade policy research better, and the food reserve research was well received.

The management work had support but mainly as service to a host of other people. This was true of the benefit-cost work as well. Moreover, the benefit-cost work was often thought of as something to be done ex post facto. The work on information systems was an exception -- the proposed information management work was substantive. Also that inter-disciplinary report itself was strong. I did not include it in Table 1 as being specifically social science in orientation.

The marketing and food science work did not impress many as being scientific or important, and mainly was considered only as an extension of production.

Table 1. Recommended social science research priorities, World Food and Nutrition Study, National Academy of Sciences, 1977

| Priority area | Nature of research effort | Major effects | Sources of support |
|---|---|---|---|
| I. Policies and organizations | | | |
| A. National food policies and organizations | Improve policies and organizations affecting food production, distribution, and nutrition in developing countries:<br>1) human performance in food systems;<br>2) comparative studies to identify transferable improvement factors (decentralization, local participation, staff development);<br>3) interactions of income distribution with food production and nutrition;<br>4) methodology of sector analysis | Early results in improving effectiveness of policies and organizations relating to food systems and orienting selection and implementation of other biological and physical research; give farmers incentives for production and provide prices that will give more effective distribution | AID, NSF, USDA |
| B. Trade policy | Improve effects of trade policy on food production and nutrition:<br>1) studies on effects of trade liberalization;<br>2) consequences of international management of trade;<br>3) optimum trading patterns | Early effects on orientating country food policies for balance between own production and reliance on trade; improve diets, incomes, and national economic performance | USDA, AID, State, DOC |
| C. Food reserves | Improve role of reserves in relation to other measures for stabilizing food supplies:<br>1) improving developing country food reserve practices;<br>2) identifying improved mixes of reserves and other measures to stabilize food supply | Relieve hunger and malnutrition due to production instability | AID, USDA |
| II. Management | | | |
| A. Farm production systems | Improve production systems, particularly for small farms in developing countries:<br>1) methodology for identifying | Realize potential for two to four times present production in humid tropics; more modest increases in semiarid tropics | AID, USDA, EPA, ERDA |

Table 1. (Continued)

| Priority area | Nature of research effort | Major effects | Sources of support |
|---|---|---|---|
| | 2) multiple cropping;<br>3) soil and water management;<br>4) equipment-labor relationships | | |
| B. Management services to other groups | Improve the functioning of technical areas by giving management services:<br>1) pest management<br>2) tropical soils<br>3) water and irrigation<br>4) aquatic systems<br>5) weather and climate variation<br>6) information systems<br>7) food losses | Much more efficient use of resources in many other special areas and when interdisciplinary effort is needed | AID, USDA, NASA |
| C. Benefit-Cost analysis | Assess the expected returns from other programs:<br>1) Chemical fertilizers<br>2) Animal diseases | Orientation of programs of research and use for these two areas | AID, TVA, USDA, NSF |
| III. Marketing | | | |
| A. Market expansion | Extend market scope for consumers and farmers in developing countries:<br>1) enhancing purchasing power;<br>2) transportation;<br>3) marketing institutions;<br>4) managing marketing flows of major commodities | Stimulate production and consumption and cut food losses. | AID, USDA |
| IV. Nutrition | | | |
| A. Policies affecting nutrition | Improve effects of full range of government policies:<br>1) effects on nutrition of policies and practices usually formulated with no consideration of possible nutritional consequences | Nutritional improvement in short run; in long run dietary changes may benefit health and life expectancy of large population segments in United States and developing countries | NIH, NSF, USDA, AID |

The nutrition policy makers expressed interest in the social science input, but they viewed a rather narrow range of social scientists as being helpful.

This may be an unnecessarily pessimistic view. It may put too much emphais on the attitude of the counterpart scientists, the staff and the steering committee as proxies for the scientific community and the policy makers. Yet one has to be honest -- the agenda failed to impress these groups relative to the support they gave other areas.

The social science proposals put forth were from several sources. The study team reports usually were from teams which had two social scientists among possibly a dozen or more counterpart scientists. Several teams were predominantly social scientists, but often their work was circumscribed, e.g., to research needs on methodology, trade, information systems, or food availability. Various conferences on social science input were held with public and private groups and individuals. Much staff and steering committee effort was devoted to the subject. The basic problem was not that we did not have good ideas, and many of them. The problem was lack of a well defined, finite set of priorities which were understood by, and excited others.

## PROFESSIONAL IMPLICATIONS

The study design and organization may well have put the social scientists at a disadvantage. Yet, concern about our professional orientation is not limited to this study or to me. Thus, what can we learn?

Six things about the posture of social scientists stand out. All six made the job of the social scientists in the WFNS more difficult.

First, their professional opinions were more varied than those of their counterparts. They spent much more time agruing among themselves, particularly with regard to what constituted their most important research opportunities. They had more difficulty in compromising for their common good. Interestingly, the most outspoken critics of the social science input were other social scientists. This was not generally true in the other fields. Diversity of thought is a cornerstone for scientific goals and objectivity, but can be overdone. We may have outgrown the need to "discuss" everything to death. For example, several of us have long thought the inordinant use of discussants at this meeting is an anachronism.

Second, the scope of the subject matter, and probably method, spanned by social scientists was wider than that of their counterparts. The other scientists were thrown off guard by the lack of specialization of the social scientists and the quickness with which the social scientists would claim their proficiency and the feasibility to research so many areas. Ronald Freedman, a member of the WFNS Steering Committee, summarized this feeling: "The research agenda is so broad and sweeping that, if it could be successfully attacked, the social scientists would have made more progress than in the whole last century." He showed his social science background (demographer), going on to say "it is, perhaps, inevitable that an honest view should give such an impression, because the areas are all important and the social science knowldege that is reliable is meager in most of the major areas." The fact is that our study asked specifically for priority recommendations, the best-foot-forward, of the social scientists. Relatively, this came much, much harder for the social scientists than it did for their counterparts.

Third, the social scientists are understaffed and poorly resourced. The other scientists often asked whether or not the social scientists had a "critical mass." The counterpart scientists were serious in saying what they could do, but they were just as serious in saying what it would cost and how many more scientists would need to be trained if society expected them to do the job. The social scientists seemed to be willing to take on things much more quickly, and dollar and manpower costs were often an afterthought.

Fourth, social scientists do not delineate well between their service role and their specialty competence. Their versatility causes confusion about the role that is expected of them by both other scientists and policy makers. Also it probably relaxes the priority assessment pressure on them when they are deciding what they can do specifically on their own. Social scientists have a pride about their interdisciplinary competence. The WFNS organization put emphasis on interdisciplinary work; this made it difficult for the specialized competence areas to evolve. The problem is inherent in our profession. The service demands are immense and they are competitive with other things. We must do something to delineate the service role, decide how we can handle it, and articulate our professional policy about it to others. Confusion on this point clouds our respectability. Glenn Johnson has proposed his troika of (1) problem-solving research, (2) subject matter research, and (3) disciplinary research. This is a start. Our key social science study team sorted their recommendations along this line, but it has not sold too well. But this notion deserves consideration.

Fifth, social scientists were more resistant than their counterparts in trying to estimate the impacts of their work. This may have been a more difficult role for them, or they may be more knowledgeable about the pitfalls

in such activities. But the WFNS is unique in the degree to which scientists did attempt to justify their existence. So far, policy makers and funding agencies are impressed with this aspect.

Sixth, social scientists had more trouble communicating with their counterparts than the others did among themselves. This probably could have been expected. But it caused at least two problems. It tended to be used as an excuse for not ironing out mutual scientific notions between social scientists and the others. Also, it resulted in the burden of communicating being placed upon the social scientists. The counterpart scientists has the social scientists on the defensive, e.g., they would ask, "how do you research an institution -- anyway, what is an institution -- a farm, a court of law, a family, a marriage?" By then the social scientist was frustrated, if not mute.

Thus we are too opinionated, too broad, too poor, too overworked by others, too cautious, and too hard to understand! In addition, there were times when I thought the agricultural economists must be professional orphans. The family disciplines for most counterpart scientists were the basis for their underpinning and often the stimulant for their pragmatism. Kelso demonstrated how essential this is. This problem came to me many times when counterpart scientists asked me or other social scientists what we considered to be our basic research needs, and so many times we mumbled a nonanswer. I wonder if the general economics profession is pushing us hard enough — but that is the basis for another paper.

THE OPPORTUNITY

My enthusiasm for the potential of social science overshadows my concerns about the WFNS problems and my rationalization for them.

The time has now come for someone to lay out the research agenda! So, I will list eight areas. Unfortunately (or fortunately) their discussion must be brief.

This list may verify my own inability to avoid the generic dilemma -- so many exciting things for us to do and so little willpower to say "no!" But the list attempts to use the experience of the last two years; the thought, work and frustration of my many colleagues who struggled with this subject; the broad picture of what other scientists are doing; my inability to explain my professional colleges to many others who wanted to support us; and my attempt to select research that could be additive and hopefully holistic.

Allow me to return a moment to Castle's new direction. He said

"Examples of the kind of research issues I believe should be on the agenda for high priority examination are:

a. Investigations which will lead to a better understanding of population growth under a variety of conditions.....

b. Comparative analysis among nations and states of adjustments to particular resource endowments.....

c. Investigations which will lead to better understanding of the compatibility of systems and organizations with the objectives of the larger society.....

These investigations will have the following characteristics:

1. They are concerned with social control mechanisms.....

2. They will be concerned with the linkage and the flow of information between large systems and individual decision makers.....

3. They will constitute a significant change in perspective..... they will tend to concentrate on the determinants of consumption over time....." (Castle, 1977).

Resource economists are making important contributions to the formulation of this type of problem. Castle has an important orientation, and Kelso has now given us an exciting personal testimony that intellect, insight, hard work and great patience actually can integrate a piece of our integrative science. The trick is to avoid Boulding's devil of suboptimization -- a theme which my management research has twisted in my gut much as Kelso's land economics twisted it in his.

Meaningful problems in this world food and nutrition characteristically are broad. Optimization equipment is difficult to construct. Too often an important part of the problem is assumed away to fixed constraints -- we did this with early linear programming formulations involving the divisibility problem on fixed costs. The paradox of optimization lies in its increasing power as the problem is expanded. This is the thrill that motivates the large-model optimizer.

Kelso and Castle scare me as they expand our definition of consumption in such problem formulations. This becomes especially troublesome in world food and nutrition research with its geographic and time-dimension variations. But I am convinced that we must try. We may need the leavening effect of some of the notions of Lindblom's "muddling through," but we do have a chance of surrounding these massive problems.

Now for my list of eight.

People around the world need help on policy evaluation. We have techniques and tools, such as sector analysis, for such evaluations. Also, we have some understanding of policy implications, part of which might help others, especially on policy dealing with technology. Policy evaluation is an area of our strength. In our policy evaluation research itself, we need work, i.e., more work on food reserves. We might do some method work on the potential of business meangement planning techniques as a way to assist developing country goverments to avoid policy by crises. Many other possibilities exist. The areas outlined in Table 1 are researchable. We should continue to play to our strength in this area.

Trade is essential if we are to avoid the temptations so attractive to technologists and others to make every country self-sufficient. Inter-regional trade research within countries and among groups of closely aligned countries is a need. The appropriate emphasis on self-sufficiency is a major problem which we must tackle. Presumably we have the tools to do so. The profession has delineated the trade problems somewhat effectively and their judgment is reflected in the WFNS. But overall we are not staffed and organized well to do an outstanding job on trade. Some good trade work is being done and I am encouraged. But we need critical masses at more institutions and more collaborative work with analysts abroad, including emphasis on our traditional markets as well as developing competition (Krause, 1976).

Global models for coordination of production and consumption decisions are needed. These are more than current world models. We assessed the world model work in selecting the methodology for the WFNS. These models must be strengthened greatly if they are to do the job. Such research also may give the profession an opportunity to do more with the theory of holism. Currently

we scientists use it as a buzz word to replace "scenario" which we have run into the ground. As I understand holism, the theory does not merely state that synergism gives a total greater than the sum of the parts. It also contends that a holistic component is identifiable in and of itself and should be the focus of the conceptual notions. The statisticians did more when they decided to look at the error term for what it was than they did when they handled it as a nuisance. Possibly we have another handle for big problems. Thrust of the world modelling effort is certainly not in our professional hands. If we we are to do much about that, we have much work ahead of us.[3/]

Research on institutions and their place in development was much discussed in the WFNS. The need for such research is dramatized by a crucial decision in the WFNS. We concluded that our current knowldege was inadequate to design a food and nutrition institutional arrangement that would assure a high priority research agenda. So we were forced primarily to tackle the agenda itself. Some think that our approach was frivolous, but we considered it our best alternative.[4/] A growing body of literature on institutional analysis has evolved. But, we do not have a solid social science institutional research thrust, and a dire need for it exists.[5/]

Farm management may be in for rebirth. Many questions asked in the WFNS had to do with some of the simple concepts of farm management and where farm management fits in the total development process. A serious adaptation will be needed to improve our knowledge about economies of scale, especially for the small farm in the myriad of settings in which it is found worldwide. The WFNS makes a strong assumption, along with many other studies, that we must get world production up substantially through smaller farms. It may be

sacrilegious, but I hold that social scientists have a significant responsibility to test that hypothesis. We have some good work going at Purdue and elswhere, but so much depends on the outcome of that test that the work should be strengthened. The emphasis on small farm development in the centrally countries is a special problem of note (Lazar, 1977). Hopefully the farm research can be coordinated with needed research on household economies.

Technology diffusion research has had some inputs from this profession, but mostly it has been done by sociologists. Also, my impression is that the amount going on currently is nominal. Probably the most asked question in the WFNS was whether or not social scientists could explain why we were not getting more adoption of science and technology findings. Some in-house assessment of the value of social science research generally might be quite important, and cathartic.

Marketing and business management research will be needed for improved world food and nutrition. Probably it is already overdue. The research in this area relevant to developing country problems was not impressive. Michigan State and Harvard work is encouraging, but inadequate. The private sector work gave signs of declining and it is too sketchy to be a major factor. Research on the role of the market as a link between commercial systems and agrarian development is recommended.

The poverty problem, as viewed by many including the Steering Committee of the WFNS, is nearly an identity with the world food and nutrition problem. Problems of the poor are obviously intertwined with the problems of hunger. Will social scientists do much about this problem? Our track record on poverty has been miserable in our own country and in our

profession. We should make a decision as to whether we want to gear up for this one worldwide. If we do not register a disclaimer we will be held responsible for research in the area.

## IN CONCLUSION

The WFNS adds support to a strong public and scientific expectation for social science research. Agricultural economics can lead the way for this research on world food and nutrition problems. The profession has the potential, especially in its own scientific personnel and in external support. Ironically, the biggest problems of the profession currently are in applying its own expertise in better managing its own profession. But doctor's wives die young, and psychiatrist's kids are often truants or delinquents!

## FOOTNOTES

I especially would like to acknowledge the ideas and support from colleagues on the staff and study teams of the WFNS. Charles E. Hanrahan and Emery N. Castle gave helpful reviews of an early draft of this paper. Yet, in no way does this paper purport to represent them or the findings of the WFNS per se.

1/ Among the job specifications assigned me in the WFNS was staff responsibility for the economic input. This acknowledgment should make clear the ultimate responsibility for any concerns about such input.

2/ This overstatement, of course, is unfair to many fine papers given at this meeting in prior years, especially the papers by Farrell, Soth Schuh, Barkley and Warley in 1976. (AJEA, December 1976).

3/ At least one recent analysis holds that we should be more involved in the world modelling effort as it is now evolving, such as at the International Institute for Applied Systems Analysis in Austria.

4/ We did make important organizational recommendations in the WFNS, but at best we compromised and worked on both organizational and subject matter priorities. I have elected specifically to avoid discussion of the organizational recommendations. (I am pleased that they are discussed elsewhere in this meeting.) Another proposal was that we wait and have the social scientists make the priority assessment, but we thought urgency foreclosed that possibility.

5/ See the forthcoming report of Study Team 7 of the WFNS where one classification for institutional research is proposed.

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