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Agroenvironmental Economic Research for the 21st Century: Anticipating and Responding to Change

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It is proposed that "real" research issues are socially relevant, provide findings with utility beyond the profession, and are anticipatory. An industrializing agricultural sector, an evolving political economy of agroenvironmental policy, an increasingly transnational economy, and rapid population growth are important sources of change to which research on real agroenvironmental issues must respond. Specific, identified "real" research issue areas include: benefit-risk assessment methodology; agroenvironmental regulation for industrialized agriculture; trade agreements and environmental quality; the recreation-tourism-agriculture interface; sustainable development; and the formation of preferences. The nature of identified issues suggests more interdisciplinary research, and advances in theory and methodology.

The need for credible, relevant economic research and information on the many facets of the agriculture-environment interface has probably never been greater. Analytically-based information is needed as input to a growing number of legislative issues, administrative and planning processes at national, state, and local levels, legal disputes, farm and firm-level decision making, and the evolution of new and emerging markets and institutions. Meeting the exciting challenge this demand poses will require a focus on real rather than hypothetical or presumed problems, and the development of new and innovative analytical approaches. However, casual observation suggests that, with bright glimmers of individual exception, the agricultural resource/environmental economics profession has adjusted its issue orientation and methodological procedures much less rapidly than the world around it is changing.

In this paper, I give a personal interpretation of what characteristics distinguish "real" issues for investigation from less substantial issues, describe the nature of changes in the agricultural and environmental arenas that are likely to shape the set of "real" issues in the year 2000, and then discuss a variety of specific issues on which we can expect there to be high future demand for research. I con-

clude with some thoughts on what the shifting set of issues implies for intellectual and methodological approach to agroenvironmental problem solving.

What Makes a Research Issue "Real"?

Deciding what research topics are most important is a subjective process shaded by one's professional and personal experiences, values, and biases. Yet strategic research planning, prompted by declining real research funding and increasing accountability, is a recommended if not a mandated process at a growing number of public and private institutions. The goal of strategic research program planning is to identify for focus, research issues whose pursuit will retain current levels or generate new levels of funding for the researcher (or the researchers' institution) and generate a positive rate of return to investment by the customers of the institution. It is thus essential to have some notion of what typically makes a research issue "real." I propose the notions of social relevance, targeted utility, and anticipatory direction as most critical in defining a research issue as a real issue.

A real research issue is one that is at the root of a societal problem or problems. I state this not for purely altruistic reasons. The public sector is undergoing a fundamental reassessment of the relationship between science and the public interest

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(Griffiths, 1993). The end of the Cold War has diminished the perceived need for research to strengthen national security, and the Federal budget deficit and state-level fiscal problems have led to questions about the relative returns to investment of public funds in research. Senator Barbara Mikulski has suggested that if Federal funding for research is to be retained at anywhere near current levels, there will have to be a new and compelling justification for its allocation, and greater accountability of Federally-funded scientists to the government and to the public (Mikulski, 1994). Research that does not address a well articulated social goal will not fare well under what appears to be a rapidly evolving, new science policy for the nation.

There is an identifiable, extradisciplinary audience for which the findings of research on "real" issues are expected to have utility. In other words, there is a practical use, aside from the refinement of conceptual or methodological approach, for the results of the research. This does not mean that methodological advances cannot come out of research on real research issues. However, our professional journals appear to be jam-packed with excellent economic research applied to issues for which there is no demand for analytical results. It is not evident, for example, what audience will actually use the values generated for a majority of the nonmarket goods examined in order to refine or refute the contingent valuation method (CVM), suggesting one of two things; either that researchers are choosing curiosity-driven or simply accessible subjects for CVM experimentation rather than seeking policy or legal contexts in which a particular good's nonmarket value is required, or that the professional energy going in to the CVM is disproportionately large in relation to the demand for CVM-generated results.

The final general characteristic of a real research issue is that it is anticipatory. Anticipation is a prerequisite because the typical research proposal-funding-conduct-review-publication continuum is longer than the life over which many research results will have utility, and because judgment of what constitutes a social problem changes as new, often quite foreseeable problems supersede older ones. There are a number of agroenvironmental issue areas (such as soil conservation) in which the bulk of economic research effort appears to be countercyclical to trends in public concern and policy making. An issue is "real" as a research issue only if the research findings can be expected to coincide with the posit of questions on the issue. This means looking ahead to what the issues of the future are likely to be.

Changing Paradigms for Agricultural and Environmental Issues

A number of readily observable trends give strong clues about the future's real research issues. Among those with implications for agroenvironmental research are: a rapidly changing domestic agricultural sector; a national political economy that, with reference to agricultural and environmental issues, bears less and less resemblance to past years; a globalizing economy; and unprecedented rates of world population growth.

Emerging Trends in the U.S. Agriculture Sector

A recent survey of leaders and experts in the food and agriculture system (Allen, 1993) indicates several emerging forces that suggest a need to reexamine the issues we choose to research and how we approach them. Perhaps the most pervasive, but also least studied trend is an accelerated rate of U.S. agricultural industrialization. The increasing consolidation of farms, concentration of production, and vertical coordination of the stages of the food and fiber systems are already apparent in the broiler industry and in a number of fruit, vegetable and specialty crop industries. But the tempo of industrialization is expected to quicken within livestock sectors, and its influence is apparent and growing in the grain sectors as well (See Schertz and Daft, 1993). Penn has said that "The policy structure that exists today is fundamentally out of synch with the reality of the (industrializing) agricultural sector" (C-FARE, 1994). The implication is that much of our policy-oriented research, which has focused to a large part on existing institutions and the historically predominate farm structure, is also "out of synch" with the future.

At the same time, it is predicted that "policies and regulations designed to address serious threats to environmental quality will increasingly change the nature of the agricultural production system" (Allen, 1993). This is nothing new to economists working on the agriculture-environment interface. But we still know surprisingly little about the precise nature of the change that is predicted, especially in conjunction with other growing influences on the sector. With enactment of the Coastal Zone Management Act, which specifically addresses agricultural nonpoint sources of estuarine contamination, the encroachment of pesticide reform into the context of farm legislation, an anticipated reauthorization of the Clean Water Act which targets agricultural nonpoint source pollution, and contentious debate in agricultural circles about a reauthorized Endangered Species Act all occurring simul-

taneously, and in conjunction with State and local action, it makes little sense to proceed in stepwise and partial fashion to analyze the independent effects of alternative policies under the typical *ceteris paribus* assumption. The "real" issues will be found in the interrelation of multiple, environmentally-related and other concurrent forces. Witness, as a current example, rising interest in the concept of "green payment programs" as a means of simultaneously supporting farm income, mitigating the effect of regulation on farm income, and purchasing environmental amenities.

With special reference to the Northeast, shifts in the composition of goods and services comprising the agricultural sector, such as the rise of the importance of the nursery sector, and urbanization, combined, are giving rise to a whole new set of "real" research issues. Bruce Gardner has aptly stated that, "in terms of economic theory, policy analysis, and empirical investigation, metropolitan agriculture is ripe to become one of the most exciting research areas for agricultural economists" (Gardner, 1994, p. 108).

The New Political Economy

The 1970 farm bill debates involved, almost exclusively, a coalition of 32 commodity groups and cooperatives, and the American Farm Bureau Federation as industry lobbyists, working with insular Congressional Agriculture Committees for or against an Administration proposal crafted with input from a single Executive Branch Department (Congressional Quarterly, 1971). As we look toward the 1995 farm bill, no fewer than 300 grassroots, regional, and national interest groups representing consumer, environmental, sustainable agriculture, and trade interests, as well as commodity and increasingly fractionalized farm groups, are gearing up for a debate that will center around substantially open Congressional committees (with lots of new, relatively inexperienced members of the House Agriculture Committee) and involve input from the Environmental Protection Agency and other formerly uninvolved Executive Branch agencies along with the USDA. This astounding degree of pluralism typifies the environmental regulatory process, as well. Add to it an habitual concern with budget deficit reduction and you get an agroenvironmental policy making climate that has no historical precedent. This budget-constrained, open, and pluralistic environment is important in identifying "real" research issues in that it determines, in perhaps unpredictable ways, how the "real" policy analytical issues will be framed.

International Trends

Perhaps overriding all of this are trends on the international front. With cheap, effective, global telecommunications, rapid modes of international travel, a multinational business environment, finely interconnected financial markets, and merging tastes and preferences, worldwide, the economy is becoming truly "transnational." It is thus increasingly difficult to do relevant economic research on local impacts or national policies without accounting for how they relate to the global economy.

That global economy will, without doubt, but in uncertain ways, be affected by an alarming course of human population growth. Demographers now project that the world's population will double during the next 50 years, from 5.3 billion people in 1990 to over 10 billion in 2050 (Bongaarts, 1994); an unprecedented rate of growth. Given that the majority of land suitable for agricultural production is already cultivated, most potential irrigation sources have already been tapped, and Green Revolution yield gains have flattened out (and in some places are declining), adequate food production into the middle of the next century will rely on technological innovation. But many worry that traditional technological pathways will do irreparable harm to natural resources and environmental quality, at local, national, and international levels. A range of critical research issues lies in the realm of *sustainable* development to feed 10 billion people.

Real Agroenvironmental Research Issues: What's on the Horizon?

Given my suggestions for what makes a research issue "real," and the identification of some trends apt to determine what issues will emerge as social problems and beg for relevant analysis, a number of "real" research issues can be imagined. I will outline a dozen of those I think are on the immediate horizon.

Regulatory Issues

A first category of research issues for the 21st century is the need for realistic approaches to analysis of environmental regulation affecting agriculture; approaches and methods that adapt to the changing pace and climate of regulatory decision making. To date, analyses of agroenvironmental regulatory options have largely focused on the economic efficiency of alternative policies or policy sets. Thus we have hundreds of pesticide regulatory analyses

which, in various, sometimes innovative ways, estimate the changes in producers' and consumers' surplus arising from pesticide bans and offer this, without clues as to how efficiency and risk criteria should interact in policy analysis, for political comparison with environmental and safety risks. Another example involves the seemingly endless empirical comparison, again by efficiency criterion, of voluntary versus mandatory approaches to soil conservation, which continues even as soil conservation has taken a "back burner" political position among agroenvironmental problems. Such analyses are the stuff of our professional journals. But it is very hard seeing how they can contribute to contemporary regulatory decision making. Economic efficiency criteria, which had something to offer when constituent groups were relatively homogeneous, budgets were less restrictive, and policies were more likely than now to have single objectives, are almost totally irrelevant today. What is needed, instead, are empirical studies on the political economy of environmental regulation, development of methods that can be used to rigorously compare benefits and risks of environmental regulatory actions, and research on alternative institutional arrangements for regulation in an industrialized agricultural sector.

The political economy of agroenvironmental regulation. Exactly how do agroenvironmental regulatory decisions get made in this era of multiplying interests, and what does the process imply for the feasibility and effectiveness of alternative regulatory policies? This is a relatively unexplored issue area. The political economy of general environmental policy decision making has for a fairly long time received conceptual and theoretical attention (e.g., Buchanan and Tullock, 1975). More recently, Abler and Shortle (1991) have given some clues to the suspected political viability of alternative water quality policy approaches and provided some direction for empirical study in the agroenvironmental arena. And Cropper et al. (1992) have done a fascinating *ex-post* analysis of the role of interest groups in EPA pesticide regulatory decisions. What is lacking, however, are empirical, political economy studies directed towards providing some *ex-ante* guidance on what constitutes a reasonable (rather than theoretically correct) set of agroenvironmental policies and the probability of success of alternative regulatory approaches. Specifically, how do the expected distribution of gains and losses among interest groups, and the size and strength of policy support or opposition by those groups combine through rent-seeking to create constraints on or opportunities for specific regulatory actions? And what does

this knowledge mean in terms of recommendations to regulators? Also, how do general economic conditions and trends affect the likelihood of the political success of alternative agroenvironmental regulatory proposals, and what does this suggest by way of recommended strategies for interest groups?¹ This is an exciting, open field of investigation which can be anticipatory, definitely has audiences for which findings would be useful, and addresses a set of current social problems from a more holistic perspective than do typical, welfare analyses of policy options.

Benefit-risk assessment methodology. Taylor (1992) has wisely suggested that economists redirect their efforts in pesticide assessment away from ever more precise measures of welfare adjustments in agricultural markets, toward "new theoretical models that might . . . provide a framework or method that will assist pesticide policy makers with weighing risks and benefits." I couldn't agree more, but I would extend that advice to a whole range of social problems that are likely to receive regulatory attention in the future. Wetlands preservation, endangered species regulation, controlling the entry of harmful non-indigenous species, other food and agricultural quarantine policies, applications of food and agricultural biotechnology, and a slew of food safety concerns are among the problems whose regulation requires a comparison of economic consequences against environmental or health risks. Benefits and risks are now weighed in an ad hoc fashion (which could be illuminated by political economic investigation) by regulators who, I will postulate, might welcome a more systematic approach as guidance to their difficult decision making process.² Of special social merit would be the development of techniques which would allow for clear distinction between science-based, probabilistic rankings of risk, and social valuation and orderings of risk. Methods for understanding, eliciting, and incorporating into regulatory decision frameworks the social values of risks could do much to reconcile, conceptually, the demand for reduction in involuntary risk with the scientific claims that certain risks are already too low to justify public concern. This reconciliation may, in turn, improve policy decision making and evaluation regarding the regulation of sources of

¹ See Pinstrup-Andersen (1993) for a particularly convincing argument for additional research on the political economy of food and nutrition policies. Most of his well articulated points are equally applicable to the agroenvironmental policy arena.

² If a rigorous and systematic method for weighing benefits and risks makes the decision process more transparent, it may, in fact, be disfavored by some regulators but would have great utility to the interest groups influencing and/or affected by regulatory decisions.

risk. As Krupnick et al. (1993) point out, ignoring the differences between scientific risk and the public's different but "rational and replicable assessments about risks, . . . means ignoring the economic consequences of such differences."

Environmental regulation of an industrialized agriculture sector. Judging from the agricultural and resource economics literature, our evaluations of agroenvironmental regulatory options consider incentives structures aimed almost exclusively at farmers. With the obvious rise of vertical coordination in the food and fiber system, and the consequential loss of personal decision making flexibility experienced by farmers who produce under contract or in integrated cooperatives, this farmer-focus seems more and more irrelevant. I am not aware of anyone who is currently examining the agroenvironmental implications of industrialization, the conceptual differences between regulation in a decentralized versus concentrated food and fiber industry, or alternative institutional arrangements for environmental regulation in an industrialized sector. Will continued industrialization make punitive regulation more likely as agricultural pollution takes on characteristics resembling point sources and the public sees contractors as less worthy of supportive regulation than independent farmers? What role(s) can contractors (like Tyson's) or cooperatives (like Ocean Spray) play in the agroenvironmental regulatory arena? How does industrialization affect the distribution of regulatory costs? Are market-based policy options (e.g., pollution permit trading) more feasible with a more limited and concentrated set of principal actors? These are very timely researchable questions which should only gain in importance into the 21st century.

Issues in the Context of a Globalized Economy

As the economy increasingly assumes a transnational character, the concept of "local" impact takes on an entirely new meaning. The inextricable linkage of local economic conditions, trends, and events with global economic activity suggests that we economists must take global factors into explicit consideration in any address of "real" research issues at the local level. Similarly, research issues surrounding the establishment of international standards, multilateral agreements, and other international compacts will be most "real" when they address local, rather than simply aggregate implications of alternative provisions. This interconnectedness spawns a host of real research issues for the coming century.

Agricultural trade and the environment. There

has been no lack of attention to the environmental implications of freer international trade in recent years. The bulk of economic studies have focused on the conceptual relationship between national or international environmental quality and trade liberalization, generally (e.g., Anderson, 1992), or on the conceptual expectations for environmental quality in the context of specific GATT or NAFTA trade agreements (e.g., Abler and Pick, 1993; Harold and Runge, 1993). However, as Antle (1993) has demonstrated, and Ervin (1993) nicely amplifies, local environmental impacts of trade liberalization can easily be either positive or negative but the disaggregated data requirements for empirical investigation of actual (vis-a-vis conceptual) effects are almost overwhelming. Addressing whether free trade, conceptually or in the context of particular multilateral trade agreements, improves or degrades the environment is useful and compelling. But, I doubt that it will be as "real" a research issue in coming years as will issues around specific environmentally based disputes over provisions of trade agreements, or resultant moves for explicit incorporation of environmental provisions in (as opposed to side agreements to) trade agreements.

There is, among policy circles, a remarkable complacency about the current GATT agreement's exemption of "green" domestic programs from consideration as sector-supportive, trade-distorting subsidies. Admittedly, the text of the Uruguay Round agreements appears amenable to just about any environmentally motivated subsidy program, but it is very vague. Details have yet to be worked out, and they will likely be worked out in the context of challenges by one country to another. For example, if an American "green payment" scheme replaces commodity program payments with larger payments for actions that protect environmental quality in some way, but does not substantially alter the current distribution of government payments (as a possible prerequisite for political support), I think it is entirely reasonable to expect a rival exporter to accuse the U.S. of retaining trade distorting subsidies simply by changing the label on those subsidies. Other challengeable, domestic "green" programs might include those which would pay farmers in one country to retain amenities (such as various landscape features) that are valued differently in other GATT countries. The anticipatory economist will be gearing up for such challenges now. Research contributions will be needed on such things as the now nonexistent specific criteria that will distinguish arbitrary trade-distorting from environment-enhancing domestic programs under the legal strict-

tures of multilateral trade agreements, and techniques to measure the social costs and benefits of alternative criteria for deciding disputes.

Ultimately, pressure for addressing environmental costs in future GATT rounds or in new international trade agreements will demand a lot of innovative thinking by agroenvironmental economists on a range of real research issues. If, as Paden (1994) suggests, "well-constructed" free trade agreements will mean that the price of a good traded on an international market fairly reflects the total (private *plus* social) costs of its production, empirical research on intercountry differences in nonmarket valuation, and what those differences mean, will be a "real" necessity.

Social valuation in the context of international harmonization of health and environmental regulations. A closely related "real" research issue is the role of sociocultural values in the designation of international standards for food manufacturing and safety (i.e., the Codex Alimentarius Commission of the United Nations), phytosanitary and animal health measures, and for what human health, food safety, and environmental risk-motivated product bans constitute non-tariff barriers to trade. As in the case of risk assessment for domestic policy decision making, the "correct" basis for international standards is seen by most "hard" scientists as internationally irrefutable measures based on the best available scientific information. (We are not talking "social sciences" here.) What this strictly science-based approach requires, however, is that local or national values about the safety of internationally traded commodities (such as the European Community's historical fear of hormone-treated meats) or the practices involved in traded commodities' production (such as dolphin safe tuna) are entirely superseded by "science." I see an important role for economists who understand nonmarket values to act as technical arbiters in the increasingly heated debate between those who favor quantitative science and those who appeal to moral arguments as bases for setting standards across countries whose populations value things differently. The "real" research issue surrounds the development and testing of methods which might combine consideration of sociocultural and scientific values in international policy harmonization.

Exploring the Recreation-Tourism-Agriculture Interface

We know pitifully little about how the nonagricultural amenities provided by various kinds of agricultural activities are valued by individuals or con-

tribute to economic development. In the Northeast, especially, the relationships between farming, recreation and tourism are likely to become increasingly important. At least three sorts of real research issues can be identified as potential contributors to our understanding of these relationships.

Valuing agricultural landscape amenities. Americans' affection for many agricultural landscapes, like Vermont's rolling green hills dotted with cows and sheep, is probably manifested in an assortment of pecuniary and nonmarket values, including option and existence values. These various landscape-related values could be capitalized upon for economic development if there was a better understanding of how different characteristics of the landscape (e.g., hilliness, public access, expanse) and its associated agricultural activity (e.g., presence of livestock, condition of farm buildings) affect the sum and relative distribution of market and nonmarket values associated with it, and how differently valued attributes relate to one another. Armed with this information, institutional arrangements may then be explored or developed to preserve, conserve, or market those amenities. The United Kingdom's Countryside Commission recently conducted a survey which showed that "the countryside is a vital component of the quality of life for more than six in every ten people in England" (Countryside Commission, 1994). While I readily admit that the British probably value the "countryside" differently than do Americans, we have much to gain by exploring the applicability of such British mechanisms as walking path systems (which go through multiple private properties) to American economic development through recreation and tourism in rural pockets of the densely populated Northeast.

Effect of resource/environmental quality on recreation/tourism. The existence of or potential for a rural-based tourism industry in the Northeast depends, in some way that I don't believe has been estimated, on the quality of environmental amenities valued by tourists. I refer specifically to the combination of and interactions between environmental goods in the creation of a bundle of goods that fosters tourism. We may very well know how the recreational value of a fishing stream varies as the stream's water quality varies. I think we are less likely to know the nature of complementarities or tradeoffs among stream quality, air quality, landscape amenities, and service industries in the demand function for tourism. As the tourist industry becomes more important to the Northeastern economy, this is a "real" research issue. In a broader context, empirical estimation of the relationship between job creation and economic devel-

opment, and environmental and resource quality, in general, is a major research issue for the future. (See Templet and Farber, 1994, for an interesting empirical example.)

Farms as tourist attractions. The distribution of farms under an industrialized agriculture is expected to be bimodal, with large, consolidated and integrated firms comprising a commercial subsector, and a set of smaller, nonindustrialized farms dependent upon off-farm income or diversified enterprises for financial survival. The smaller farms, many of which are and will be located in the Northeast, will be searching for options to diversify. Judging from the popular farm press, there is a growing interest among farmers in the possibility of mixing agricultural production enterprises with services related to recreational, tourist, and educational opportunities. Farm-based hunting, "bed and breakfast" operations, experiential "adventures" where city folk pay for the opportunity to help in haying or other farm labors, and for-fee educational services which introduce school children to agriculture and the farming experience are among those business opportunities I have seen mentioned as opportunities for smaller-scale farm operations. Assessing the demand for such extracurricular services, the viability of associated businesses, and the appropriate mix of agricultural and nonagricultural on-farm enterprises might, then, constitute a set of "real" and interesting research issues for address by teams of farm management, rural development, and resource economists.

Research on the Formation of Preferences

Henry Aaron (1994) makes a compelling case for the inability of standard economic theory and methods, specifically those based on the economists' notion of utility, to address major social issues. He provides a long list of major social concerns (one of my prerequisites for "real" research issues) that cannot be addressed by conventional economic theory because the behaviors that cause them or the values reflected by public response to them are anomalies within the context of the disciplinary framework. He cites a reference to some economists who, upon finding that their subjects failed to behave according to the axioms of expected utility, concluded that the subjects were irrational and had "intellectual shortcomings" (Aaron, 1994, p. 19).

This problem concerning what is actually a theoretical shortcoming has significant implications for many of the "real" research issues I have covered thus far as most of them require some measure-

ment of how groups value a nonmarket good or attribute, like risk. Conventional economics suggests that such valuation take place within the construct of utility theory. But, utility functions require the assumption that preferences are stable, egoistic, rational (in that they obey the laws of transitivity), and cover all states of the world. Clearly, these assumptions do not hold unambivalently as we note that people value a particular recreational opportunity only under certain states of the world, exhibit irrational (nontransitive) orderings of risk preferences, and are subject to changes in preferences as a result of public policies. We will be much more effective in providing valuations for use in policy decision making if we can explain more effectively, and incorporate into analyses, the mechanisms by which preferences are formed, how they vary under different circumstances, and how they change over time. Some theoretical work, well beyond the question of which utility function is best for contingent valuation, is needed to achieve this.

Sustainable Development

Whether or not the dramatic, near-term rise in world population leads to a food crisis, a reemergence of widespread social concern about the world food situation seems inevitable, and, I think, has already begun. This new period of concern will be very unlike that of the 1960's and 1970's, where focus was fairly unilaterally on the need to increase food crop yields. Now, the recognized need to increase global food production is tempered by concerns about the sustainability of the natural resource base and human systems within which production gains are to be achieved. The agricultural research system must be prepared to identify, investigate, and develop technologies that retain or enhance resource and environmental quality while simultaneously increasing yields. There are several roles for economic research in this endeavor.

Social accounting. With no disrespect at all for those innovative economists who have estimated rates of returns to investment in "green revolution" research, I contend that one of the reasons that the direction of agricultural R&D has not shifted significantly towards the joint achievement of environmental and production gains is that conventional means for measuring productivity, returns to research investment, and national income overestimate the contribution of strictly yield-enhancing technologies by failing to account for social costs. If research institutions and national

governments are to make well-informed decisions about research priorities under global population stress on food and resources, they will need measurement tools that incorporate the value of non-market benefits and costs along with pecuniary benefits and costs. Linking social and economic indicators in national accounting systems is not a new idea (see Fox, 1974). But empirical examples (see Ahmed et al., 1989; Repetto et al., 1989) in which the contribution of income flows and the contribution of market and nonmarket resource stocks, valued according to quality, are combined to suggest sustainable development paths are scarcer than one might expect.

Technical innovation in a policy context. Measuring the actual or expected contributions of different agricultural development and technological pathways will be of little use in resolving the sustainable development problem if effective policies cannot be devised to encourage shifts towards sustainable pathways. A "real" research issue for policy analysts lies in the development and evaluation of alternative mechanisms for inducing a shift in R&D towards technologies which jointly achieve environmental and production goals. The world would be well served by a bright, innovative economist who would probe and apply the theory of induced innovation to identify practical policies and institutional arrangements to address what I expect will be vociferous demand for very new kinds of agricultural technologies.

Ex-ante technology assessment. Many of the social costs of current agricultural technologies could easily have been foreseen, valued, and weighed against the market benefits of the technology well before developmental work led to commercialization. If social scientists were more active in the preassessment of technology types, bundles, or characteristics, the broader agricultural R&D community would have access to information allowing socially beneficial research program planning and mid-course corrections in technologies' development and introduction. While the public agricultural research community can benefit from such information by assuring that research directions reflect public priorities (and thus receive public support), the private sector has as much or more to gain. Preassessments which indicate that commercialization of a particular class of technologies may meet with public resistance can save companies the commercialization, legal and public relations costs that would come from those technologies' development for introduction into the market (or indicate that those companies must devise certain marketing strategies for introduced technologies that carry social costs with them).

Conclusions and Implications for the Profession

The list of a dozen "real" research issues I have reviewed here is not meant to be comprehensive. I am sure there are many more that meet my simple criteria for being "real" research issues. They, like most of those I have identified, will be highly complex, because they recognize the complexity of contemporary social problems and incorporate the interconnections inherent in a globalized economy. They will also be issues that require new theoretical approaches, new ways of applying existing models and methods, or whole new ways of thinking, because the world to which we address our research findings has, in several respects, outgrown many of our old, familiar ways of going about our research business.

First, relevant social problems have become too multi-faceted to be addressed in useful context by any one discipline. Thus, agricultural and environmental economists who are successful in tackling the real research issues of the future will increasingly address those issues in an interdisciplinary context. This might include, for example, working with psychologists on risk perceptions and preference formation; with sociologists on technology assessment for sustainable development; with political scientists on the new political economy; or with regional planners on agricultural tourism.

Second, the utility aspect of the criteria for "real" research issues suggests increased focus on the development, evaluation, and application of economic measuring devices. For example, measures of "sustainability" are necessary to illuminate for decision makers, who are not necessarily economists, the expected outcomes of different technological pathways. Acceptable expressions of the benefits vis-a-vis the risks of technologies and policies would resolve not only some problems of regulators, but also those of a public which relies on wise and fair regulation to reflect its preferences. Definitions and/or measures of "trade distortion," and a method for adjusting scientific measures of risk to reflect social or "lay" interpretations of risk, are other examples of indicators which might usefully arise from developmental research on real issues of the future.

Finally, the combination of high decision stakes and high systems uncertainties in contemporary social problems suggests the need for research approaches that readily and usefully incorporate inherently qualitative information. Most economists will readily acknowledge that the output of their research provides only one set of factors for consideration in decisions regarding real social issues.

Far fewer are working on or with alternative models for organizing and using economic information along with nonmonetary values, ecological and other scientific information, political variables, and, even, moral and ethical considerations in resolving contemporary problems. Economists' involvement in the evolution of what Funtowicz and Ravetz (1990) call "post-normal science" will be critical if we are to be successful in addressing the "real" research issues of the coming century.

What I have prescribed here—more interdisciplinary research, increased focus on development of indicators and measurement devices, and theoretical and methodological efforts that reach well beyond neoclassical economic theory—is not the currently popularized recipe for professional success in agricultural and resource economics. However, as research funding becomes relatively scarcer and post-Cold War, national science policy continues to shift towards the address of public interests beyond national security, these characteristics may be important elements in the success of future professionals. The challenge we face now is in assuring that professional rewards, social relevance, and practical utility can be simultaneously achieved in setting and pursuing agroenvironmental economics research agendas for the future.

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