A CENTURY OF SCIENCE IN AGRICULTURE: LESSONS FOR SCIENCE POLICY

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". . . unapplied knowledge is knowledge shorn of its meaning."

Alfred North Whitehead

Today all of science seems to be in some political and policy difficulty. There is rising conflict over the funding for and the performance of science. In the agricultural sciences there has been a crescendo of external criticism in Congress and elsewhere ever since the National Academy of Sciences "Pound Report" in 1972 (NAS). Repeated criticisms from the national science establishment suggest that agricultural science lacks a basic science foundation and is a third-rate enterprise. Their usual prescription for this problem is quite simplistic: eliminate all the "politically allocated" Hatch-type formula funding substituting for it peer-reviewed, competitive grants -- open to researchers anywhere, not just in colleges of agriculture.

Various advocacy groups, the media and some politicians are also highly critical of the agricultural sciences. They focus on such dangers as uncontrolled new genetic technologies, and the threats to health, safety and the environment of other agricultural technologies. The public attitude toward science has shifted from unqualified support to a questioning ambivalence and even fear of its consequences.

At the same time some state legislatures perceive their land grant college to have abandoned the land grant mission and agricultural problem solving for the glories of basic science. These land grant colleges of agriculture are in difficulty with their clientele and legislatures. Still other colleges of agriculture have become so applied and isolated from many of the basic disciplines that they are losing scientific and intellectual vitality. After resisting the idea for over a decade, agricultural science now shows some sign of understanding it must adjust its mission and adapt its institutions to a society and an agriculture greatly different from that of 50 or even 25 years ago.

It would appear that the national science establishment is also slowly beginning to understand that it too faces some fundamental questions. Since World War II public sector national science policy, except in medicine and agriculture, has been focused only on the basic disciplines. Applied science and technology, when considered, is treated separately as primarily a private sector matter of industrial R&D plus a few federally funded R&D centers. Dissatisfaction with this policy posture is growing, especially in industry and politics (David; Shapley and Roy; Norman). The current administration and its recent science advisor, Dr. Keyworth, have argued, as others have before, that U.S. science policy should be directed to help achieve greater economic competitiveness (Keyworth; Press). This would require that science policy place more emphasis on mission oriented, applied research, and on its coordination with disciplinary research.

Science policy then involves not just disciplinary funding priorities but what kinds of science research (disciplinary and applied) should be funded; how such a diverse scientific enterprise should be institutionalized, funded and managed; what role the private sector should play and; indeed, what philosophic values should inform the priority setting process. The debate, however, is poorly informed and inflamed by parochial ideologies and self interest -- in science and out (Johnson 1984). Many scientists still believe there is only one problem -- inadequate funding.

First, I will examine some of the lessons I believe we should have learned from a century of science in agriculture. I will then attempt to extract from those lessons some implications for science policy today.

My remarks reflect two theses. First, the future competitiveness of the U.S. economy will depend more on the performance of science than in the past. But national science policy is not currently well adapted to serve society in improving its competitiveness (Press). Second, the experience of agricultural science holds important lessons not only for agricultural science, but for national science policy and the institutional design and management of science. However, as Schuh has pointed out, the value structures and behavior pursued today in many land grant universities and their colleges of agriculture suggest that the land grant idea is being abandoned. We have little institutional understanding of our own historical experience, face different demands and are not currently as capable of sustaining our traditional mission as once we were. If we do not understand our own past, we cannot learn from experience nor explain its meaning to others.

I believe the problem in both agricultural and overall science policy in the U.S. arises out of their successes. The accomplishments of science have changed both science and society. The consequence is that both are very different and more complex today. This results in demands on science generally to expand its scope into a much more complex role -- to something different from but more like the land grant research mission. The specific pressures on agricultural science suggest a role more like that of science generally, since a rapidly growing private sector is taking over some basic science but much more of the applied R&D and extension functions. It also appears that responsibility for coordination of agricultural science policy is shifting from a predominantly public function to more of a shared public and private responsibility, making both policy and its coordination more complex.

The Lessons to be Learned

I have argued elsewhere that what evolved in U.S. agriculture was an articulated system of science-based developmental institutions (Bonnen 1983, 1986, 1987). There are lessons to be learned from a century of experience in these institutions that speak to the problems that we face today in general science policy as well as agricultural science policy. We in agriculture need to understand these lessons, act on them and explain them to our colleagues in science.

Lesson 1: Greater Societal Capacity Comes From Four Prime Movers

Science research is not the only source of increased productivity. The experience (and the literature) in international agricultural development leads one to the conclusion that the primary sources of increased societal capacity include not only technological change (only some of which is science based) but institutional improvements, increases in human capability (human capital formation) and the growth of biological and physical capital.[1] While all four of these prime movers are considered in the development literature and in our research efforts, there is a frequent lack of balance in their treatment (Johnson 1986).

Many biological and physical scientists and some economists focus on technological advance to the neglect of the other forces. Indeed, our society tends to exhibit a disordering "technological fixation." The development process is a search for the appropriate balance in complementary investments needed in all four. That balance is determined in good part by the nature and limitations of the specific social, biological and physical environment within which development occurs.

There is as well the matter of the values we attach to the human purposes in which the four prime movers are used. Substantial changes in values (monetary and nonmonetary) or in a value's perceived importance transform the capacities of society and modify the mix of its activities as it changes their relative productivity and impacts on human welfare. An example is the clear change in valuation that has occurred with respect to environmental degradation and the many other negative external effects of agricultural production technologies.

Lesson 2: The Continuum of Knowledge and the Research Process

Thoughtful examination of the application of scientific knowledge to human problem solving exposes a continuum of institutions and processes involving several categories of knowledge they help to create and manage. We need to understand this continuum of knowledge.

The term basic research I take to refer to disciplinary knowledge, which is the theory, empirical measurements and measurement techniques, and methods used to explain the fundamental class of phenomena of concern in a discipline such as physics, botany, economics or philosophy. This knowledge improves the capacity of a discipline. It includes research on values in the social science disciplines and humanities.

An applied, multidisciplinary mode of inquiry produces subject matter and problem-solving knowledge. Subject-matter knowledge is multidisciplinary knowledge useful to set of decision makers facing a common set of problems. This knowledge is organized under such labels as biotechnology, animal nutrition, agronomy, marketing or farm management. Most departments in colleges of agriculture are more like broad multidisciplinary, subject-matter institutes (e.g. agronomy, animal husbandry, agricultural economics, horticulture, agricultural engineering) than the disciplines of traditional colleges and universities. Similarly, professional schools and institutes are also typically multidisciplinary, subject-matter organizations. These units organize inquiry and knowledge from different disciplines needed to understand a subject. Their knowledge bases are necessary to support systematic and sustained problem solving.

Rarely can one go directly from subject-matter knowledge to a decision. Before even multidisciplinary, subject-matter knowledge has direct relevance to a specific problem, it must usually be fashioned into multidisciplinary, problem-solving knowledge -- i.e. into a form that is relevant to a single decision maker with a specific problem (or set of decision makers, all with one specific problem). Problem-solving knowledge comes in prescriptions -- i. e. "should" or "ought" statements for which knowledge of values is essential.[2]

Production of these three types of knowledge requires a complex research process. Disciplinary knowledge either does or does not have some known relevance when it is created. If that without known relevance is to have meaning or value someone must devote themselves to research developing the implications of the new disciplinary knowledge. After its potential becomes clear, one can begin to think about specific uses for it. That is, what kinds of technologies, institutions and human capital are appropriate and useful to develop out of physical, biological and social science and humanistic disciplinary knowledge?

After developing a new biological technology (or a new institution), one must face the problem of making that new technology (or institution) work in thousands of ecosystems (and social systems) across the United States. This requires research in every state to adapt its potential of productivity to each state's unique, diverse environments. When first developed, hybrid corn production was limited to five cornbelt states. It took 20 years of adaptive research in all the states before it could be grown commercially across the entire United States and before its potential yields were fully realized, even in the corn belt. The location-specific character of agriculture makes adaptive research a central feature of agricultural research programs.

Creating an increase in productive capacity disturbs the ecosystems into which it is injected. New host populations and ecological niches are created making it possible for pests and diseases to attack and destroy new productivity. Thus maintenance research across multiple ecosystems is necessary in perpetuity to defend the productivity that has been created. As scientific knowledge increases and with it agricultural productivity, the proportion of total R&D investment going to maintenance research must inevitably grow (Ruttan p. 60). Livestock is less location specific and thus requires less adaptive and maintenance research than crops do. Given the location-specific nature of agriculture, a geographically dispersed institutional system with diverse ecosystem-specific capability is required to sustain adaptive and maintenance research as well as technology transfer activities. All of this seems fairly obvious but I find many scientists, including some in agriculture, who do not understand that nature imposes these varied activities on agricultural science. This makes unique demands on agricultural science policy.

Another form of maintenance research is increasing important. Various external effects of agricultural technology are now creating a need for assessment of new technologies and institutional innovations. As we engage in R&D efforts we need to understand the potential effects of an incipient technology. Will it undermine rural communities, degrade the environment or poison the food chain? What positive and negative values do we place on the impacts of an institutional adaption to the new technology? Who gains, who loses? Assessment research is needed to guide the research process and to legitimize and protect its integrity. Like problem solving, assessment involves research on values.

The agenda of issues in agriculture today strongly suggests that more social science and humanities knowledge is needed. This is due to growing needs for the modification of old institutions or development of new institutions, the adaptation and transfer of technologies, for resolution of ethical problems, and in the creation of new human capital. The mix of relevant disciplines varies with the problems addressed. When the agenda of problems changes as drastically as it has in the 1980s, so does the appropriate mix of disciplines. Implementing the right mix is imperative to future public support of agricultural science. The growing complexity of the industry, and thus the chance of error, as well as the fact that science is increasingly expected to minimize deleterious impacts on society means that we may not ignor current criticisms and expect continued and adequate public funding. The need for social science and humanistic research on agriculture and rural life is growing.

Lesson 3: A System of Interactive, Coordinated Linkages

It has been found necessary in agriculture for the continuum of knowledge, from creation to use, to be not only institutionalized but coordinated and focused on problems. The literature on returns to investment in U.S. agricultural R&D, education and extension/technology transfer demonstrates that a large part of the productivity achieved in agriculture arises out of the interactive linkage coordinating these various investments and the institutions that manage them, rather than flowing directly from separate investments in the four prime movers themselves (Evenson et al; Ruttan; Johnson 1986). This is because the four prime movers are complements in production. Each is necessary but not sufficient to achieve an optimum output.

The same conclusions can be drawn about investments in the different kinds of research. Investment in disciplinary research, although absolutely necessary, is not sufficient to achieve high levels of capacity and productivity. The full potential of productivity from disciplinary research is not realized until it is complemented with investments in multidisciplinary, subject-matter and problem-solving research. The reverse is also true; without a continuing basic science investment the productivity and economic return to applied science will decline.

The same principle applies to many elements that affect productive capacity: they are often individually necessary but not sufficient. It is the investment in all relevant complementary factors plus systematic coordination of decisions about the combinations and timing of the various factors influencing productivity that is most important. Failure to link together in the same goal-driven system the public and private decisions about investments in disciplinary research, various types of applied research and technology development, the development or modification of institutions, extension, education and other human capital and conventional capital slows the achievement and reduces the level of productive capacity that can be extracted from a given investment in agriculture. When an investment is potentially profitable, productivity deferred is in some part lost forever (Knutson and Tweeten). The near exclusive focus on disciplinary research in United States science policy has led to a disconnection with subject-matter and problem-solving research that is a drag on productivity.

For a set of institutions to be a developmental system, the individual institutions and functions must be interlinked or articulated, so that they communicate and cooperate in action to achieve some common goals. Interlinkage and coordination speed interactions and the setting and achievement of goals. This linkage is iterative and interactive and is a major source of the system's adaptive capability. Successful systems of science-based development are not conceived or planned as a whole and then put into place. No one knows enough to do that successfully. Scientific inquiry, policy and institution building decisions are all made under great uncertainty, with imperfect knowledge. Many failures and mistakes occur. Thus, institutional behavior and science must be iterative and interactive in their mode of both inquiry and action to sustain the learning necessary to maintain adaptive capability. Substantial adaptive capacity is necessary not only to deal with uncertainty and mistakes but also with the tension and conflict between institutions and multiple goals within the system.

Lesson 4: Decentralized Decision Capacity

Another characteristic of the U.S. agricultural research system closely related to its interactive nature is decentralization. While it is a national system, authority is not concentrated solely at the national level. The conditions of agricultural production are highly varied and location specific. Decentralization is necessary for successful adaptation of science knowledge and technology to the many varied, local ecospheres that characterize agricultural production. There are, in addition, all sorts of local political, cultural and social traditions that make it necessary to accommodate institutional structures to local polities and resources to ensure a legitimized and coordinated system.

Lesson 5: System Decisions are Consensual

It follows from its decentralized nature that policy decisions affecting all or large parts of the U.S. system must be developed by consensus, if they are to be accepted as legitimate and implemented effectively. The goals and major initiatives of the system historically have evolved out of a debate of issues ending in a bargained consensus. Unilateral power plays to achieve something that substantially affects the whole system generally create excessive conflict, reduces cooperation and interlinkage and ends in system failure.

Lesson 6: The Agendas of Science and of Agriculture are Integrated

The institutions of agriculture combine and manage in a single system societal problem solving and the pursuit of the agenda of science. The pragmatism and political expediency necessary to sustain effective societal problem solving involves organization, values and expectations that are in some degree inconsistent and, in the same system, in perpetual tension with those of science, especially the goal of knowledge for its own sake. Much of the societal support as well as the productivity generated by agricultural science has arisen out of the sustained interlinkage of these functions and the management of the resulting tensions to maintain a working balance between the agenda and capacities of science and the agenda of problems in agriculture. Effective science-based problem solving requires coordination and integration of science with any economic sector in which science is expected to drive major increases in productivity.

Lesson 7: Chronic Underfunding and Spillover are Endemic

What have we learned about funding agricultural science? Measures of the annual rate of return on public investments in agricultural research run 3 to 5 times the rates on most alternative public investments (Ruttan p. 248). While the causes are not fully understood, this suggests that agricultural R&D is substantially underfunded by normal investment criteria (Oehmke). Today one must deal with the counter argument that the world's markets are awash with the products of excess agricultural capacity because of science research. This is pure nonsense. The creation of this excess capacity is the consequence of mistaken investment decisions by farmers based on excessively optimistic expectations, induced in part by subsidies and foolish national policies (Johnson 1985). These same expectations were subsequently destroyed by the growth in world supplies, increased industrial nation subsidies of their agricultures and a U.S. macroeconomic policy that has destroyed U.S. agriculture's export potential while escalating its costs and leaving it in the deepest financial crisis since the Great Depression.

We are in a contest for international markets which depends in part on a healthy agricultural research enterprise, including social science research on policy, markets, other institutions, human capital formation and our capital and natural resource bases, to maintain comparative advantage in production costs and market access. The argument that R&D causes surpluses arises out of the equally fallacious reverse argument of scientists who urge increased biophysical R&D to solve world hunger, when hunger is with few exceptions due to inadequate income or its maldistribution (Johnson 1985).

Today, state appropriations greatly exceed federal funding for the state experiment stations (OTA). In real terms federal funding of the state system all but ceased to grow after 1967. This raises serious questions about the federal commitment to its historic partnership with the states in agricultural research. The issue is fundamental to the long term performance of the system since a large part of the benefits of research funded by one state spillover into other states. The empirical evidence on spillover of the benefits from research financed by one state accruing to farmers and consumers in other states is strong (Havlicek and White). While it varies greatly from state to state, typical spillover losses range from one-half to two-thirds on basic science and one-third to one-half of technology oriented investments (Evenson et al.). Losses of state level benefits deter state investment in agricultural science. Without compensating federal funding, the states, acting alone and rationally, will never achieve an optimum level of national investment in agricultural research. This is the classic public finance problem faced by federal systems with two or more levels of government.

Ruttan argues that the primary rationale today for federal support of state agricultural research is to compensate the states for spillover -- in order to achieve a socially optimum national rate of research investment (pp. 251-59). Thus, each state's share of federal support for agricultural research should at minimum approximate their spillover losses. This suggests that the federal government should be matching state funding on an open-ended basis rather than the reverse. The only other way to achieve the optimum level of investment is to fund all agricultural R&D at the federal level.

Despite some secular decline in spillover rates, its continued existence means that the private sector, although large and growing, cannot be expected to perform all of the applied or basic research necessary to reach an optimum level of investment in agricultural science (Evenson et al).

Spillover also occurs internationally. The benefits of European and U.S. agricultural research have flowed to both developed and developing countries over many decades. The rising perception of this today in the midst of a ferocious international battle for export markets gives rise to attempts, such as that of the U.S. soybean producers, to eliminate U.S. international aid in agriculture in a futile effort to monopolize U.S. agricultural research results. The only real long run hope for increased demand for U.S. farm exports lies in higher incomes in developing countries due to development. In addition, agricultural science is an international enterprise today. We are about as likely to benefit from research elsewhere, as the reverse. Withdrawing from international cooperation in agriculture would be suicidal for our own productivity and export markets.

Lesson 8: Stability of Funding

The research on the optimum rate of investment in agricultural science demonstrates substantial losses of productivity when the funding of R&D is highly variable from year to year and decade to decade, -- i.e., when we are unable to sustain the pursuit of the inherent long term goals of R&D and development (Knutson and Tweeten; White and Havlicek). Disciplinary research, much of technology development and major adaptive research would appear to be especially vulnerable. This principle applies to R&D generally, although the characteristics of aggregate demand and supply response accentuate the problem in agriculture. Thus, this is a race that goes to the tortoise not the hare -- i.e., to sustained long term, institutional support of agricultural research, not the jerking around that has been imposed on the agricultural system nationally for almost two decades through inflation, stagnant and uncertain appropriations, inconsistent goals, political abuse and inattention.

Implications of These Lessons for Policy in a Changing World

What then are the implications of these lessons for science policy today? In assessing this we must recognize that both science and society have changed since the system of agricultural science institutions matured.

The Search for a National Science Policy

The Funding Debate. The conflict over research funding is a debate of the deaf. The national science establishment argues that only competitive grants can be used effectively to allocate resources for science purposes; any other approach produces poor quality science. The agricultural science establishment has responded by defending Hatch or formula funding as essential for science in agriculture. Some agricultural leaders clearly fear the effects of exclusive use of competitive grants on the stability and long term vitality of their institutions. Competitive grants, of course, are now a small but regular part of the federal funding of agricultural science. The funding argument tends to be put in either/or terms. If one is good the other has to be bad for science. This obscures the real funding problem.

Formula or institutional funding was originally established to induce development of a decentralized state system of agricultural science. It has been responsive to the need for sustaining the large fixed or overhead costs of science and the mission oriented, largely applied nature of experiment station research. As Ruttan points out, the great productivity achieved by this system of institutional support places the burden of proof on those who would change it (p. 231). The cost of entrepreneuring and managing formula funding falls on the administrators in the agricultural research system, not on the researchers (Bredahl et al). The quality of science produced depends not on the funding process but on the quality of individual scientists hired; how supportive the research institutions and their incentive structures and academic freedom are of creativity; and the quality and amount of administrative support especially for the more administratively demanding multidisciplinary, subject-matter and problem-solving research (Berry; Johnson forthcoming, chapters 13-16).

Peer reviewed, competitive grants are a centralized system that is reasonably well adapted to allocating disciplinary research resources. The cost of entrepreneuring and managing competitive grants falls mostly on the researcher (Bredahl et al). This is why you find senior scientists who no longer have time for anything but developing grant proposals and managing a laboratory. This cannot be the best use of a creative scientist's time. Short-run, project-by-project grant proposals do not add up into coherent long-term research programs necessary in much applied research and technology development. Competitive grants often do not cover the total costs of research (Ruttan, chapter 9). Consequently, the current battle over the efforts of the Office of Management and Budget (OMB) to reduce the overhead rates universities receive on research grants is extremely heated (Werner). OMB believes that some universities are abusing the system.

Experiments have been done on the performance of peer review panels. Panels composed of different scientists with comparable background and ability, produce quite different rank orderings of the same set of project proposals (Cole et al 1981; Lyon). While there is an unavoidable subjective element in the awarding of peer reviewed, competitive grants, the experiments do not suggest any systematic bias (Cole et al 1978; Lloyd). The recent flurry of attempts to use political power to go around the peer review system in obtaining federal research support nevertheless appears to be based on the belief that an "old-boy system" does dominate peer review and rewards the long established and discriminates against smaller and less well established institutions (Norman, Norman and Marshall, Greenberg). I do not believe this is true but the perception has become a growing problem in legitimizing science budgets.

Neither allocative device is perfect. They work reasonably well for some purposes but not for all. The real problem, which tends to be obscured, is that science and its purposes have become so complex that research funding requires some integrated mixture of funding devices including, but not limited to, formula or institutional funding and competitive grants. The type of research and purpose should control the mix of allocation devices. The purpose and criteria for funding, evaluation, administration and conduct of disciplinary research differs from subject matter research, which in turn differs from problem solving research (Johnson forthcoming, ch. 13-16). The appropriate peers in evaluating problem solving research, for example, are quite different from those for disciplinary research.

New ways of funding science are needed to deal with the increasing complexity of science and the growing demands of society. We need to stop our senseless arguments and look at science, its multiple purposes (public and private) and examine pragmatically the ways in which we might best fund different types of research. First, however we must agree on the role of science in society. Without clarity of purpose, very little else can be decided.

The Scope of U.S. Science Policy. The overall science establishment needs to take a more comprehensive view of science and its role in society. It functions today like a special interest pleader since its only effective goal is support of basic (i.e., disciplinary and academic) science. This leaves academic science isolated from and failing to make its greatest contribution to society. Society's support of science is consequently not as strong as it should be. Somehow the private sector is supposed to cover all the applied research and development and coordinate the continuum of knowledge. With little or no public policy direction and substantial public good elements in applied R&D this has never been realistic. The change in science policy after World War II in my judgment has led to weaker linkage between basic science and technology development and transfer, and thus to lagging productivity. A comprehensive national science policy would include not only disciplinary research but subject matter and problem solving research in which there is a substantial national interest. A comprehensive national science policy would attempt to prioritize all federal science expenditure not leaving priorities segmented as we do now by setting priorities within but not between disparate categories such as federally funded R&D centers and labs; large, unique "big science" facilities; NSF-NIH grants to individual researchers; federal funding of state agricultural research; and funding of internal federal agency research programs (Koshland). Such a policy would recognize appropriate public and private roles in R&D and contribute to guiding its coordination. This may be the counsel of perfection, but it is the only path I can see to lower levels of conflict within science and to an improved performance of science.

The necessity for addressing the complete continuum of knowledge, even within the university, was put in perspective 50 years ago by the philosopher Alfred North Whitehead. At Harvard's Tercentenary celebration of its founding he said:

> In the process of learning there should be present, in some sense or other, a subordinate activity of application. In fact, the applications are part of the knowledge. For the very meaning of things known is wrapped up in the relationships beyond themselves. Thus, unapplied knowledge is knowledge shorn of its meaning. Careful shielding of a university from the activities of the world around is the best way to chill interest and to defeat progress."

Much of the creativity in any discipline comes from the intellectual stimulation of confronting disciplinary knowledge with the test of application, knowledge from other disciplines, and the challenge of societal problem solving. We badly need to recapture this catholic view of science and make it central again to the ethic of science. Failure to do so will leave science less creative and productive, whether viewed from science or society's needs.

Many colleges of agriculture appear to be abandoning their classic responsibility for the full continuum of knowledge in agricultural research. They should reflect on the historical lessons from agricultural science and adapt their behavior and institutions to recapture their original vision, which included the same catholic view of science.

The pressures society is putting on science are pushing us toward a modern science and technology policy that would provide a conscious, coordinated balance of public and private investment across the entire continuum of knowledge from creation to use in areas critical to society. Today, only in medicine and agriculture can full science-based systems of developmental institutions be said to exist, and both of these systems need some institutional rethinking and reform (Bonnen 1986, 1987). Implied are changes on campus, in industry and in government to interlink basic science and its policies with a limited number of long term, science-based missions of significance to society. Consensus-based coordination of effort and policy across disciplines, government and industry is involved. We need to face the fact that subject matter and problem solving research involve more complex and costly administrative support. The lessons learned from science in agriculture are clearly relevant to any U.S. science policy that focuses on raising U.S. productivity. Many of the same lessons can be seen in the history of government involvement in industrial R&D (Nelson; Nelson and Langlois;) Pavitt and Walker; Shapley and Roy).

The Continuum of Knowledge is Expanding Coordinating the continuum of knowledge from creation to use is made even more difficult today by the growing stock of more and more complex knowledge and by the progressive specialization of science. In terms of organizational distance, the extreme ends of the continuum of knowledge are moving away from each other as knowledge increases and science grows more specialized. Any policy that hopes to extract greater productivity from science through a coordinated attack on the practical problems of some economic sector must face the fact that the problems of coordination, administration and management of research have become far more complex. This, combined with the growing complexity of most economic sectors, makes the coordination and linkage of public and private sector problem-solving research and technology and knowledge transfer (extension) much more difficult to conceptualize and manage, especially in what must for the most part be a decentralized system (Feller). Only a decentralized system is likely to have the adaptive capability to work well in such complexity. Policy for it must clearly be arrived at through bargained consensus.

The Paradigm of Science is Changing. Another reason we need to rethink the scope of science policy and its funding system is that the modal paradigm of science appears to be changing. For example, once reasonably separate domains of disciplinary inquiry have, as the frontiers of knowledge have expanded, begun to overlap extensively and interpenetrate each other. Vast areas of physics and chemistry are now common to both disciplines. Indeed the identity of chemistry is all but lost in the rest of science (Browne). The last two Nobel Laureates in chemistry were not chemists but mathematicians. This overlap results in new disciplines or at least separately organized units. We have departments today of biophysics and biochemistry. Advanced math and statistics are essential to the cutting edge in most disciplines. The boundaries of biology and its subdisciplines are transcended not only by physics, chemistry and other disciplines but by technologies and techniques -- e.g. microbiology and molecular biology. Technological capability drives the biophysical sciences as much as science drives technological capability (Price; Shapley and Roy). In the social sciences and humanities this two-way causation includes not only technology but institutions. The social sciences have long had large overlapping domains.

The point is that to practice at the cutting edge in almost any discipline today, even in the biological and physical sciences, requires not only command of a discipline but also of major components of knowledge from related disciplines well beyond mathematics and statistics. As science grows more complex and interactive, a growing proportion of disciplinary inquiries pursued to completion take one through multiple disciplines and techniques. This overlap has become so extensive that creating new disciplines or departments has ceased to be the best or only response. Thus, paradoxically to practice a discipline today one must increasingly collaborate with other disciplines or become in some degree multidisciplinary. This is not really news. The change is only one of degree, but it is so fundamental that it suggests a change is underway in the modal paradigm of science.

All of this is without considering applied science, which is inherently multidisciplinary. The scale, scope and complexity of applied research in the private sector and nonprofit institutes as well as government have grown immensely since World War II, as the importance of applied research and technology development has become more critical to national productivity.

An ever larger part of the economy is more and more dependent on science for profitability. The pace of innovation in some sectors and the integration of world capital and commodity markets places a premium on getting from basic science ideas and technological innovation to a successful product or service with the smallest lag in time. Firms and nations who become laggards tend to get squeezed out. Success requires selective command over the full continuum of knowledge and excellent management and coordination of the R&D, market intelligence and marketing activities involved. National research and technology policy has become a critical element in maintaining a nation's comparative advantage in world markets.

The idea of how science is practiced that evolved out of the 19th century and around which the policies and funding of science, especially basic science, has been organized is predominantly that of the individual scientist surrounded by a few graduate students or laboratory assistants. The importance of this idea of science has been magnified by the mistaken but common belief that all technology arises out of basic science. The rapid development of the scope and importance of mission-oriented, private sector R&D, even in basic science areas, now combines with the growing scale, scope and complexity of applied science and the interpenetration and overlap of one discipline with others to erode the relevance of the old paradigm. What one sees increasingly are R&D consortia and cooperative research endeavors of various sorts. Every session on science policy or university strategic planning I have attended recently has emphasized the need for more multidisciplinary research -- usually without being clear about the difference between problem-solving, subject-matter and disciplinary research. The pressure for collaboration among disciplinary researchers in universities is growing. The numbers of multiuniversity consortia have grown. So have those that combine university and industry R&D efforts to span the full continuum of knowledge addressing some agreed purpose. Industrial R&D consortia have existed for decades. Specific cases of cooperative research involve quite different purposes and portions of the continuum.

The motives for these arrangements include cost sharing where there are economies of scale (often involving large, specialized research facilities or tools) as well as the need to assemble diverse disciplines for fundamental research or to bridge some part or all of the continuum of knowledge from disciplinary through applied, subject-matter and problem-solving inquiry.

This shift toward more collective or cooperative research consortia makes the funding question very much more complex for everyone from NSF and NIH to the foundations and industry, as well as the Congress and state legislatures. It opens up the question of what are appropriate funding mechanisms and puts the emphasis on the very different purpose or goals of disciplinary, subject matter and problem solving research and thus the differing mixes of these types of research presided over by different kinds of collaborative research efforts. I believe both the competitive grant, basic science experience as well as industrial and the agricultural science experience are relevant -- if all parties to the debate will listen to the others and think objectively about the problems we face in common.

<u>The Private Sector R&D Role is Growing</u>. The private sector presence in R&D has been growing rapidly. This is especially significant in economic sectors where vertical integration produces large oligopolistic firms who have substantial influence and control over the industry's demand and supply functions. Such firms know they will be around 10 and 20 years from now. They can and increasingly do invest in both basic and applied science to guide and control the conditions of that future. Even so, few of these firms can afford the scale of basic and applied science investments they might like or that

society needs if it is to be effective in international competition. This creates a growing interdependence between public and private R&D that generates pressures for collaboration and joint ventures. It is pulling university basic science into coordinated efforts that cover large parts or all of the continuum of knowledge and into new arrangements for funding both problem-solving and subject-matter as well as disciplinary research.

In agriculture, private sector R&D has developed more slowly than in industry because of the more atomistic nature of agricultural production. However, concentration is proceeding rapidly in agriculture and especially in agribusiness today. With the ability to patent plant material and biotech processes and with concentration has come a more rapid growth of private agricultural R&D. The private sector is now taking over many areas of applied research that have been a public responsibility. The consequence is that agricultural science and the land grant system are becoming somewhat less problem and product specific in some areas and more a general science wholesaler than a retailer. Agriculture science is being pressed toward a mix that is relatively heavier on basic science combined with a somewhat different set of applied science and extension activities. Agriculture is moving rapidly toward a considerably more complex, vertically integrated, high technology sector.

While still rather different, agricultural science and academic disciplinary science are being propelled toward a more common set of responsibilities, problems and activities. In common they face the need to redefine the changing boundary between public and private R&D responsibility. The growing role of private R&D means that the private research institutions will have to have a far more significant role in the coordination of science policy. With greater intermixing of public and private motives, the public sector (Congress, universities, science professions) must find new ways to assure the integrity of science and its decision processes. Private purposes can easily dominate joint ventures thus forfeiting much of the larger social benefit that might otherwise be achieved from collaboration between the public and private sector (Ulrich et al). The public institutions are responsible to assure that the public interest in science is served. Much applied R&D remains a public good that will be ignored, if applied science is ceded to the private sector without thought.

A mature industrial nation's comparative advantage in international markets rests on high technology and high human capital industries such as electronics, computers, communications systems, education, finance and, in many cases, agriculture. The United States economy is increasingly dependent on a coordinated scientific effort to remain competitive in markets for high technology products and services.

Our ability to sustain the kind of R&D policies that will support a high technology economy is being undermined by the drift of the academic community toward the view that the only research of importance and the only research worth financing and doing is basic science (disciplinary) research (Shapley and Roy). At the same time that the academic community's capacities have shifted to the disciplinary end of the research spectrum, the problems of society have become more specialized, interactive and complex, requiring (besides disciplinary research) greater coordination with and investment in applied, multidisciplinary research of a subject matter and problem solving nature. If all the applied research could be done by the private sector this would only be a problem of coordination. But most early technology, human capital and institutional development and much of the adaptive and maintenance research in biology are clearly a public good and beyond the private sector's capability. Thus, the training and values of much of academic science are undermining the society's capacity for problem solving, while the need for such capacity grows more intense.

<u>Agricultural Science Policy</u>

Up until 1916, agricultural research activities accounted for one quarter or more of the USDA budget. Today, a far larger research enterprise accounts for less than two percent of the USDA's budget and about the same percentage of total federal R&D expenditures (OTA).

Today the private sector accounts for about two-thirds of all agricultural R&D expenditure (Ruttan pp. 181-186). Two-thirds of this is concentrated in physical science and engineering and only a small but growing part can be described as basic science. At the state experiment stations about three-quarters of the research is in the biological sciences and technology. According to Ruttan, social science research accounts for less than three percent of private sector R&D and less than ten percent of public sector R&D in agriculture (p. 186).

The funding of science began to change after World War II with the creation of the National Science Foundation (NSF) and the great expansion of the National Institutes of Health (NIH). These institutions today support a large public and private academic science structure, mostly disciplinary in nature and, with a few exceptions, largely outside of and unconnected with the land grant-USDA system of agricultural science institutions. This means that the bulk of basic biological, physical and social science and humanities research, some portion of which is undoubtedly relevant to agriculture, today lies outside the system of agricultural linkages.

The impact is more pronounced because of the fragmentation of university disciplinary science into a progressively greater number of separate academic units as scientific knowledge has expanded and become more specialized. The consequence in the land grant colleges is a steadily increasing organizational distance between applied research in agriculture and some of its disciplinary roots. It has increased the difficulty involved in interlinkage and coordination of the continuum of knowledge from its creation to use. In part this is why we are having difficulty maintaining a balance across this continuum in agriculture. Some land grant colleges have worked to maintain effective linkages, others have not.

Complicating this is the dominant value belief of the academic science establishment that only disciplinary research in the biological and physical sciences is academically respectable and justified. The support given the social sciences tends to be limited to the behavioral sciences and to positivistic inquiry within them. Questions about what has value are not considered to be in the domain of science and are treated as subjective and nonscientific. The response in some colleges of agriculture to the changing values and distribution of power in academic science has been a parochial, nearly exclusive focus on applied problem-solving research for agriculture.

Other colleges of agriculture, many land grant universities and some agricultural professional associations have absorbed as their ideal the academic science establishment's focus on disciplinary research. Their "search for academic excellence" is denaturing the land-grant tradition of problem solving and service to all people, irrespective of wealth or position. A near exclusive focus on basic discipline depreciates applied, multidisciplinary research, denies admission of problem solvers and prescriptive analysis to the academic pantheon, and turns good land grant universities into second-rate, private academies. Such an environment destroys the basis for effective extension education and problem solving, and lowers the potential productivity of any agricultural science investment. Today these two parochialisms of "pure" and "applied" science constitute an obstacle in the search for an appropriate balance of investment across the continuum of knowledge necessary to achieve greater national capability (Johnson 1984).

Agricultural research is nationally of minor political concern today. Over recent decades the congressional interest in USDA research budgets has focused primarily on applied commodity research and the proliferation and location of regional research laboratories in selected congressional districts. The narrowing of farmer interest to immediate farm program benefits combined with the lack of scientific vision in either congressional or USDA political leadership has over several decades contributed to a confusion of purpose and to an erosion, isolation and fragmentation of the USDA's national research capability in the biophysical and social sciences.

A once-effective priority-setting process has been undermined by abuse of the R&D function by Congress and USDA political leadership, the erosion of USDA research dominance, a decline in the dependence of the colleges on the USDA, as well as the rise of new public and private R&D actors of varying importance to agriculture but outside the agricultural science system. Relevant research activities are not as well interlinked and coordinated. Agricultural research is in large part a public good. In a policy process dominated by highly organized economic interests with destructively narrow, short-term views of self interest, public goods are of little concern. Why worry? The agricultural cornucopia will always flow. But will it? Or will the public interest be served?

Institutional changes since 1977 attempt to deal with part of this problem. The establishment of the Joint Council on Food and Agricultural Sciences, the National Agricultural Research and Extension Users Advisory Board and more recently an Assistant Secretary for Science and Education create a potential for greater clarity of purpose and coordination of priorities.

As an industrializing nation's agriculture develops, its production and marketing processes inevitably become highly specialized and its welfare and performance increasingly vulnerable to disruptive forces from outside the sector. The result is growing government policy intervention in agriculture, an expanding private-sector interest in public policy outcomes, and ultimately severe fragmentation of economic interests as development proceeds. This fragmentation leads to rising levels of political conflict and disorder among the institutions of agriculture, along with the domination of the policy process by progressively narrower economic interests that make it far more difficult to pursue long-term goals, especially research that promises to provide only diffused or problematically distributed benefits. As a consequence, as long term, steady support for agricultural science research has become absolutely essential to the future of a high technology agriculture, the increased fragmentation and narrowing of the economic interests in agriculture make it increasingly difficult to mobilize broad support for long term goals. This can be seen in both Europe and the U.S.

Thus, most industrial nations with highly productive agricultural sectors face an eventual political-organizational crisis in deciding whether or not, and in what form, they will sustain the science-based developmental system in agriculture that with varying degrees of success they have created. Failure to maintain that system will substantially disadvantage an industrial country both internally and in international affairs. Food will always be a strategic necessity, whatever a country's resource base.

Since agricultural sector political power would appear inadequate to sustain a modern, balanced science base for agriculture, agricultural science needs to become more nearly an integrated part of the science establishment. But the integration and cooperation needed between the two science establishments will not come unless there is greater mutual appreciation of the strengths each would bring to a common, more coordinated endeavor. The old land grant model exhibits many of the desired characteristics of such a system, but even it is in need of institutional rethinking to adapt it to the modern political environment of science and agriculture.

The developmental system of institutions of agriculture, although in some disarray, now seem to be adapting to these changes. There is in enough disorder to raise questions about the systems' continuing viability as a system. Most of the institutions will survive, but will the system? I believe it will.

Outside of agriculture most scientists are both ignorant and critical of the agricultural research system. Indeed, it is complex and not easily understood. National commitment to these institutions is in question. Yet, if past experience means anything, we must provide some kind of system for coordination and management of the complex of relevant national and local, public and private institutions, if we are to continue to have an agriculture that is competitive in world markets. Agricultural science differs in its needs from medicine and other parts of science. Such differences must be recognized and accommodated in science policy and its funding. Agricultural scientists must be able to explain those differences.

The Social Sciences in Agriculture

The current agenda of issues in agriculture and the rising criticism of the impacts science suggest strongly that inadequate investment has been devoted to social science and humanistic inquiry in agriculture. Successful biological and physical science-based technological growth has created externalities and imbalanced investments in the four prime movers. We now face needs for technology assessment, new institutional innovations and research on ethics and values. The latter two especially are the domains of the social sciences and humanities for which we have failed to provide adequately in science policy, public or private. There is no market incentive for industry to assume the costs of dealing with externalities. Many influencial agricultural scientists either do not want to believe that their scientific inquiries involve such problems or simply see their social responsibility as limited. The public sees it otherwise.

Look at the larger agenda of issues now facing agriculture. It includes the largest financial crisis in agriculture since the Great Depression; complex and poorly understood national and international macroeconomic impacts on agriculture; major international trade issues ranging from protectionism and an immense trade deficit to the impact of obsolescent international monetary institutions on exchange rates and market stability; and the impacts of national deregulation (in finance and banking, petroleum-based energy sources, and transportation) on agricultural and rural welfare and property rights. These problems all fall in the domain of the social sciences.

Look at the need for new or modified institutions. The institutional structure supporting science in agriculture and nationally is in transition to some new configuration with almost no research on the issues involved -- such as research funding systems, or the means for interlinkage and coordination of R&D actors, or on science policy itself. The experiment stations, USDA, NSF and NIH all should be targeting this area of R&D. The nation's farm policy is in shambles but continues for lack of convincing alternatives to constrain instability and periodic excess capacity. The new genetic technologies are changing the way agricultural science is funded and managed and is raising new issues in property rights to genetic material as well as to processes for genetic manipulation. Information age technologies are changing the way we receive, process, store and use information in farm and agribusiness decision making. Adapting agriculture and its institutions and policies to these and other new technologies presents a substantial research and education challenge to the social sciences. A major issue that must be faced is the future social performance of a farming and agribusiness sector that will be much more highly concentrated and vertically integrated. One could go on.

These are all issues that science policy must address seriously, if it is to meet society's expectations and needs. In agriculture this requires more than economic expertize. Major sociology, political science and legal research is necessary. In addition some social psychology and cultural anthropology research is also relevant.

As for the humanities, the lack of any systematic historical perspective on agriculture disorders agricultural leaders views. The history of the development of agriculture and its institutions needs to be taught and researched. The growing set of ethical issues and value related problems in agricultural policy and in science require philosophic attention.

Agricultural economics is often the only established social science department in colleges of agriculture. As a consequence I believe agricultural economists have a responsibility to make the case for the missing social sciences and humanities in the colleges. We have neither the personnel nor the expertize to meet this flood of issues by ourselves. We also have to improve our own sense of purpose and performance before we can provide much leadership.

Since World War II agricultural economics has been drifting toward an antiempirical and a disciplinary outlook -- away from the great empirical tradition around which the profession was built and upon which its reputation still rests. Today we celebrate theory and statistical methods while ignoring the data collection and problem solving necessary to validate our theory and models. Any profession becomes what it celebrates and rewards.[3]

Why is this happening? First, we are emulating academic economics, which, with some distinguished exceptions, now exhibits little commitment to the empirical (Leontief). Another source of the problem, I believe, is the search for "academic excellence" in agricultural economics that places excessive or sole emphasis on rewarding the development of disciplinary knowledge, almost to the exclusion of the development of subject-matter and problem-solving knowledge, both of which are essential outputs of an effective agricultural economics department.

Thus, a badly flawed notion of what agricultural economics is about is leading to incentive structures for tenure and promotion penalizing those who do empirical work or who would spend large parts of their lives in applied, problem-solving and subject-matter research, without significant disciplinary contribution. It is not surprising that many of these same departments now have some difficulty sustaining a vital extension activity and are losing public support because their clientele perceive them as not very useful. Agricultural college departments are applied, subject-matter fields with responsibility not only to science but to clientele for specific areas of problem solving. Disciplinary capability is vital but we are not, as an institution, free to focus exclusively on disciplinary research: individuals yes, departments and colleges, no. When entire departments devote themselves solely to pleasing disciplinary peers, they eventually lose much of their understanding of and relevance to the society and its problems. This undermines the social value of agricultural economics and the capabilities that brought the profession to where it is. It leaves agricultural economics without a culture capable of sustaining extension or many types of applied research. We cannot surrender the goals and culture of agricultural economics to that of economics. If we do, we will have become at best second-rate economics departments of which there is already a sufficiency.

This failure afflicts entire colleges some of which have become collections of disciplinary researchers unable to address the problems of agriculture effectively. Unless they rediscover their mission they are unlikely to survive in the long run, for not only will clientele desert them, but the college's rationale for independent existence disappears.

The model of an agricultural college department as a collection of "pure disciplinarians" producing disciplinary and some applied disciplinary knowledge is a pathological distortion of the land grant mission. Yet that is the model some colleges and agricultural economics departments are now following. Just as pathological is the purely applied model of a subject-matter organization unconnected to the appropriate range of supporting disciplinary capacity in teaching and research.

The social sciences in agriculture face great challenges and opportunities. But we must be able to address them with a balanced command over the full continuum of knowledge, if we are to be successful. We need to see our role as one of producing disciplinary, subject-matter and problem-solving knowledge. We need to do this in an enterprise that brings into an integrated focus both the agenda of science and the agenda of problems in agriculture.

Many colleges are now striving to regain a better balance in their science capability. We only need to keep a clear focus on the problems of agriculture and rural society and on the capability and potential of science to drive our enterprise back toward a better balanced command of the continuum of knowledge. Agriculture needs leadership today with a broad vision for the future of a science-based agriculture. It needs strong leadership from the social sciences.

We are entering an exciting new era in the social sciences in agriculture. Young professionals especially face great opportunities and new challenges. We have the capacity. I know we will succeed. We have only to do it.

Footnotes

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The author is endebted to colleagues at Michigan State University for critical reviews, most especially Glenn L. Johnson, Eileen van Ravenswaay, Stanley Thompson, Lester V. Manderscheid, Larry J Connor, James F. Oehmke, Lindon Robison, James D. Shaffer, Allan Schmid, David Schweikhardt, Jack McEowen and Larry Hamm.

1. Institutions I define to include both the rules of behavior that govern patterns of relationships and action as well as public agencies, private firms, families and other decision-making units (Knight, Commons).

2. See Johnson and Rossmiller (pp. 29-33) and Johnson (forthcoming) on types of knowledge.

3. For a more complete treatment see Bonnen (forthcoming).

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