

Land, Economic Change, and Agricultural Economics

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This paper analyzes in three contexts the effects of changing economic conditions and varying economic perspectives on the way land is considered in economic doctrine. The first considers agricultural land use where agriculture is connected to the rest of the economy exclusively through input and commodity markets, and when all other parts of the economy are assumed to remain constant. The second connects agriculture to the remainder of the economy by virtue of a shared natural environment, facilitating a discussion of natural resource and environmental economics in relation to agricultural, institutional, and land economics. The third context permits economic change in the entire economy with particular attention given to population density, space, and distance. Private and public decision making are discussed with attention to federal, state, and local division of powers.

Key Words: applicability limits, central place theory, public and private decision making, rural economics

The Inconstant Role of Land in Economic Doctrine

Land plays numerous roles in economic doctrine. Economies always are in a state of change as they move on their unique trajectories through time. Further, those who make use of economic doctrine have different motivations for doing so. If the role of land in economic doctrine is to be understood, one must account not only for economic change, but also the motivation of economists.

The applied economist does not have the luxury of using a single conceptual lens to view reality. Land provides a different bundle of goods and services in the rural areas of the Great Plains than it does in the urbanized Northeast. Further, the motivation of researchers who seek to understand land varies among, as well as within, geographic situations. When speaking or writing about land, economists will not communicate well even within their own

discipline, unless the economic characteristics of land, as well as the motivation of analysts, are recognized explicitly.

Three distinct contexts are considered in this paper. One is concerned with the agricultural firm and the agricultural industry in an economy wherein the remainder of the economy is placed under the heading of “all other conditions remain the same.”¹ A different context arises if the agricultural use of land has effects on the remainder of the economy that cannot be reflected in the outcomes of unregulated markets. I make reference to external effects which are not consistent with market-generated input or output prices under “ideal” conditions, a condition described in introductions to numerous texts in environmental economics. The third context considered involves the use of land as a policy instrument to influence the nature, direction, or impact of economic activity.

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¹ See Hausman (1992, chapter 8) for the methodological justification of the phrase “all other conditions remain the same.” He refers to John Stuart Mill who believed the laws of economics would hold true in practice if it were not for “disturbing influences.” Holding “all other conditions the same” was a means of removing the effect of “disturbing influences.” When read today, the writing of Mill on the subject seems somewhat quaint. Upon reflection, however, Hausman shows the issues identified by Mill are highly relevant to the way economics is practiced in our time.

The view here, then, is that land plays different roles in economic affairs, and there exists more than one valid analytic approach that may be applied to land. This view is consistent with a pluralistic economic research methodology described by Bruce Caldwell (1982). The contrary prevailing, but largely implicit, view of economic research methodology asserts that one basic paradigm for economics exists. This paradigm is usually considered to be the neo-classical model underlying the theory of the firm, consumer demand, price determination, partial and general equilibrium, and welfare economics (see Hausman, 1992, figure 15.1, p. 271).

In addition to this neoclassical core, numerous fields exist in economics such as labor economics and public finance. If empirical work in economics is to be appraised thoroughly, these fields within economics should be considered, as well as that stemming directly from the theoretical core. If this were done, it would be found that numerous approaches supplement, replace, and modify the neo-classical model. Yet, to the best of my knowledge, there is little literature appraising how decisions get made which establish limits of applicability of particular economic models in the various applied fields of economics. I suspect such decisions by applied economists typically reflect their judgments that some models just don't "work well" in practice. Standard theoretical assumptions often are replaced by more realistic auxiliary conditions. This may limit the scope of the model, but with the result that it "works better" for the problem at hand.

In other cases, neoclassical economics may be modified or replaced by institutional economics, game theory, Austrian economics, or even with (gasp!) a discipline other than economics. The process for making these kinds of paradigm shifts is not well understood for reasons set forth by Kuhn (1970). The appendix to this paper constitutes a mini-essay on intellectual constructs and the limits of their applicability.

The term "land" as defined here is consistent with the way classical economists used the term. Those parts of the natural environment with past, present, or potential economic value constitute land as the term is used in this paper. In some passages, land will refer to a part of the earth's surface, but elsewhere the term may be used to pertain to another part of nature such as the atmosphere, or a species. In all cases, the context will make clear what is intended. This paper is mainly about the way agricultural economists have thought about land as circumstances vary. A subtitle of the paper could

well be: *The Ways Agricultural Economists Think About Land*.

Land and Agriculture

Land played a central role in the writings of Adam Smith, David Ricardo, and Thomas Malthus. In many respects, these classical economists were the first agricultural economists. Food production was a principal economic activity at the time they wrote, and it is not surprising they devoted considerable attention to food-land relationships. Land, labor, and capital were advanced as the principal factors of production with the justification that each factor was distinctly different from the others. This point of view dominated economic literature well into the 20th century.

David Ricardo lived from 1772 to 1823; Thomas Malthus from 1766 to 1834; and, of course, Smith's *Wealth of Nations* was published in 1776 (Eatwell and Newman, 1987). Food production technology changed greatly in the more developed nations during the century following the death of Malthus. It is not surprising that 20th century economists were forced to reconsider the received wisdom with respect to land and food production.

Two important milestones can be identified. One, of a conceptual nature, was the work of Frank Knight on risk and uncertainty, published in 1921. He directed attention to the importance of the human agent in adapting to risk and uncertainty. In Knight's view, there was no need to distinguish land from other forms of capital. The other milestone was empirical in nature and occurred when the economics profession came to understand that food-land relationships had changed in a fundamental way since the time of the classicists. If one wishes to date this milestone, consider 1951, when the T. W. Schultz article entitled "Declining Economic Importance of Agricultural Land" appeared in the *Economic Journal*.

Shortly after World War II, production economics emerged as an important part of agricultural economics. Earl Heady's *The Economics of Agricultural Production and Resource Use* (1952) influenced the thinking of many agricultural economists on numerous subjects, including land. Heady saw no need to give special treatment to land and, consistent with Knight, believed only labor and capital need be considered as factors of production.

The work of the agricultural production economists first focused on the farm firm and then progressed naturally to agricultural industrywide

considerations. Empirical work multiplied rapidly as a result of new quantitative techniques and data processing equipment such as numerical programming and electronic computers. As a result of these developments, a subtle shift in emphasis occurred in agricultural economics research. The greatest emphasis and recognition shifted to quantitative developments, perhaps at the expense of problem identification and theoretical awareness.

The agricultural economics policy literature reflected these trends as well. Even though empirical estimates of policy effects were made possible by the use of sophisticated measurement techniques, the underlying theoretical base was remarkably predictable. Constant returns to scale, atomistic competition, and homogeneous products were often assumed, and modern welfare economics provided the normative base of assessment. Technical change typically was considered an exogenous variable, and bore responsibility for declining costs over time.

Conclusions resulting from such models usually either explicitly stated or implied government price supports were inefficient, and that supply-demand imbalances would disappear if the industry could just get through whatever the crisis existed at the time, assuming government could be persuaded to leave agriculture alone. This traditional approach did not anticipate the industrialization of agriculture, even though economic theory has long recognized the importance of, and empirical information has suggested, the reality of increasing as well as constant returns to scale.

The denial that land has characteristics distinguishing it from other forms of capital encouraged the view that agriculture is connected to the rest of the economy only through input and commodity markets. This notion resulted in the neglect in the agricultural economics literature of both positive and negative nonmarket effects of agricultural output on the rest of the economy. As will be noted later, a few resource economists within agricultural economics devoted considerable attention to nonmarket considerations related to land prior to Earth Day in 1970.

The methodology used by the production economists after World War II was based largely on partial equilibrium models pioneered by Alfred Marshall. The work of John R. Hicks and others permitted ordinal utility assumptions to be substituted for the cardinal utility formulations of Marshall. The partial equilibrium models of the early production economists meant the assumption that “all other things remain the same” continued to be the prevailing

model for all of the economy which did not fall within the scope of the partial equilibrium model.

In recent years, computable general equilibrium models have come to economics. These have been made possible by developments in theoretical economics and in data processing technology. There is little question that many more economic relationships can now be modeled simultaneously than was the case earlier. Yet even the most elaborate general equilibrium model must neglect a great deal of economic reality. “Partial” and “general” equilibrium as used in contemporary economics are not end points that encompass economic activity. Rather, they are points along a continuum extending far beyond anything that has been modeled to this time. The assumption “everything else remains the same” continues to apply whether made explicit or implicit. Even so, computable equilibrium models of interdependent economic relations may bring important information into the open. For example, a recent general equilibrium study of international trade reports even modest increasing returns in an industry can have significant effect on factor demands (Antweiler and Trefler, 2002).

There are many interests in rural land, over and above its contribution in agricultural production. Even so, agricultural economists have a special responsibility to understand the role that land plays in agricultural production. A full understanding of that role depends on a better understanding than we now have of the relation of technical change to economies of scale in agricultural production.

The assumption of constant returns to scale, widely employed in agricultural economics, has been very convenient in econometric and programming work. Yet the empirical base for such an assumption does not appear to this observer to be well established. If the ambiguity surrounding this important relationship is to be removed, the following issues must be addressed:

- There must be clarity as to whether reference is being made to agriculture or farming. With the passage of time, farms have purchased more inputs and more processing and marketing has moved off the farm. It seems clear that farms do not occupy a fixed place in the agricultural industry.
- The quality of inputs used in farming must be considered when measuring the quantity of inputs used over time. Traditional economic theory treats technical change as exogenous to the industry, and this results in shifts in the

long-run supply function. Yet technical change occurs in many ways, and when input productivity changes over time because of endogenous technical change, it is difficult to measure the quantitative change in inputs over time.

- A part of endogenous technical change has been associated with the shift of a portion of farming activity to nonfarm firms in both input supplying and in output processing and marketing. The consequence is that a comparison of the cost of farm output over time violates an assumption of a homogeneous output. What does it mean that costs are constant over time if the nature of the output changes over time?

These issues are introduced here because they pertain in an important way to the conceptual foundation of the agricultural policy literature in agricultural economics. I fear this literature has been so preoccupied with agriculture-government relations it has neglected important structural considerations in the industry. The industrialization of agriculture should not surprise us. The determinants of this development have been in place for some time (Castle, 1998).

Institutional Economics, Environmental Economics, and Land

As economic growth occurs, the face of the earth is modified as a result of that growth. Some changes may be benign, but not all will be, and there will be uncertainty about the long-run impact of others. As incomes rise, and the location and concentration of people change, the importance ascribed to different attributes of the natural environment will change as well.

Earth Day occurred in April of 1970, but by that time resource economics was a recognized specialty in both agricultural economics and economics. The establishment of this specialty anticipated the general recognition in society that unregulated input and commodity markets did not, and could not, reflect the interdependencies imposed by the earth. This realization directed attention to nonmarket institutions which had been used by societies to define relations among individuals and groups in the management of the natural environment. Interests in such institutions and institutional economics arose quite naturally.

Classical and neoclassical economics have played an important role in agricultural economics throughout its existence. Nevertheless, it would be a great

mistake to neglect institutional and land economics when discussing land and agriculture. Fortunately, the late Maurice Kelso provided a short and competent account of the origin and content of institutional economics, and how land economics arose from it (see Castle et al., 1977, pp. 394–406).

It is not surprising that institutional economists would pay particular attention to land. Many societies have found it necessary to develop special institutional devices for the management of land because of its unique characteristics. The durability of land, its capacity to serve multiple ends, its capacity to absorb human-created capital across generations, and its indivisibility are examples of characteristics requiring special treatment in formal and informal institutions, or to be reflected in “the rules of the game.” Those rules vary over time and among societies.

For many years, land economics occupied a respectable, but never a dominant, place within agricultural economics. Three considerations probably led to its decline after 1950. First, institutional economics was basically incompatible with logical positivism, as modified by Popper (1957, 1959), the dominant philosophy of science in the second quarter of the 20th century. Institutional economics lacked a formal theoretical structure to guide empirical work, although it was, and is, empirically oriented.

Second, since World War II especially, developments in neoclassical economic theory and quantitative techniques stimulated research on farm and marketing firm problems, demand analysis, and agricultural industry adjustments. Economic theory, used to guide data collection and analysis, seemed to be more “scientific” than the writings of institutional land economists. (See the appendix for an explanation of why agricultural economists may have embraced the philosophy of Karl Popper.)

Third, shortly after the end of World War II, the natural resource economists arrived on the scene in both economics and agricultural economics. Natural resource economics made considerable use of neoclassical theory and quantitative techniques, as did agricultural economics generally. Land economics, as a specialization within agricultural economics, has nearly disappeared, and agricultural economics has become homogeneous methodologically as a consequence.

The economics of institutions, however, still exists in economics. Douglas North has become a Nobel Laureate as a result of his work in institutional economics (1990). Rutherford (1994) draws

a distinction between the “old” and “new” institutional economics. The distinction appears to be that the “new” institutional economists make greater use of neoclassical economic theory in their research, and often implicitly or explicitly accept the norms of welfare economics. The “new” welfare economists generally come to more conservative policy positions than did the “older” institutional economists.

The use of the neoclassical market paradigm for the analysis of economic institutions poses special problems. Market incentives are often cited as a principal reason for institutional change in the new institutional economics; inefficient institutions become too costly to be tolerated. Yet some institutions persist for long periods even though they clearly are inefficient on the basis of market criteria. This leads to concern about the simultaneous use of market criteria for both normative and positive purposes as is practiced in the “new” institutional economics.

Is it appropriate to explain the existence of something on the basis of a criterion, and then judge its desirability by use of the same criterion? If applied literally, such a theory would have no capacity to explain the persistence of nonconforming institutions. To be sure, the new institutional economists have explanations for the persistence of “inefficient” institutions, but such explanations seem to be of an ad hoc nature, usually pertaining to some form of path dependence. Unless path dependence can be explained in institutional terms, a theory of institutional development has not been provided.

Resource and environmental economists now have their own professional association and journal. The market paradigm has become paramount. Market type incentives and outcomes are used to judge the success or failure of environmental nonmarket institutions, policies, and programs. The norms assumed in resource and environmental economics typically come directly from welfare economics rather than from concerns reflected by the policy process itself. The field is based solidly within the neoclassical paradigm that dominates both economics and agricultural economics.

As one who played a role in the early development of resource and environmental economics, perhaps I am entitled to identify some troubling issues I believe lurk beneath these indicators of success. One is of a technical and methodological nature. The other is also methodological, but more fundamentally, an epistemological matter. Permit me to elaborate.

Resource and environmental economics literature seldom gives explicit attention to the emerging role

of the federal establishment, relative to state and local government, in environmental policy. Since the turn of the century, the federal establishment has assumed considerable responsibility for the development and conservation of natural resources. More recently, especially since Earth Day in 1970, the federal government has passed environmental legislation pertaining to water, air, and species protection. Use rights remained largely under state jurisdiction. Such integration as occurs, other than by litigation, often comes from the role state agencies play in the administration of federal programs. Local government influence stems mainly from local initiative within the framework of state law.

Standard welfare economics and neoclassical policy models do not make provision for decision units intermediate between firms and individuals at the micro level and the state at the macro level. Mainstream resource and environmental economics reflects this same structure. The implications of such simplifications become apparent when federal, state, and local interests come into conflict as they have recently in the application of the Endangered Species Act in the Klamath Basin of Oregon. Yet the problem is a fundamental one, and not of recent origin.

The maximization of net national benefits is the standard criterion used in benefit-cost analysis to judge resource and environmental policy actions within a nation. Can you imagine those with responsibility for state and local affairs using such a criterion to evaluate their options? I cannot—perhaps because I served for seven years as a member of the Environmental Quality Commission of Oregon. My knowledge of resource and environmental economics was useful in that service, but not because I applied welfare choice criteria or empirical findings from resource and environmental economics literature.

Perhaps an example will illustrate my concern. Assume the federal establishment provides financial aid to farmers who have had water deliveries curtailed because of application of (say) the Endangered Species Act. From the standpoint of the Nation, any such payment would create certain negatives or costs if the standard welfare criterion were used.² Of course, the possible benefit of preserving a species would be “off the table” because, by decree, species preservation is of the highest priority, regardless of cost. Even so, there may be interest in knowing the amount of economic sacrifice associated with the

² Some may consider such payments purely as a transfer and at no net cost to the Nation. However, if the payment requires increased federal spending, the Nation’s net national product likely will be reduced.

water curtailment. From the standpoint of the local economy, however, government financial aid to farmers is not a negative, but rather a positive item. And, of course, the farmer receiving the aid will consider the payment positively.

Let us now consider outlays farmers might make for well drilling to obtain groundwater to compensate for the curtailment of surface waters. Such expenditures would be a cost in the calculus of farmers, but a benefit from the standpoint of the local economy. If the well drilling proved to be a productive investment, it would increase the net national product and be a positive influence in the national account. On the other hand, it would be a negative if this involved additional government spending. If the payments made to farmers were diverted from other government programs, there would be no cost to the Nation.

These straightforward and simple examples illustrate the complex and often conflicting objectives of decision units as movement is made from the micro to the macro level. The welfare maximization criterion, or “optimality” assumptions, so readily applied in resource and environmental economics, results in gross oversimplification if intermediate decision-bodies are ignored. If they are not ignored but considered individually, optimization of the welfare criterion will accomplish little except to highlight inconsistencies and conflicts. Recently I heard prize-winning research described as pertaining to the “optimal choice of unintended consequences.” For some time, I have believed economists’ preoccupation with optimality had gotten out of hand; that description did nothing to change my mind. Think about it!

With the help of Daniel Bromley, I now see that the neglect of intermediate decision-bodies in resource and environmental economics is but one manifestation of the second concern I have regarding contemporary resource and environmental economics (Bromley, 1990, 1997; Vatn and Bromley, 1994). Economic optimality measures, or benefit-cost analysis, typically provide the yardstick by which a particular rule, policy, existing condition, or practice is judged in the natural resource or environmental economics literature. Existing entitlement, endowment, choice criteria, and income distribution, except for the item under examination, are assumed to be optimal or acceptable. The same is true of prices. Except for the price of the item under examination, prices are assumed as optimal, acceptable, or capable of being adjusted. The determination of “optimality” which results clearly is affected by such

assumptions. The justification usually given is that the assumed conditions would have been changed by society if they were not acceptable.

Suppose, however, the real reason a proposal is supported or opposed by someone in the policy process is that they wish to change indirectly one, or more, of the assumed conditions by means of the proposed policy change. Many, and perhaps most, policy proposals attract support or opposition arising from multiple motivations of proponents or opponents. In such circumstances, economic optimality measures, based on conditions proponents or opponents wish to change, may be either irrelevant or misleading to many people because they do not accept the determinants of the optimality test.

When resource economics first came into existence approximately a half century ago, an economic environmental problem was believed to exist if it was thought decentralized markets did not reflect incremental social costs and returns. The early legislation relied mainly on command-and-control measures to correct market failures. From the outset, economists called attention to the contribution market type incentives could make if used in conjunction with command-and-control measures. A fundamental change appears to have occurred in environmental policy. Instead of presuming markets have inherent limitations in dealing with environmental issues, the presumption appears to be that unless an “optimal” market solution is found, a policy failure has occurred.

Hausman has noted that economists have taken steps to preserve the “separateness” of core economic theory even when the “inexactness” of their assumptions has been demonstrated. (The term “inexactness,” as used by Hausman, is a different concept than unrealism; see Hausman, 1992, chapter 15.) Something of this nature may have occurred in resource and environmental economics. Although fraught with theoretical and practical difficulties, a benefit-cost mentality may be implicitly accepted by many economists for a wide range of issues including, but not limited to, resource and environmental economics (Arrow et al., 1996). Many of us enjoy doing those things we believe we do well, especially those things we believe we do a bit better than others. And economists can measure economic optimality better than anybody, especially as we define it.

Consider again the Endangered Species Act. The Act has been interpreted by the highest court in the land to say that species protection is of the highest priority, regardless of the cost of doing so. Even

though benefit determination is off the table, many have legitimate interest in knowing something about the cost of compliance. Economists have a high comparative advantage in supplying such information. Yet I submit the most important policy issues are of a different nature. For example, we need to know far more than we do about how the Act is being applied, or not applied, in different circumstances.

It is clear the Act is not being applied according to Congressional intent as stated by the Supreme Court. We know considerable negotiation occurs prior to the listing of particular species, and that political and economic considerations enter into this process (Huntsberger and Walth, 2002). Until there is knowledge of how the Act actually works, it is guaranteed there will be bitterness arising from particular applications. In addition, there is great need for debate and discussion of different approaches to species loss, such as habitat protection, rather than a single-species approach. Resource and environmental economists can be of great assistance in illuminating such questions. To do so, they will need to avoid sweeping under the rug information that is at variance with disciplinary assumptions. And they may need to surrender some disciplinary autonomy in order to work with other disciplines.

Two recent environmental problems bring these methodological issues into the open. Both involve knowing when the limits of conceptual models have been reached. One pertains to appropriate public policy in the face of possible climate change. The other deals with the Exxon-Valdez oil spill. I turn first to climate change.

Since benefit-cost analysis first came on the scene, economists have been concerned about the appropriate discount rate to use when evaluating government investment in the natural environment. Numerous tracts have been written on the subject under the assumption that an appropriate social rate of discount always exists if only it can be discovered. In other words, the universal applicability of benefit-cost analysis is not questioned; the challenge is to just be clever enough to use it.

Not all economists have been of this persuasion; Talbot Page (1988) has labeled the search for an optimal discount rate a "will-o'-the-wisp," but the search continued. Recently however, possible climate change has raised the question of whether steps should be taken to delay or forestall such a development. If such steps are to be taken, can they be evaluated by benefit-cost analysis? If so, what discount rate should be used? Any finite discount rate defines a planning horizon that will

not encompass all future effects of climate modification measures.

Two leading economists, William D. Nordhaus and Robert C. Lind, have declared there is no logical basis for establishing a discount rate for such a problem. In effect, they have said benefit-cost analysis may be useful in estimating the opportunity cost of certain actions. They believe it should not be substituted for ethical decisions pertaining to the welfare of different generations.^{3,4,5} They are to be commended for stating there are limits to the applicability of a conceptual framework.

In the case of the Exxon-Valdez oil spill, the law required that damages be assessed on the basis of the damages inflicted on individuals and the environment. Our system of justice, of course, requires this should be done for those who were damaged and can be identified, because compensation can and should be paid to them. Moreover, damages to the environment may well affect people in future generations,

³ William D. Nordhaus has long been concerned about climate change. He writes:

- Ad hoc manipulation of a discount rate to achieve long-term goals is a very poor substitute for policies that focus directly on the ultimate objective. Moreover, there is no simple formula for modifying discount rates that incorporate long-term objectives in an efficient manner.
- The dilemma of whether or how much to override conventional market or benefit-cost criteria is not usefully informed by the use of special, low overall or sector-specific discount rates. These merely hide the underlying trade-off between the long-term objective and the economic cost.
- Focusing on ultimate objectives has the advantage of showing trade-offs explicitly, making the cost of violating a benefit-cost rule transparent and allowing public decision-makers to weigh the options rather than having technicians hide the choices in complicated and abstrusely argued second-best rules of thumb (Nordhaus, 1999, p. 158).

⁴ Perhaps Robert C. Lind has given as much attention to the problem of discounting as any contemporary economist. He states:

... I do not believe that a decision such as whether to go forward with a program to mitigate global warming can be made on the basis of a decision model that projects future costs and benefits and discounts them to a present value using any rate of discount. In this regard, I am in agreement with Thomas Schelling that the fundamental choice is whether to transfer resources from the present and near-term future generations to generations living in the distant future that may be better off or worse off, and an exponential discount rate or a market rate of interest is largely irrelevant to making this ethical decision. Further, I believe this decision requires more information than is often contained in a discounted cash flow analysis, such as how rich are current generations compared with future generations? This means, in particular, that I reject the arguments associated with the approaches to choosing a discount rate based on a utilitarian welfare function as set forth in optimal growth models. A corollary to this is that these models and this mode of analysis cannot tell us what the optimal policy with regard to climate mitigation is either (Lind, 1999, p. 174).

For Shelling's position, see: Thomas Schelling (1995), "Intergenerational Discounting," *Energy Policy*, vol. 23, no. 4/5, pp. 379–401 (as cited in Lind, 1999).

⁵ The positions set forth by Nordhaus and Lind are consistent with the writings of Talbot Page on this subject, at least since 1988. See Castle and Berrens (1993) for a similar argument.

as well as those geographically dispersed, yet for practical reasons such individuals cannot be identified individually and cannot be paid compensation. The argument given for estimating the losses such people might suffer was to assess the polluter an equivalent amount.

The beautiful symmetry of equating losses and assessments appealed to many economists. Economists flocked to the controversy of whether contingent valuation was an appropriate technique for estimating passive use values for those, distant in space and time, who may have suffered a loss of well-being as a result of the spill. It was financially rewarding for economists to do so, of course, because there were juicy consulting opportunities on both sides of the dispute. Yet little attention was directed to the logic of equating polluter assessments to possible losses incurred by people dispersed in space and time (for exceptions, see Vatn and Bromley, 1994; Castle, Berrens, and Adams, 1994).

Discussion of this question not only would have directed attention to the appropriateness of benefit-cost procedures here, but also to laws and administrative procedures pertaining to such matters. A relatively new methodology was available, contingent valuation, which held promise for assigning monetary measures to passive use values. The ensuing debate had little to do with the social problem that needed to be addressed, the prevention of future spills. Economics is relevant to the solution of such a problem, but it is not primarily a problem of economic optimality. It requires a value judgment that oil spills are undesirable, and then investigation of the best way of preventing them.⁶

If resource and environmental economists embark on the kinds of policy research relevant to the difficult issues identified here, rather than the endless search for economic optimality, they will encounter a different set of problems. They will find it necessary to work closely with representatives of other disciplines. In this connection, I call special attention to environmental ecology and environmental biology.

There is growing tension between resource and environmental economists on the one hand and environmental biology and ecology on the other. This is not the kind of problem that will be settled in the usual forums of bringing disciplines together. Every thoughtful biologist, ecologist, and economist interested in the natural environment should, I believe,

⁶ These remarks are not intended to be critical of attempts to value non-market goods and services generally or of contingent valuations specifically. The criticism is directed toward the failure to establish a case for doing so for this oil spill.

direct attention to this problem. I do not believe it is in the long-run interest of these disciplines to permit this situation to continue to deteriorate. These disciplines are important contributors to the scientific base of resource environmental policy. Yet many do not agree even on the extent, much less the nature, of environmental problems that exist. This situation would be of less concern if those involved were attempting to resolve their differences by methods of dispute resolution traditional in the scientific community.

The extreme (disgraceful?) reaction of some biological scientists to *The Skeptical Environmentalist* (Lomborg, 1998) in *Nature* appears to be more a manifestation of hostility than a difference of opinion within the scientific community (see *Nature*, November 8, 2001 and May 2, 2001).⁷ Some biologists and ecologists apparently have similar impressions of economists. They are appalled at the notion that economic analysis would be considered an appropriate base for certain environmental decisions. To their mind, economic activity is the cause of the problem, and they want to go “outside the box” for a solution. They are suspicious of solutions which depend on criteria that come from “inside the box,” and appear to believe economists are wedded to the market paradigm with a tenacity that cannot be explained in science alone. They may be as perplexed as we are in regard to an appropriate base for dialogue.

Not all biological scientists react in the way some have to *The Skeptical Environmentalist*. And the recent writings of Nordhaus and Lind demonstrate all economists are not wedded to rigid benefit-cost analysis. There may exist a basis for dialogue among representatives of both disciplines who recognize the need to identify limits to the applicability of every conceptual approach, even our own.⁸

⁷ Prior to the publication of Lomborg's book, *The Skeptical Environmentalist*, the *Economist* ran an essay by Lomborg which offered a summary of this work. Subsequently, the *Economist* reviewed the book highly favorably. This began an extremely heated debate involving the *Economist*, some of Lomborg's chief critics, Paul Ehrlich, E. O. Wilson, Stuart Pimm, and Jeff Harvey; and independent members of the scientific community. For Lomborg's essay and the full text of the position held by the *Economist*, see its publications of August 4, 2001, pp. 63–65, and February 2, 2002, pp. 15–16 and 74–76. For an excerpt of the position held by Wilson et al., as published in *Nature*, see November 8, 2001, pp. 149–150; for the full text, see Wilson et al., “Biodiversity Distortions in Lomborg's *The Skeptical Environmentalist*,” online at www.ucsusa.org. For Ehrlich's position, see *Nature*, May 2, 2002, pp. 21–22. For responses to Lomborg's critics by independent members of the scientific community, see *Nature*, January 24, 2002 (Budiansky, p. 364), and March 28, 2002 (Pielke, pp. 367–368).

⁸ In closing this section, I call attention to a contemporary resource and environmental economist, Daniel Bromley. More than any person I know, he has probed the fundamentals of the questions I have identified here. In doing so, he has drawn upon some of the concepts of the old institutional economists, John R. Commons and Thorstein Bunde Veblen, and modern pragmatic philosophy. The road he has chosen to travel has been a lonely one at times, I am sure. Nevertheless, I believe we are all in his debt.

Land and Economic Growth

As economic growth and change occur, the use of space becomes of greater importance as commercial and residential users compete with each other and with agricultural users for particular locations. Land cannot be viewed only as an item entering individual production and consumption functions, even when allowance is made for externalities. It may also become a tool to achieve collective ends, or to guide and shape economic change. No single conceptual framework has been discovered appropriate for an understanding of the multiple roles played by land under conditions of growth and change.

The theory of the firm provides a framework for an analysis of the role of land in agriculture, when it is assumed agriculture is connected to the remainder of the economy through input and commodity markets. Welfare economics and benefit-cost analysis have provided a comparable framework for the consideration of environmental externalities. As noted in the previous paragraph, the role of land in economic growth becomes exceedingly complex. Both private and public land use issues are involved. Attention must be given to all competing uses of land, and this immediately broadens the scope of analysis. Rural economics becomes more descriptive of the scope of analysis than agricultural or resource economics. Group action at the local level must be considered, and this may require insights from disciplines other than economics.

Central Place Theory

Central place theory provides the principal conceptual lens economists use to understand the geographic implications of private sector activity. Such understanding is of great importance, of course, in public land use policy, even though “optimal” private sector activity is not necessarily an appropriate normative standard for collective decisions. With this orientation, the central city is the beginning point from which all other economic activity stems, and has become the principal orientation of the field of urban economics. In its original formulation, central place theory did not make the hinterlands interesting places, although James Hite (1997, 1999) shows a great deal can be deduced about economic activity located far from the central city core.

No attempt will be made here to do justice to the extensive literature on central place theory and application. The name of Walter Isard (1956) looms large

in this connection. Writing in 1953, T. W. Schultz offered a hypothesis about divergences in economic development related to location (see chapter 9). The key argument was that rural factor markets worked better when there was easy access to the urban industrial complex. And many agricultural economists have worked in this arena, including George Tolley and the late Lee Bawden. There are many contemporary agricultural economists noted for their work within this paradigm, including Tom Johnson and David Kraybill.

Krugman (1991, 1995) modified traditional central place theory by developing concepts that have become a part of what today is known as new growth theory. Hite has demonstrated the new growth theory increases the deductions that can be made from central place theory concerning the hinterlands. Kilkenny (1998a,b, 1999) has done original theoretical work in this arena, and has also made empirical estimations of spatial relationships.

In recent research, Wu (2002) has modified the assumptions typically associated with central place theory. Rather than assume a featureless plane, he permitted amenity values to enter location decisions. This modification suggests there may be different reasons for urban “sprawl” and skip distances than traditional central place theory suggests. Wu and Plantinga (2002) have examined the impacts of open space designation on the urban landscape in a spatial city model with two empirically relevant features: (a) residents prefer to live close to open space, and (b) open space amenities attract migrants to the city. This model indicates open space policies should be viewed neither as independent of, nor necessarily compatible with, growth management goals.

The record of economists in serving public land policy formation and administration is mixed. Economists and lawyers have long recognized use and development rights in land could be separated, and devised policies for permitting this to happen. This permitted individual, as well as collective, objectives to be considered. In my opinion, we have done less well with zoning and similar land use policies. The so-called “Smart Growth” policies have not yet spawned a literature with significant standing in economic doctrine. A concept of “metropolitanism” has been advanced which appears to be at odds factually with descriptive literature about “edge” and “galactic” cities (Katz and Bradley, 1999; Lewis, 1995; Garreau, 1998).

As noted earlier, state and local governments have considerable responsibility and authority over land

use, as does the federal government. Federal and state programs frequently are manifested at the local level in an inconsistent and uncoordinated fashion. When local government can speak for the principal interests in its jurisdiction, it may be able to influence federal and state programs. The effectiveness of local government depends to a considerable extent on whether there are local groups with sufficient common interests and shared norms to advance a collective cause.

The Local Community

As Hausman (1992) notes, the assumptions of economic theory typically are inexact. This does not mean they are false, but rather that they are incomplete. One manifestation of this incompleteness is the neglect of intermediate groups in much economic modeling, as was noted in the previous section of this paper. An examination of local group decision making directs attention to whether the concept of community, as that term is defined in sociology, is of relevance in economics.

Distinctions between rural and urban, metropolitan and nonmetropolitan, and the city and the countryside, are becoming increasingly blurred. Such categories have always been arbitrary, but they may do more harm than good in the contemporary scene as attempts are made to establish collective land use policies. In the analysis of many issues, there is a need to proceed directly to more fundamental variables such as population density, space, and distance. This, in turn, directs attention to the need for local government to coordinate with other units of government both horizontally and vertically. Whether this will happen depends in an important way on the extent to which people recognize they have shared interests with those in another jurisdiction. The traditional local community served a more homogeneous population, in both production and consumption, than is now the case. More recently, special groups have arisen to address particular needs, but often over a different geographic area than was previously the case. An unintended consequence of local land use control measures may be the concentration of lower income rural residents in particular jurisdictions (Fitchen, 1991).

The meshing of individual decision making with collective choices is of interest to sociologists as well as economists. The presidential address of David L. Brown to the Rural Sociological Society, entitled "Migration and Community: Social Networks in a Multilevel World," is reported in the March 2002

issue of *Rural Sociology*. Brown's message is that migration and community concepts have complementary roles in sociology. As a demographer, he reflects the view that the study of migration will not reach its potential unless the conceptual framework used provides both for group objectives as well as individual decision making (p. 3). He attributes the demise of human ecology in the study of migration in part to the inadequate provision it made for individual autonomy and action.

In a paper published in *Rural Sociology* (September 2002), I maintain the interdisciplinary concept of social capital, as formulated by Coleman, is of value in meshing individual expectations with group actions. My proposal is that the total capital available to a community can be classified as social, human, human-created, and natural. All these forms of capital arise from individualistic motivations. Individual title to each can be held in some degree, except for social capital. Because social capital is a public good, individual appropriation of such a good is counterproductive because use by one does not preclude use by another. To make the total capital concept operational, individual and group decision making need to be integrated at the local level. The total rural capital concept requires that differences in the various forms of capital be recognized, but that no one form be emphasized to the exclusion of the others.

The land economists correctly surmised it is not realistic to consider land independent of social arrangements for its use and control. Economic models which proceed directly from individuals and firms at the micro level to the macro level are unable to accommodate such realities. Central place theory has long made it clear that factor combinations will vary with distance. Yet central place theory does not provide a theoretical basis for group as well as individual decision making.

There are significant implications in both economics and sociology if the concept of social capital is interpreted as I propose. In economics, it means social capital must meet certain requirements if it is to qualify as capital. In sociology, it means the modifier *social* cannot acquire meaning which is not relevant to that which it modifies, *capital*. Specifically, this means social capital cannot be regarded as a social theory, or as encompassing particular normative goals.

But does *social capital* qualify as capital? A recent article in the *Journal of Economic Literature* by Sobel (March 2002) provides an excellent discussion of this question. Clearly, many forms of

social capital require sacrifice and permit investment. As Coleman (1990) advanced the concept, social capital arises when group action permits individuals to better achieve their aspirations than if they act independently. To be sure, social capital is in the nature of a public good, but that is also true of certain forms of natural capital. The demand for social capital is a derived demand; it does not have an explicit price, but it may be valued implicitly in an economic system.

As is the case for any form of capital, useful discussion requires consideration be given to specific, as well as general, types of social capital. In Coleman's estimation, the family and community were the primary forms of social capital. He asserted the incentives for social capital formation were weak, and that it arose as a by-product of other activities. The early empirical work on social capital by Putnam (1995) emphasized civic or social organizations that did "good works" in addition to socializing functions. Woolcock (1997), however, generalized the concept greatly and discussed social capital formation across, as well as within, groups. Across, or inter-, group social capital relations tend to be more formal and impersonal. Viewed in this way, much of institutional economics in one way or another overlaps with, or can be considered, social capital.

Consistent with this view, social capital arrangements would include: a partnership involving no more than two people, a cooperative with large numbers, a community-based economic development group, as well as a cartel. Many social capital arrangements are in the nature of public goods in that additional use would not preclude existing uses, although, in some cases, additional uses or users might decrease the value of existing use. In such circumstances, entry may be controlled, and excludability be practiced.

The great advantage of the social capital concept from the standpoint of rural economics is that it permits intermediate level decision making to become endogenous in economic analysis. Even though geographic interdependence exists in more developed societies, geography also conveys a degree of uniqueness to each local area. The control of (say) 40% of the land of rural community is not likely to be of significance regionally or nationally, but such an ownership or control pattern may be exceedingly significant in affecting opportunities within such a community.

Knowledge of social capital arrangements may be necessary to (say) understanding the income patterns, migration, or history of rural places. The

literature on collective choice and the theory of cooperation may be of considerable relevance in rural economics. Recent articles by Daniel Bromley and Gordon Rausser (ed. Dinar, 2000) demonstrate rigorously how individual aspirations are best achieved by cooperation when interdependence in the use of a natural resource exists. Traditional economic efficiency measures associated with decentralized markets will not be descriptive of reality under such circumstances. Economic models that abstract from such realities will not explain outcomes, or serve prescriptive purposes well.

In summary, the social capital concept assists local community understanding by (a) integrating individual and group actions in intermediate decision making; (b) directing explicit attention to the relation of past, present, and future conditions because of the durability of capital; and (c) permitting all forms of capital—human, social, natural, and human-created—to be integrated in a single system.

Even though group cooperation at the local level has long been a characteristic of farm communities, recent developments in agricultural industry appear to have weakened local ties and encouraged connections along commodity or other special interest lines. Although many, including some agricultural economists, accept the myth that "agriculture" and "rural" are synonymous terms, in my opinion farmers have a great deal to lose from such an attitude. In most rural places, farmers are in a minority even within their own communities; nationally, more farm family income is derived from off-farm sources than on-farm sources.

There is more to be said for viewing agriculture from a rural community perspective than there is for viewing the rural community from an agricultural perspective. The implications of doing so are several. For example, community developments would be evaluated as to the amount and types of off-farm employment which would affect farm family income. Once this step is taken, agricultural interests will come to adopt the perspective of individuals and businesses that may locate in the community. Practical people and programs may be ahead of the agricultural economics literature on such issues, although the work of the sociologist, Woolcock (1997), is of great relevance.

Rural Economics—A Pluralistic Specialization

The previous discussion indicates rural economics requires different approaches from within economics. Central place theory makes use of the traditional

assumptions of classical and neoclassical economics. Institutional considerations are also relevant. Game theory and imperfect competition have application as well as collective choice. More than either production economics or resource economics, rural economics is pluralistic.

Rural economics is one discipline within the multidisciplinary field of rural studies. The social problems of the countryside continue as areas of concern. These problems include poverty, welfare reform, rural schools, rural health services, land use planning, and rural resources and the environment. Rural studies will develop best if research, education, and extension efforts all are forthcoming. Although funding has been difficult to come by, conditions may be changing. Much will depend on whether agricultural interests will continue to view rural studies as a threat, or as complementary, to a healthy agriculture. It must be admitted, however, the 2002 Farm Bill is not encouraging in this respect.

How is rural economics viewed by the agricultural economics profession, the land grant system, and by the USDA research and educational establishment? Clearly a great deal of encouragement has been provided to this time. The American Agricultural Economics Association has a section on community economics. The USDA has a distinguished record of accomplishment in documenting conditions and economic change in rural places. As one who was present during the formative years of both production economics and resource economics, I believe rural economics now compares favorably with those fields on the basis of critical mass of researchers, problem importance, and conceptual framework. Nevertheless, it is important that the distinctions among these fields be kept in mind when comparisons and evaluations are made. At least for the foreseeable future, rural economics will be more pluralistic. This provides a source of vigor and breadth not only to agricultural economics, but to the land grant-USDA complex as well.

Summary and Conclusions

This paper maintains changing economic conditions and varying perspectives of economic analysts have resulted in great differences in the way land is considered in economic doctrine. Three contexts have been analyzed. The first considered agricultural land use where agriculture was connected to the rest of the economy exclusively through input and commodity markets, and when all other parts of the economy were assumed to remain constant. The

second connected agriculture to the remainder of the economy by virtue of a shared natural environment. This facilitated a discussion of natural resource and environmental economics in relation to agricultural economics, institutional economics, and land economics. In the third context, economic change in the entire economy was permitted with particular attention given to population density, space, and distance. The topic of private and public decision making also was discussed with explicit attention given to federal, state, and local division of powers.

As we look to the future, applied economists concerned with land will need to give special attention to the particular circumstances and to the social problems driving their inquiry. This may limit the scope of economic models as implied by the theoretical core of economics, and may require the participation of non-economists as well. Such tendencies should not necessarily be resisted. Land has long been a complicating factor in economic analysis, but it is important that its role be understood in the context of actual situations and the social problem of concern at the time. When viewed in this way, it can become a source of breadth and depth for both economists and economic doctrine.

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Appendix: Intellectual Constructs and the Limits of their Applicability: A Digression

Richard Just noted in his 2001 Fellows address to the American Agricultural Economics Association that the analysis of historical data by even the most sophisticated techniques may be of little help in current decision making when public policy, or technology, changes rapidly. Just surely is correct in noting the ability to explain historical data does not necessarily mean an economic model is applicable currently.

There is less basis for associating such an approach with the philosopher Karl Popper (1959). Popper believed in risky conjectures which were capable of being falsified. If the objective is to understand current affairs, it is doubtful Popper would believe econometric models applied to historical economic data would constitute such a test. It may well be that few theories are capable of being falsified as Popper envisioned they should, or could, be. But it is doubtful economists should blame Popper for our preoccupation with "tests" based on historical data. Such econometric work should be evaluated on the basis of whether it contributes to improved understanding of the economic history of whatever period is being analyzed, rather than constituting a "test" of an economic theory.

Economists should not blame Popper for their claim that their work was consistent with his definition of science, and thereby create the impression economists were "scientific" as a consequence. Although Popper spoke favorably of the use of mathematics in economics, there is little evidence to suggest he ever made a judgment that the econometric evaluation of historical data was consistent with his definition of science (De Marchi, 1988). Popper even wrote a book (*The Poverty of Historicism*, 1957) on the limitations of history in providing evidence for scientific "tests."

How, then, should it be decided if, or when, a theory or conceptual model has reached the limits of its applicability? Rather than relying on comparisons of empirical estimates of different conceptual approaches, skillful applied economists, and thoughtful practical people, apparently evaluate the output of economic models with various plausibility tests. Does the model yield results consistent with information that has proven reliable under other circumstances? To be sure, the "other circumstances" may reflect historical conditions the analyst may be trying to escape, but the experienced analyst may make subjective allowance for how "other circumstances" vary among situations. The ultimate test, of course, is whether an economic model works well in practice, and this often means how well it helps anticipate future events. The real-world application of economic models to practical affairs is indeed a hazardous activity. Errors are often costly, not only to those making the application, but to others in society as well.

In economics there are few substitutes for real-world tests, experimental economics notwithstanding. There may be instances when it is possible to subject theoretical conjectures to actual experience, in addition to comparing predictions with the results of designed experiments.

McClosky (1985, see especially chapters 4 and 5) has noted theoretical approaches typically depend on fundamental propositions that are matters of faith which have not been subjected to empirical verification. Such "faith-based" propositions may be accepted implicitly by followers of a particular approach, even though they may be quite conscious of unstated propositions when they encounter an approach "outside their box." In such cases, they may demand documentation of implicit propositions of unfamiliar approaches, even as they resent a comparable request by others.

It is constructive to consider the recent experience of Long-Term Capital Management (LTCM), an investment firm that relied heavily on markets involving arbitrage, options, and derivatives (Lowenstein, 2000). The partners of LTCM operated as though there were no limits to the reach and power of markets. In economics, their motivation would be described as profit maximization; "greed" would more commonly be used in ordinary discourse.

Among LTCM partners were Nobel Laureates in economics, Robert C. Merton and Myron C. Scholes, who had developed theories pertaining to the marketing

of derivatives. Several major financial institutions provided LTCM credit—but only for a while. Even though market theory may yield reliable predictions over time, or for particular circumstances, such a theory may not do well for all circumstances or for a particular time. Apparently LTCM failed to make proper allowance for these imperfections, and credit constraints eventually brought them down. Yet this did not occur before they had thrown a major scare into global financial markets. Alan Greenspan facilitated a meeting of creditors, who then decided to pull the plug from the bathtub of credit LTCM had been enjoying. The demise of the firm occurred before major damage was done to global financial markets.

There are parallels between the LTCM experience and the recent collapse of the Enron Corporation. In the case of Enron, however, collapse did not occur before significant pain and suffering were inflicted on many who were not responsible. Regulated utilities provided the original base for the Enron Corporation. Deregulation provided incentive for mergers and created markets for energy, and property instruments making claims to energy. Enron engaged in energy marketing with gusto, and then expanded its trading activities to many other items. So far as I know, a complete and generally accepted explanation for the collapse of Enron does not yet exist, although numerous factors apparently contributed. Practical people, both inside and outside the corporation, became concerned about the movement of Enron debt to subsidiary corporations which distorted the Enron balance sheets and income statements. When Enron's stock prices began to decline, a "run on the bank" occurred.

In both cases, there seems to have been an implicit faith in the reach and power of markets, fueled by enormous

greed. In the case of LTCM, historical data had been analyzed but extensive experience with arbitrage operations involving derivatives and options apparently did not exist. In both cases, great reliance was placed on conceptual models that had not been used extensively under actual circumstances. Hopefully, it was coincidence that LTCM had two Nobel Laureates in economics as partners, and that a PhD economist was Chairman of the Board at Enron, with a Harvard MBA as CEO. In each of these instances, there appeared to be an implicit faith in the capacity of the market to discover and internalize all relevant information.

As noted, practical people, inside and outside the Enron Corporation, triggered its decline. In the case of Long-Term Capital Management, two practical investors, Warren Buffet and George Soros, declined to participate, although they were invited to do so. Concerning LTCM, Soros is quoted as saying: "The idea that you have a bell shaped curve is false. You have outlying phenomena that you can't anticipate on the basis of previous experience" (Lowenstein, 2000, p. 149). Clearly, Soros has criteria for judging the limits of applicability of conceptual models.

It is unlikely a single, grand economic model will ever permit an analysis of all land-related economic events. Debates about the relative merits of various conceptual approaches make for interesting meetings of economists, but often fail to reflect the reality these concepts probably were advanced to illuminate different parts of reality in the first place. We are fortunate the literature provides different conceptual treatments of land, because a pluralistic approach permits us to gather information and develop insights that would be excluded by a single approach.