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THE CASE FOR METHODOLOGICAL PLURALISM

RICHARD B. NORGAARD

Department of Energy and Resources and Agricultural and Resource Economics, Rm. 100, Bldg. T-4, University of California, Berkeley, CA 94720 (U.S.A.)

(Accepted for publication August 5, 1988)

ABSTRACT

Norgaard, R.B., 1989. The case for methodological pluralism. *Ecol. Econ.*, 1: 37–57.

Ecology and economics share the same Greek root. Both address complex systems. Ecology consists of numerous approaches to understanding natural systems: energetics, population biology, food-web models, hierarchy theory to mention just a few. Within ecology, field knowledge and the reporting of new observations are well respected. Economics, on the other hand, is dominated by one pattern of thinking and standard of "proof", the market model and econometrics. Within economics, field knowledge and observations *per se* are little valued. Agreement on a correct method is frequently taken as an indication of the maturity of a science. The argument is developed in this paper that all the aspects of complex systems can only be understood through multiple methodologies. The agreement on method within economics, however, seems to reflect stronger pressures within the discipline for conformity than for truth relative to ecology. Since ecological economics seeks to understand a larger system than either economics or ecology seeks to understand, a diversity of methodologies is appropriate and pressures to eliminate methodologies for the sake of conformity should be avoided.

INTRODUCTION

Ecological economics, both in this journal and as a discipline, will evolve over the coming decades from whatever we—the community of scholars and practitioners dedicated to its development—bring to it. We are already committed to starting with both ecological and economic "genetic" material. There is considerable diversity in each of these fields. We understand ecosystems through models of population dynamics, nutrient webs, energetics, foraging and reproduction strategies, and coevolution, among others. We understand economies through political economy, market, institutional, input–output, accounting, monetary, and Keynesian models. In addition,

0921-8009/89/$03.50

ecologists can provide links to the other natural sciences while economists can provide links to the other social sciences. Ecological economics is starting from a very broad evolutionary base.

From this base, the evolutionary course of ecological economics will depend on what "proves" fit. We have, of course, no end of economic and ecological problems on which to work. Fitness might eventually be shown through the successful application of our thinking to problems. Testing through application, however, will never be definitive because of the difficulties of controlling for many variables, because hypotheses can only be disproved (Popper, 1959), and because the superiority of new world views and their respective models cannot be determined (Quine, 1953; Feyerabend, 1974). Thus fitness will be determined over the coming decades by: (1) the breadth and depth of our own understanding of good method; and (2) the intellectual environment we create to sort the good from the bad.

Fortunately, ecology and economics offer a rich history of methodological approaches. In spite of the current neoclassical hegemony in economics and the excesses of mathematics in ecology, we can draw upon diverse ways of knowing as we participate in the evolution of ecological economics. Unfortunately, however, both ecologists and economists have historically been under the illusion that there is a right way of pursuing questions which will lead to right answers. This belief has been reinforced by a broader faith in the West, the idea of control or the belief that people, both individually and collectively, can precisely predict the consequences of alternative decisions. In opposition to this long-standing belief in a right way of knowing and precise prediction, my objective for this paper is to present the case for a conscious maintenance of methodological diversity and cultural adaptation to working with a range of answers. The general arguments I will present are applicable to all of science. They are, however, especially appropriate for an emerging field which combines two earlier traditions.

In the first section of this paper, I discuss the similarities and differences between economics and ecology and present an initial argument for retaining the full range of methodologies available in both disciplines rather than merely the approaches they hold in common. Key characteristics of the currently dominant methodology of science and its relation to social action, logical positivism, are described in section II. I develop a taxonomic framework for identifying different methodologies in section III. In section IV, I describe how different patterns and problems of thought in economics and ecology illustrate how economists and ecologists have adopted the different methodologies identified in the conceptual framework. In section V, I document the perils of not having a broad methodological base. I conclude with summary arguments in section VI for methodological pluralism.

I. ESSENCE, CHANGE, AND METHODOLOGY

Ecologists are fundamentally more dubious about "progress" than are economists. This difference has become increasingly accentuated both as materialism continues to displace broader Western ideals and as technologies—that we rarely publicly, consciously choose—reduce cultural and natural systems to crass caricatures of their original fullness. That to which economists point with pride, ecologists point with dismay (Luten, 1980; Ehrlich, 1981).

Yet, economics and ecology share the same Greek root, *oikos*. Furthermore, economists and ecologists both explore complex systems in a manner sufficiently similar that there have been important conceptual transfers. Darwin and Wallace credit Malthus with alerting them to the dynamics of a population meeting a resource constraint. The mathematical models of population biology and the patterns of explanation used to account for foraging and reproductive strategies are the same as those of economics. The similarities are well documented (Boulding, 1966; Rapport and Turner, 1977; Dunbar, 1984; Stephens and Krebs, 1986).

Some economists apply their knowledge to ecosystem protection and some ecologists apply their knowledge to economic development. Nevertheless, the two disciplines are the scientific components of divergent world views. Economic and ecological arguments are invoked by people with different values, different interpretations of the nature of systems, and, consequently, different images of how people should relate to their environment. Thus while the theoretical similarities are intriguing, whatever new world view might get us around the current stalemate and guide us through the coming century is unlikely to evolve from the theoretical intersections alone *.

The Aristotelian notion that things fall into categories because they have unique essences is commonly held. The terms "economist" and "ecologist" call forth distinct images. Economists Milton Friedman and Paul Samuelson have fought over economic interpretation for years, but certainly their views on questions of environment and development are closer to each other than they are to those of ecologists Paul Ehrlich and Daniel Janzen, who also have their differences. Each discipline must have a special essence by which it can be definitively sorted into types. But, like the problem of defining

* Environmental economists tend to deny these differences (see, for example, Randall, 1986), ecologists use economic arguments strategically (Myers, 1983), and the World Bank is currently trying to incorporate environmental concerns within the economic paradigm that patterns their decision-making process (Warford, 1986). Nevertheless, I remain convinced that the world views are very different (Norgaard, 1985).

species in biology, every taxonomic rule denies the differentiation and happenstance that explain the evolution and speciation of ideas in the disciplines *. This logical paradox is critical to the development of my argument.

The market model is the dominant paradigm among North American and European economists. This model links individuals—as suppliers of labor, capital, and land and as demanders of products and services—through numerous markets. Economists have steadily developed the model over the past century through more refined mathematical treatment. Parameters are also now estimated through increasingly sophisticated econometric analyses of generally better and better data. The steady progress has led to broad acceptance among a growing and powerful profession. Many economists are convinced that it provides profound insight into questions of markets and economic efficiency and hence much of economic policy (Schultze, 1982; Hirschleifer, 1985; Nelson, 1987).

Critics, on the other hand, are dismayed by the simplicity of the model's assumptions and the fact that mathematical elaboration and statistical estimation have not resulted in an accumulation of usable knowledge. In fact economics consists of logical arguments that can more or less tell any story desired. The downward slope of demand curves is the only thing that approaches a law. There are neither relationships, other than the downward slope of demand curves, nor constants which have been shown to be universal. Nor do economists test for these (McNown, 1986). Thus economics has much in common with adaptionist "stories" in evolutionary ecology (Gould and Lewontin, 1978; Levins and Lewontin, 1985).

While a few economists have become concerned that they do not practice their methodological beliefs, the debate is almost evenly divided between those who think they can and should (Blaug, 1980) and those who think their methodological beliefs have no hope in practice. The latter argue that economists need to understand the strengths and weaknesses of the methodologies they actually use and work toward an epistemology appropriate to the nature of investigating complex systems (McCloskey, 1985; Johnson, 1986).

Economists with a much more historical leaning competed successfully with the simple use of the market model into the early part of the 20th Century (Knight, 1951; Pribram, 1983). Historical, institutional, or marxist economists still dominate in a few schools in Europe and the United States. In addition to this diversity in views, even those who hold to the dominant model have harboured various non-market models to explain the aggregate levels of output, employment, and inflation. Thus, though economics has a dominant paradigm, it is not monolithic. The patterns of economic thinking and the methodologies associated with those patterns have varied over time, across regions, on different problems, and by schools of thought.

The development of methodological beliefs in ecology are more difficult to trace than in economics. Evolutionary theory has stimulated clearly defined epistemological debates (Greene, 1981). Other fields of biology have not. Mayr (1982, p. 113) argues:

> Some historians of science like to distinguish different periods, each with a single dominant paradigm (Kuhn), episteme (Foucault), or research tradition. This interpretation does not fit the situation in biology. Ever since the later seventeenth century, one finds more and more often that even within a given biological discipline or specialization, two seemingly incompatible paradigms may exist side by side, like preformation and epigenesis, mechanism and vitalism, iatrophysics and iatrochemistry, deism and natural theology, or catastrophism and uniformitarianism, to mention only a few of the numerous polarities.

Discerning patterns and critical episodes in the development of ecological methodology is also hampered by the relative newness of the discipline. Ecological thought goes back to the ancients, natural historians increasingly observed the interactions of species and environmental features, but the term ecology was first proposed by Haeckel in 1866 while the term ecosystem was coined by Tansley only in 1935. The development of ecology as a body of thought and of ecology as a discipline is mostly a 20th Century phenomena (Glacken, 1977; Worster, 1977; Mayr, 1982). In addition, the boundaries between the biological disciplines are not so well demarcated by professional association and practice as are those of the social sciences. As a consequence, what passes as acceptable methodology in ecology has been influenced by all of biology. A methodological literature distinct to ecology is only now developing (Levins, 1966; Mayr, 1982; Allen and Starr, 1982; Salt, 1984; Sober, 1984a, b; Levins and Lewontin, 1985; Taylor, 1987).

Whereas periods of methodological consciousness can entail tightening and enforcement, the present surge in epistemological pondering appears to be opening up and rejuvenating the methodological bases of both economics and ecology. Through a broader and more open base, new approaches can flow. If we limit our understanding to the methodologies of each discipline that happen to dominate today, or worse merely the beliefs they hold in common, we will miss the diversity that provides the base for an evolutionary response to the new conditions we are facing.

II. LOGICAL POSITIVISM

Logical positivism has been the dominant methodology of science for several centuries and has formed the basis for most of the relations between

* Mayr (1982) structures much of his book on the history of biological thought around the antimony generated by our understandings of essence and our understandings of change.

science and society in the West and the modern portions of developing countries. Since other methodologies are best understood with respect to how they deviate from the dominant notion of science and its relation to action, logical positivism deserves detailed elaboration.

Enlightenment philosophers sought to free men from unreason, from the idols of the mind, from all that kept them from knowing and acting upon pure, universal truth. Philosophers and scientists flowed rather freely from the discoveries and methods of the natural sciences into social inquiry for three reasons. First, there were no divisions between philosophy, religion, natural science, and social science during the enlightenment. Second, "unreason" with respect to things social appeared so plentiful that the temptation could not be avoided. Third, the new knowledge of the physical sciences could only be the engine of progress if society abandoned "irrational" traditions. Through this cross-over, social inquiry, indeed all inquiry, received a strong influx of beliefs with respect to objectivity and universal laws that dominated its character during the 19th Century and heavily influences it today.

Western science has sought to know the universal, unchanging characteristics "behind" a changing reality. In the dominant view today, the various disciplines of science are leading toward one consistent set of laws about the nature of all things. To be sure, most of the disciplines are isolated islands now, but the physical sciences and microbiological sciences already join in some places. Other parts of islands can at least be temporarily bridged with sufficient interdisciplinary effort. And on the whole, as scientists continue their inquiries, the sea of ignorance will recede until it is perhaps entirely gone. Models mirror reality, and falsification, the universal method of science, will ultimately assure the unity by eliminating false reflections. Existing disciplines, or at least one's own, are islands rather than entangled kelp adrift in the currents. Belief in the unity of knowledge is consistent with progress in the physical sciences until this century and in much of microbiology since.

A presumed positive knowledge of how things actually were led science to become a powerful, separate authority that often countered the church and state. Perpetual conflict was avoided through a division of responsibility. Religion assumed the role of questioning and transferring values; science questioned and transferred knowledge about how things were; and the state and the economy served a functional role in between, linking the desirable to the possible (Unger, 1975; Hirschman, 1977). Belief in both the link between science and progress and in the objectivity of science, the waning role of the church, and the difficulty of democratic decision-making in a technically complex world led to a new alliance between science and the state. The progressive movement—a derivative of Plato's philosopher kings, envisioned by Francis Bacon, and expounded by August Comte—had politically neutral scientists making decisions on the public's behalf under the broad guidance of elected officials. Though the progressive movement peaked during the 1920s and 1930s, it is still well embedded in our governance today (Torgerson, 1986; Maxwell and Randall, 1987).

Logical positivism has been sufficiently widely believed by scientists and laymen to become generally incorporated in how we organize and operate. The organization of the scientific establishment, the organization of science within individual public agencies, and the role of scientific information in the policy-making process stem from logical positivism *. The development–environment debate of the past quarter century is widely recognized as one of the major erosive forces of the progressive alliance. Scientists with quite different types of knowledge, values, and images of the future have broken from their factual, instrumental roles and directly advocated different policies. Though advocacy science is now widely practiced, it still does not have a supporting public philosophy (Primack and von Hippel, 1974; Schrader-Freschette, 1985).

Thus the environment–development stalemate is not simply a scientific problem with respect to the inadequacy and separateness of economic and ecological thinking. The methodological beliefs that supported and influenced inadequate, separate scientific inquiry also patterned social organization and processes. Since past beliefs about science contributed to social and scientific organization and helped guide us along the course of history, our concern with that course must be, in part, a concern with methodological beliefs. In the next section I develop a taxonomy of methodological beliefs to facilitate further elaboration.

III. A METHODOLOGICAL TAXONOMY

The following taxonomy takes four key assumptions of logical positivism and classifies other methodologies with respect to whether they make the same assumptions or not. This simple four category, either/or division results in sixteen combinations. Some of these combinations are readily observed in the history of economic and ecological thinking, while examples are scarce for others. The combinations are based on whether the methodology: (a) assumes methods of understanding reality are independent of

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* This theme is well documented from many perspectives (Merchant, 1983; Berman, 1984; Prigogine and Stengers, 1984). I have presented the causal relationship here and in the subsequent paragraph as one of methodological beliefs patterning social relations. The argument can just as well be made the other way and is by Merchant and Berman. I prefer coevolutionary explanations, each patterning each other (Norgaard, 1984) but have settled for simple cause and effect for most of this paper.

culture; (b) assumes reality is independent of methods of understanding; (c) assumes reality can be understood in terms of universal laws; and (d) assumes reality can be understood through one set of universal laws.

Logical positivism makes all four assumptions. The first assumption is the essence of positivism, the idea that objective knowledge is neutral with respect to the beliefs and values of the people who desire and produce the knowledge. The second assumption makes the existence and use of knowledge neutral. This assumption is coupled to the third. The natures of things, both natural and social, are unchanging, hence logical positivists can believe there are universal laws about their nature and using these laws does not change their nature. Reality may assume different states, like alternative configurations of the planets, but reality, like the celestial mechanics of the planetary system, is unchanging and hence can be described with universal laws that are unaffected by human action on the natural world. Lastly, the laws through which we understand reality are believed to form, or to eventually form, a single, consistent set.

These four aspects of logical positivism clearly fall on the cosmology, or frame of the universe, side of methodology. On the other side, closer to the practice of science, lies the once again knotty problem of induction. I have little to say about this problem because there do not seem to be important differences in beliefs and practices among and between economists and ecologists. Nevertheless, the reemergence of the problem of induction—after decades of faith in unpracticed falsification—is very important to the weakening in the belief in logical positivism.

Logical positivism is a cluster of the above and additional beliefs. Most scientists, including economists and ecologists, implicitly hold beliefs close to this cluster or, in their collective enterprise, behave as if they hold beliefs in this cluster. In fact, few scientists study methodology or make their beliefs explicit. Individual scientists, and eventually whole disciplines, succeed by being pragmatic *. Thus it is not unusual to find individual scientists explicitly espousing and implicitly practicing conflicting methodologies. Yet at any time there are schools or patterns of thought which have explicit, or at least identifiable, methodological implications. The methodological richness of economics and ecology is identified in the next section by surveying how each discipline has approached problems which do not fit the assumptions of logical positivism.

* Quine (1953) effectively pointed out that every argument or model commits its user to the existence of posited components and relations. Different arguments or models entail different, incomparable commitments (MacIntyre, 1967; Presley, 1967). The fact that scientists collectively enforcing methodological purity, or the commitment to one model and method upon themselves had mixed results for the progress of science was first effectively argued by Kuhn (1962). The logical impossibility of scientific innovation under methodological purity has been strongly argued by Feyerabend (1975). The pragmatism, or common sense, of science has been presented by Toulmin (1972) and, in a more popular version, by Bronowski (1978).

IV. METHODOLOGIES IN ECONOMICS AND ECOLOGY

Early economists idolized Newton. Present-day mathematical economics emulates his mechanics. Economists have assumed the epistemological beliefs of 19th Century physics. Even marxists, infamous for pointing out how orthodox economic thinking is a product of capitalism, borrow from the language of physics and believe their own theory of history is based on universal laws. Institutionalists have also sought comprehensive, positive theory. Thus even amongst the diversity of economists using different models, the dominant methodological beliefs cluster around logical positivism.

There was, however, an important exception. The German historical school contended that everything social was conditioned by history, all history was conditioned by human values and action, and it differed from place to place. Many late 19th Century American and European economists were trained in this tradition in Germany (Pribram, 1983; Baumol, 1985). At the turn of the century, a debate among German social scientists known as the "Methodenstreit" pitted those who believed the social sciences should have their own methodology which acknowledged the influence of values on history and on interpretation against those who argued for the adoption of the positivist, value-free methodology of the physical sciences (Pribram, 1983, pp. 228–230; Hekman, 1986, pp. 22–25). The positivists "won" and until recently the use of methodologies which incorporated changes in history, culture, and values within the methodology has only been something of which others might be accused. Nevertheless, much of the diversity in economic thought and methodology can still be traced to this period.

The diversity of methodologies in ecology has several roots. As noted earlier, ecology draws upon explanations from all fields of biology, accepting the methodologies implicit to those explanations. The long tradition of direct observation in the field also supports an eclectic approach in ecology. Field research builds a very different type of understanding of systems than does the pursuit of the nature of systems dynamics through mathematical exploration. In the following subsections, I note how the methodological diversity in both disciplines relates to the taxonomy of methodological beliefs.

Methodological dependence on culture

Though marxists have long accused the methodology of market economists to be a product of their culture, all—whether marxist, neoclassical, or institutional—have sought culture and value-free explanations. The mode of explanation does not change even when economists are working with non-Western cultures.

Ecologists, on the other hand, acknowledge how culture affects method. Ecologists who work on the biological control of agricultural pests have clearly recognized how the methodologies of those who design chemical control strategies are influenced by modern, scientific culture (Van den Bosch, 1978; Perkins, 1981). Agroecologists are beginning to adopt the methodologies of anthropologists in order to interpret the beliefs of traditional peoples to help discover agroecological relationships. In the process, they acknowledge the cultural nature of their own approach (Norgaard, 1987; Dover and Talbot, 1987). Ecologists concerned with biological reserve management in developing countries are learning how to "explain" the objectives of reserve status and work with indigenous peoples based on indigenous ways of understanding (McNeely and Pitt, 1985). Participants in the new field of conservation biology are very openly allowing their cultural values to guide the field's development. They are very cognizant of how the weaknesses in biological theory are a result, in part, of the historical development of science and technology without the assertion of values by practitioners (Ehrenfeld, 1978; Soule, 1983). Lastly, some ecologists speak out in the public arena fully cognizant of how their values are embedded in their approach *.

Dependence of reality on methodology

Clearly, the economic world has been heavily influenced by economic thinking. Indeed, in the final analysis, the economic policies around which economists agree consist in making the economy more like the model. The transformations in the economy attributable to economic thinking are recognized by marxists and institutional economists (Hirsh, 1978). The situation is similar with respect to agroecosystems. Ecologists who understand biological control systems are fully aware of how non-ecological thinking amongst agricultural scientists has resulted in a massive transformation of the agricultural environment (Van den Bosch, 1978; Ellenburg, 1979; Perkins, 1981). Agroecologists also recognize that traditional agricultural systems are products of the beliefs of the culture. Microorganisms through insects have evolved characteristics in response to the selective pressures of human intervention in plant management (Altieri, 1987).

Knowledge is universal or useless

Neoclassical economists have sought universal laws, refer to the "laws" of supply and demand, and pattern their model on classical physics. Though nothing even approaching universal other than the downward slope of demand curves has been found, they continue to believe that universal policy recommendations can be drawn from economics. Institutionalists, on the other hand, tend to argue that knowledge is specific to the situation (Wilbur and Harrison, 1978). Most ecologists would also like to be able to make generalizable statements about ecological systems and their management but have become increasingly pragmatic. Roughgarden (1983, p. 17; see also Oster and Wilson, 1978, ch. 8; and Smith, 1978) argues:

> There is antagonism among many ecologists toward theory, and some of it arises, I suspect, from the fear that ecological theory is considered the "foundation" of ecology. Some sciences, like physics, are hierarchical, and physicists speak of theoretical axioms, laws, and of "truths" that have been derived from such theory. In a hierarchical field, it is conceivable that a mis-directed theory could divert the entire field away from a common-sense evaluation of its own empirical findings; if so this is a legitimate fear. Ecology does not have such a hierarchy now, I doubt if it ever will, and hope it never does. It is difficult to imagine what could ever qualify as a "law" in ecology. Ecological theory is no more than a collection of tools.

Roughgarden's methodological arguments have stirred others with "higher" hopes for ecology to respond that the profession is, or at least should be, systematically seeking universal laws *. Ecologists clearly hold different opinions with respect to the possibilities of universal laws and the meaning of science without universal laws.

On the unity of knowledge

Lastly, neoclassical economists increasingly seem to hold the view that economic thinking will eventually weld together with thinking in other disciplines to form a coherent understanding of the world. This tendency is well illustrated in Hirschleifer's (1985) "The Expanding Domain of Economics" wherein he documents how the neoclassical model is now used to explain history, politics, and sociology. He closes the essay with the follow-

* Of course, many ecologists, like economists, speak out because they think it is imperative that their objective knowledge be known, while others decry that the objective sanctity of science is at stake every time an ecologist sounds the alarm (Singer, 1987).

* Responses to Roughgarden can be found in the issue of the journal as well as in the book in which Roughgarden's paper is available. Kingsland (1985) presents a history of the debates over mathematical modeling in population biology.

ing expression of faith in the eventual unity of science (1985, p. 68):

> I must conclude very briefly, in pursuing their respective imperialist destinies, economics and sociobiology have arrived in different ways at what is ultimately the same master pattern of social theory—one into which the phenomena studied by the various social sciences to some extent already have been, and ultimately will all be, fitted.

This view, however, is relatively recent. Economists historically have had a strong sense that their models have limited applicability beyond explaining markets. Weber argues (1984, p. 109):

> Accordingly, the fantastic claim has occasionally been made for economic theories—e.g. the abstract theories of price, interest, rent, etc.—that they can, by ostensibly following the analogy of physical science propositions, be validly applied to the derivation of quantitatively stated conclusions from given real premises, since given the ends, economic behavior with respect to means is unambiguously "determined". This claim fails to observe that in order to be able to reach this result even in the simplest case, the totality of the existing historical reality including every one of its causal relationships must be assumed as "given" and presupposed as known. But if this type of knowledge were accessible to the finite mind of man, abstract theory would have no cognitive value whatsoever.

Knight (1956, p. 26) reaches a similar conclusion by a less tortuous route:

> Hence it will be evident that the other methods or approaches to economic data, notable historical research and statistical investigation, are not to be thought of as substitutes for sound theory, along the traditional lines, but as complementary to it. This is true also of social sciences other that history and statistics, notably psychology, with or without such qualifiers as social, political, analytic, etc. All are needed to supply data and interpretation, to put content and definiteness into the valid but highly abstract "laws" of economic choice and market phenomena. Without such supplementation, economic laws have little value for prediction, since the essential factor of wants is not open to sense observation and any course of events that occurs can be fitted into the theoretical pattern.

Sir John Hicks (1979, p. 12) gets to the same conclusions most concisely.

> It is because the phenomena with which economics deals is so narrow that economists are continually butting their heads against its boundaries.

Institutional economists tend to retain this understanding of how economics relates to the other social sciences. History, politics, and culture are the raw ingredients of their economic explanations rather than challenges to be explained by economics.

Ecologists have also expanded their theories to explain larger phenomena, perhaps even more aggressively than economists (Ehrlich, 1968; Odum, 1971; Meadows et al., 1972). At the same time, ecology consists of diverse, incongruous theories about population dynamics, energetics, food webs, coevolution, communities, succession, etc. Ecologists are accustomed to explaining the dynamics of temperate forests in terms of succession and of tropical rainforests in terms of light patches. The idea that evolution has proceeded in different ways at different times and places precludes universal principles at the organismal level, let alone the ecosystem level. An interpretive, or hermeneutic, methodology that acknowledges the array of explanations needed to understand different aspects of very different systems is now being advanced by some as more appropriate to ecology and evolutionary theory than the rational positivism of physics (Grene, 1985; Hall and DeAngelis, 1985; Stent, 1985; Taylor, 1987).

Hence I argue that economic and ecological methodologies have existed in most of the 16 categories of the taxonomy devised in section III. Individual economists and ecologists are certainly not arguing from all of the categories at any one time. Many, however, have made different arguments at different times which, consciously or not, have effectively meant they were arguing from different methodological positions. In the next section, I argue how the greater methodological diversity of ecology has helped it be more scientific than economics.

V. THE COSTS OF METHODOLOGICAL POVERTY

In both economics and ecology, theories have been accepted which have subsequently been shown to be logically inconsistent. While the challenges to the two disciplines were similar, their responses were decidedly different. Orthodox economists failed to respond to a major flaw in their logic for lack of alternative methodological beliefs, while ecologists responded to an equal challenge with less difficulty because they had methodological alternatives.

For perhaps half a century, ecologists believed that diverse ecosystems are more stable than simple ecosystems. A mixture of evidence and theoretical arguments supported the belief. Population variations were perceived to be small in the tropics where there are many species compared to in the arctic where populations variations are large and the number of species are few. Mixed-grass prairies have lower variation in their biomass than hay fields. The law of large numbers ought to apply to ecosystems. And predators in diverse systems can more likely choose between prey so that prey and predator population crashes are avoidable. Diversity–stability "theory" generated many of the prescriptions for ecosystem management beyond those directed toward individual species.

By the mid-1970s it became clear, though not yet broadly accepted within the discipline, that diversity–stability theory was based on arguments that intermixed different definitions of the key terms, stability and diversity, and that the logic did not hold up to mathematical exploration (May, 1973; Goodman, 1975; Murdoch, 1975; Pim, 1984). These findings divided the profession, pitting the mathematically inclined against the pragmatic and field oriented. After intense rethinking and discussion the profession now

has a richer understanding of how a well-accepted belief might only be gospel. It also is more sophisticated in its understanding of how different types of diversity relate to different definitions of stability. The new found knowledge does not generate universal principles for ecosystem management, but neither does it produce false prescriptions for action.

Economists have also accepted theories for which there has been little exploration for theoretical consistency. Institutional, historical, and marxist economists have consistently argued that the assumptions and logic of the neoclassical model narrowly restrict its prescriptive application. For example, whether both or even either nation experiences gains from trade depends on specific conditions—labor and capital mobility; all resources and environmental services being owned by fully informed, prescient owners; no transactions costs, etc.—conditions which never exist in reality when economists prescribe free trade. As mathematical proofs became popular, Lipsey and Lancaster (1956) demonstrated that economic prescriptions must be tailored to the specific circumstances except for the rare case where all of the assumptions of market theory are true but one, the prescription recommended. The argument proved logically irrefutable, but economists discussed its implications for a while, and then ignored it. Arguing from theory correctly entails digging into the specifics of each case, the nitty gritty of social and environmental systems. No universal policy recommendations applicable to the real world flow directly from the neoclassical model, but this has not impeded economists from making prescriptive statements directly from theory.

Ecologists certainly would prefer universal law with respect to stability and diversity. But they had the methodological flexibibility to rethink relationships and recast their knowledge in ecosystem and perturbation specific terms. Ecologists were already accustomed to thinking that knowledge could be specific. The dominant strain of economists, on the other hand, have not been so accustomed. They have continued to make general prescriptions, ignoring the logic of their own model. Neoclassical economics could be a science if it prescribed in accordance with specific circumstances *.

* The argument in this paper has been restricted to scientists' and lay people's perceptions of scientific world views and methods. How scientists influence each others' practice of science (Kuhn, 1962; Ziman, 1968) and how the practice is influenced by the position of scientists with respect to agencies that fund them and the interest groups and policy makers who seek their advice (Ziman, 1968; Ravetz, 1971) is deliberately being skirted in this essay simply because the argument becomes immensely more complicated to weave together. Weeks (1971), Schultze (1982), Earl (1983), and Nelson (1987) describe, from quite different points of view, the public role of economics and its impact on the profession.

VI. THE CASE FOR CONSCIOUS METHODOLOGICAL PLURALISM

The initiation of this journal is a conscious effort to find better ways of understanding the interplay between economies and ecosystems. The course and success of this effort over the coming decades will depend heavily on the methodological sophistication of those who participate in its unfolding. In this concluding section I argue for conscious methodological pluralism. This methodological stance would entail most, or at least the dominant, participants: (1) being conscious of their own methodologies; (2) being conscious of the advantages and disadvantages of the methodologies used by others; and (3) being tolerant of the use of different methodologies used by others. Some participants might also be adept at using different methodologies as indicated by the circumstances. I have six supporting arguments for conscious methodological pluralism and a final caveat.

First, logical positivism is inappropriate but necessary. Logical positivism denies that how we think affects cultural and ecological systems. Clearly, this is not simply a minor shortcoming. The web of global, national, and local economic and ecological problems are manifestations of how we have thought about economic systems, natural science, and the (non) role of ecological systems and culture in the development process. Species and cultures have been driven to extinction and economically valuable ecological processes and cultural traits irretrievably lost because ecological and cultural systems are not mechanical systems which can be pushed to new equilibria and brought back as desired.

Yet logical positivism is necessary because modern people perceive science in terms of objective, universal truths. To a large extent modern societies are organized to act on science presented to it from this, and only this, methodological stance. Until the illogic of logical positivism is better known throughout society, the use of logical positivist arguments will be justified in certain circumstances. Hopefully, the conscious use of logical positivist arguments will also incorporate warnings of potential dangers. In any case, we must be able to work with logical positivism while developing more appropriate methodologies.

Second, it is clearly too early to limit the methodologies used in ecological economics now even if a narrower set might be agreed upon later. To select a narrow set of methodologies now would eliminate, or at least reduce the access to, much of ecology given its multiple methodologies and, unless logical positivism is selected, nearly all of economics. The efforts to date at ecological economics in the methodological intersect of neoclassical economics and population biology, for example, provide very limited insights (Clark, 1976). Most of the methodological intersects between ecology and economics are simply too narrow to generate interesting results. Pressure to

define ecological economics by narrowing its methodology should be resisted.

Third, pluralism makes sense. Ecological economics must address the complex interplay of global economies and local interests, sophisticated technologies and human frailties, environmental systems and social controls on their use, and limited resources. Clearly there is not one best, let alone all encompassing, perspective for understanding and managing problems of the complexity we now face. Arguments which are adamently presented initially as right thinking (Odum, 1971; Meadows et al., 1972) are frequently better developed a decade later in a pluralist frame (Hall et al., 1986).

Fourth, pluralism prevents brash action. Those who are accustomed to "one right way of thinking" will point out that the practice of methodological pluralism will lead to multiple "answers" and no clear course of action. In fact, science only gives insights into complex issues. It is easy to suffer the delusion that the insight of a particular method is the answer when no other methods have been tried to provide other insights. Single method/answer delusions lead to brash action which are likely to subsequently prove to be mistakes. Also, people who only think one way are susceptible to twisted, deliberately distorted arguments in those areas for which the pattern of thinking is least adequate. The multiple insights of multiple methods constantly remind us of the complexity of social and ecological systems and the difficulties of taking action.

Fifth, pluralism can help sustain biological and cultural diversity. Until the twentieth century, the world can be thought of as having been a patchwork quilt of coevolving cultures and ecosystems. Within each patch, biological selection was influenced by cultural characteristics including ways of knowing while the selection of cultural traits was influenced by ecological characteristics. The adoption of Western forms of knowing, technological intervention, and social organization has reduced both cultural and biological diversity. Yet to a considerable extent, ecosystems are still different because the selective pressures applied by people have been different due to differences in how people have thought about nature. Similarly, cultural diversity still exists because of diversity in ways of thinking. Conscious methodological diversity will facilitate the return of the patchwork quilt as well as coordinated effort where needed.

Sixth, methodological pluralism promotes participation and decentralization. Any given framework is better understood by, more appreciated by, or results in answers which are more advantageous to some people than others. Any framework that has been highly elaborated to stretch its usefulness can only be understood by a few who are well informed of its technical details. The use of a single framework, without modification for regional differences, facilitates control from a single center of analysis. Thus the use of a single framework disinfranchises or disqualifies the majority, facilitates the tryanny of technocrats, and encourages centralization. Openness to multiple frames of analysis is a prerequisite to democracy and decentralization.

The case for methodological pluralism is not an argument for using just any framework of analysis. For narrow, well-defined questions, the most suitable framework is somewhat predetermined. Analysts, however, repeatedly ignore how the framework with which they are accustomed to using and supposedly most familiar contains assumptions which preclude pursuit of the question. It is inane, for example, to explore questions of intergenerational equity within a neoclassical framework that commits the analyst to discounting the future by the rate of interest determined by the current generation (Hannon, 1987). Similarly, economists have questioned the existence, nature, and social implications of long-run resource scarcity using models which assume that private resource allocators are already informed of the nature of resource scarcity and acting in accordance with this information (Norgaard, 1988). Methodological pluralism acknowledges the limits, and hence the appropriateness, of specific methods to specific questions.

Broader, less well defined questions can only be pursued through multiple, overlapping analyses, extensive discussion between diverse experts and the people directly affected, and judgment. If we accept that there is not a comprehensive right way of predicting the future consequences of our choices, we will more likely make decisions sequentially in relatively small increments, build monitoring and learning into every program of change, and be adaptive (Holling, 1978; Walters, 1986).

In summary, ecological economics will more likely evolve into a useful discipline if it maintains the breadth of the methodological base of economics and ecology and reaches out to the methodologies of other disciplines as well. Our efforts will almost certainly fail if the methodological base is limited to the methodologies held in common between the dominant strain of economics and any strain of ecology. The methodology of neoclassical economics ignores how our culture and history affect how we know and how what we have known affects the systems we are studying. We do not know which theories of ecological economics will prove better when, where, and for whom, so we should not eliminate any at the conceptual stage in too fine a methodological filter. If we hold to the belief that knowledge is accumulating to one congruent understanding, we will miss the insights provided by incongruent ways of knowing. Multiple insights guard against mistaken action based on one perspective. Lastly, if we hold to the belief that knowledge consists of universal laws with universal applicability, we will apply it accordingly and destroy the diversity in the cultural and ecological systems we are trying to sustain.

This paper was presented at the International Conference on Ecology and Economics sponsored by the European Coordination Centre for Research and Documentation in the Social Sciences (Vienna) and the Department of Economics of the Universitat Autonoma de Barcelona, held in Barcelona, September 1987. An earlier version of this paper titled "On Models and Knowledge in Economics and Ecology" was presented at the Annual Meeting of the International Society of Ecological Modeling—North America, held with the American Institute of Biological Sciences, Columbus, Ohio, August 1987. I would like to thank participants of both conferences and Brian Baker, Andrea Baranzini, Raul Garcia, Charles Hall, Richard Howarth, Brian Norton, Richard Sclove, and Peter Taylor for suggestions and encouragement.

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