

Models and Knowledge in Ecology and Economics

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Introduction

Ecologists are fundamentally more dubious about "progress" than economists. This difference has become increasingly accentuated since World War II with the loss of the broader Western ideals of progress and the rapid increase in population, material consumption and ecosystem transformation. That which economists point to with pride, ecologists point to with dismay(1). Yet economists and ecologists explore complex systems in a manner sufficiently similar that there have been critical 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 are the same as those used to describe economic markets. The optimization models of ecology are similar to those of production economics. The similarities are well documented(2).

Some economists apply their knowledge to ecosystem protection and some ecologists apply their knowledge to economic development. Nevertheless, the two disciplines are seen as the scientific components of juxtaposed world views(3). Economic and ecological arguments tend to be invoked by people with different values, different interpretations of the nature of systems and, consequently, different policy recommendations with respect to how people should relate to their environment. Many look to a Hegelian resolution of these opposing world views to get us beyond the present development-environment confrontation and guide us through the next century. Such a resolution would entail new concentrations in those areas where economics and ecology have different beliefs about knowing. It is in this broad context that I compare the models and knowledge of the two disciplines.

The Path of this Inquiry

Let me classify scientists according to their beliefs about science by two criteria with two categories of each criteria. First, scientists either believe in objective knowledge, or positivism, or they do not. Second,

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scientists either believe that science is leading toward a unified set of universal laws, a belief I label conceptual unitism, or they believe that we will only know reality through an array of incongruous ways of looking at it, a belief I label conceptual pluralism(4). Positivism and unitism have been central tenets of Western science which, though long questioned, profoundly influence our social organization and process of decision-making. In my judgment, the resolution between the economic and ecological world views entail a resolution of these beliefs. Scientists' beliefs, of course, do not easily fit into a simple two by two matrix. These categories serve my inquiry well but are not meant to serve other equally important explorations.

Economists - whether neoclassical, marxist or without an orthodoxy - tend to be positivists and unitists in the face of a long history of debate. Ecologists tend to be positivists and conceptual pluralists by default. Neither discipline's position will survive the confrontation, let alone a synthesis, of the two.

This inquiry reflects my beliefs and hopes. I am a post-positive conceptual pluralist searching for the key to sustaining cultural and biological diversity. Since how we perceive affects our decisions and thereby the evolution of both the social and natural world with which we interact, I believe that conceptual diversity is an epistemological stance that will sustain both biological and cultural diversity.

I perceive more pluralism in ecology than in economics, thus economists will find that I slight the virtues of their methodology. But since pluralism in ecology exists by default rather than by choice, ecologists will find that I am making a virtue of what most consider to be their weakness. Both will discover that I am not simply comparing the methodological practices and beliefs of each discipline but am also making an argument for post-positive conceptual pluralism as a component of the resolution.

Some Difficulties of Methodological Comparison

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 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 which can be definitively identified so that they can be sorted into types. But, like the problem of defining species in biology, every rule denies the differentiation and happenstance that explains the evolution and speciation of ideas in the disciplines(5). This logical paradox deserves elaboration.

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 two centuries through more refined mathematical treatment. Parameters are also now estimated through increasingly sophisticated econometric analyses of generally better and better data. The steady progress in the development of the model has led to broad acceptance among a growing and increasingly powerful profession. Many economists are convinced that it provides profound insight into questions of markets and economic efficiency and hence much of economic policy.

Critics of economics, on the other hand, are dismayed by the simplicity of the assumptions behind the model and the fact that mathematical elaboration and statistical estimation have not resulted in an accumulation of knowledge that is usable from one year to the next(6). Economists with a much more historical leaning competed successfully with the simple use of the market model into the early part of the twentieth century. Historical, institutional and marxist economists still dominate in a significant portion of the universities in Europe and in a few in the United States. They have always questioned how economists who predict and prescribe solely on the basis of the market model think they know. In addition to this diversity in views with respect to the dominant model, the vast majority of economists have harbored various non-market models to explain the aggregate levels of output, employment and inflation.

Economics has a dominant paradigm, but it is not monolithic. The patterns of economic thinking over time, across regions, on different problems and by schools of thought are not easily generalized. Likewise, the levels of methodological confidence and questioning in economics have varied over time and across schools. Patterns of thought and methodological confidence are even less readily characterized for ecology where an array of different models are used to pursue different questions.

Ecologists are becoming very conscious of their methodology. Indeed, a large portion of the literature has a strong methodological slant(7). The methodology of economics is coming under increasing attack and economists are beginning to respond(8). This surge in epistemological pondering adds to the difficulties of characterizing how each discipline knows.

Problems with generalizations lead directly to problems with documentation. I am presenting an interpretation of my exposure to the literatures and behavior of both disciplines.. Even if this paper were a dissertation in philosophy replete with collections of documentary quotes, another scholar could assemble another set of quotes supporting an alternative interpretation with different insights. Insights, even different insights for each of us, but certainly not universal truths or even a few stray usable facts, are the best that our quarrying will yield.

Terminological Elaboration

Positivism. Western science sought to know the universal characteristics of an unchanging natural world. Knowledge of how things actually were led science to become a powerful, separate authority that often countered the church and state. 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 served a functional role in between, linking the desirable to the possible.

Belief both in the link between science and progress and in the objectivity of science, the waning role of the church and the perceived difficulty of democratic decision-making in a technically complex world led to a new alliance between science and the state. The progressive movement, perhaps best characterized by beliefs during the 1920s and 1930s but still with us today, envisioned politically neutral scientists making decisions on the public's behalf under the broad guidance of elected officials. The development-environment debate of the past quarter century has been one of the major erosive forces of the progressive alliance. Advocacy science is now widely practiced, but neither advocacy science nor progressivism enjoy a supporting public philosophy(9).

Unitism. Many believe that 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 many of 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. Unitism is a cluster of beliefs that is consistent with progress in the physical sciences until this century and in much of microbiology since.

Conceptual pluralism consists of a different cluster of beliefs(10). Systems are complex. The complexity is different across cultures for economic systems, across physical environments for ecological systems and over time in both. The universal laws and much of the knowledge of the physical sciences are applicable to economics and ecology but lend little to our understanding of them as systems. We can look at them as mechanical systems or as evolving but must concentrate our vision on different categories of relations, parts, subsystems or levels of aggregation. No one way of looking at complex systems gives complete understanding, each gives some insights, there are no logically consistent approaches to aggregating the ways of understanding together and never will be. Models are necessarily simplifications and hence necessarily imperfect instruments for looking into reality. Multiple models provide perspectives on a question, acting as a safety net against mistaken action based on only one way of knowing.

Those who believe in the eventual unity of knowledge also tend to be positivists. These two combine to a belief in comprehensive rationality, the idea that individuals and organizations can and should make decisions on the basis of complete information and its rational processing through consistently linked models. Formal knowledge supports comprehensive rationality as the legitimate form of decision-making. Formal knowledge, of course, is not completely developed and sufficient data cannot always be collected. But accepted models should be pushed as far as they can be and data collected to the extent possible. The limits to formal knowledge should be pushed back so that politically agreed upon decision rules, experiential knowledge and tradition need be relied upon as little as possible. Politics, experiential

knowledge and tradition are unfortunate necessities, but belief in the eventual unity of the sciences sustains the belief that comprehensive rationality will at least be possible in the future.

Conceptual pluralism, on the other hand, links formal knowledge to decision-making through informal knowledge and institutions. The multiple insights of diverse models provide a rich base of knowledge for action. But it is too rich to facilitate the rush into ill-considered action - the chemical control of insects without considering agroecosystem dynamics, the development of nuclear power generation facilities without considering waste containment - that has become the focus of criticism of the dominant relation between science and action. Ecologists, of course, are famous for arguing that science is inadequate to support action. But their position is usually based on incomplete knowledge, a comprehensive rationalist argument, rather than on the conflicting insights of conceptual pluralism. In the pluralists' view, individuals and organizations reach decisions by combining scientific knowledge with experiential knowledge, judgment, politically agreed upon decision rules and organizational structure, habits and traditions. The very social phenomena that are begrudgingly accepted as "fallback approaches" by the unitists are seen by conceptual pluralists as the only way to link science to action.

Epistemological Beliefs

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 two reasons. First, unreason with respect to things social appeared so plentiful that the temptation could not be avoided. Second, 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 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.

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 economists were trained in this tradition in Germany, but only a few returned as serious scholars of economic history(11). 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. The positivists acquired the high ground such that methodologies which acknowledged values as a part of the methodology became only something of which others might be accused(12).

Early economists idolized Newton. Present day mathematical economics emulates his mechanics. Economists have assumed the epistemological beliefs of 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 coherent, positive theory.

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 demand, nor constants which have been shown to be universal. Nor do economists test for these. While economists now recognize that they do not practice their methodological beliefs, the debate is almost evenly divided between those who think they should and those who argue that economists need to understand the strengths and weaknesses of the methodologies they actually use and work toward an epistemological stance appropriate to the nature of investigating economic systems and questions(13).

The long history of near agreement on epistemological beliefs in economics, even though these beliefs have not been practiced, contributed to an environment in which innovation could be discouraged. Orthodox economists have been quick to argue that the arguments and conclusions of innovators have not been tested and probably never could be tested. At a minimum, the epistemological consensus contributed to the polarity between the orthodox and not. Members of each school of thought have enjoyed the camaraderie of pointing out the methodological insincerity of the others. These methodological arguments in economics reduce to: "Your emperor has no clothes".

The development of methodological beliefs in ecology are more difficult to trace than in economics. Evolutionary theory has stimulated clearly defined epistemological debates(14). Other fields of biology have not. Ernst Mayr argues(15):

“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, observers of natural history increasingly observed the interactions of species and environmental features, but the term ecology was first proposed by Ernst Haeckel in 1866 while the term ecosystem was not coined by A.G. Tansley until 1935. The development of ecology as a body of thought and of ecology as a discipline is a twentieth century phenomena(16). Even so, 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, the sociology of knowledge in ecology has been influenced by all of biology.

A methodological literature distinct to ecology is only now developing. Jonathan Roughgarden, a mathematical evolutionary ecologist, has been at one of its epicenters(17). His arguments are distinctly pluralistic both respect to methods and paradigms. Like many who have promoted the use to mathematics, he makes it clear that mathematical exploration is only one approach to thinking about systems. He also argues that ecologists will not find universal patterns. Those who practice other approaches, however, are distinctly fearful of formalization while at least hopeful of universals. Roughgarden argues(18):

“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 misdirected theory could divert the entire field away from a commonsense 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. A useless model should be discarded like a broken chisel.”

Roughgarden also holds controversial views with respect to observation and testing. For example(19):

“Any belief that scientists establish facts with more certainty than we can in our everyday lives is a delusion. Our distinctive activity as scientists is that we encounter and experience phenomena that are remote from everyday lives or that are overlooked during our everyday lives; but the way we try to understand phenomena is with abilities whose credibility originates in everyday circumstances.”

“As scientists we use experimental setups, specialized equipment and statistical techniques more often than we do in our daily lives; but all this is a matter of degree, not kind. In our daily lives we test the speed limit, sample clothes, alter recipes, and so forth; all activities with parallels in the practice of scientific inquiry.”

I think Roughgarden’s interpretive views represent the common sense of the discipline. But it is a common sense that embarrasses when made explicit. The responses to his arguments indicate that the dominant epistemological beliefs of science are “officially” held in ecology even if they are impossible to follow, much as in economics(20). Hence I argue that conceptual pluralism exists in ecology by default rather than by choice, though the choice could easily be made since the practice is so widely tolerated.

Lastly, ecologists have avoided the issues of positivism by emphasizing “natural” systems. Few systems, of course, have not been perturbed by people(21). Ecologists are certainly comfortable noting how modern agroecosystems are a product of our beliefs in science, noting how modern agricultural problems in particular stem from the paucity of systems thinking in the agricultural sciences(22). Still, they prescribe from a stance of “right thinking” rather than from within the system. The growing interest in agroecology, how traditional farmers have evolved agroecosystems, has brought ecologists and anthropologists together and promises epistemological enrichment and confusion for ecology. These ecosystems cannot be understood without incorporating design and selective pressure through agricultural practices and how these were formed by beliefs about ecology(23).

Research Style

The conceptual pluralism of ecologists and the unitism of economists have coevolved with their approaches to research and are clearly visible in their respective research styles today. A logical argument supports the observation. First, scientists who study complex systems in only one way - whether through mathematical models, laboratory simulations, field research, and, though to a lesser extent, historical research - are more likely to hold to the unity of science view. Scientists who study systems through multiple approaches are more likely to be conceptual pluralists. Second, through a multiple of approaches are used in both disciplines, individual economists are more prone to specialize than ecologists.

The first premise has its own internal consistency. Different conceptual frameworks have different methodologies and techniques of analysis in nearly a one to one relationship(24). Field research builds a very different type of understanding of systems than does the pursuit of the nature of systems dynamics through mathematical exploration. If we observe individual researchers practicing different techniques, they are probably conceptual pluralists.

The second premise can be empirically supported. Field knowledge, indeed repeated field experience, has been and is still almost a prerequisite to being an ecologist. Ecologists tend to eclectically fit novel field observations into various conceptual patterns. Only a few economists do field work for their PhD dissertations and only a portion of these individuals continue to base their work on direct observations thereafter(25).

Economists are noted for their tendency to look for data or case studies which fit predetermined models rather than to select models to explain preselected events. There are, of course, exceptions in both disciplines. Most notable are the rising breed of mathematical ecologists. Within economics the dwindling breed of institutionalists are exceptions as well as a few who, because they emphasize particular industries, explain specific events. Equally important, economists before World War II were much more prone to fit economic behavior, industrial structures and historic episodes to alternative patterns of thinking.

Individual economists who work with models of production functions, partial market equilibrium, computable general market equilibrium and industry input-output may appear to be practicing different methods. While the structures of these models look different, they are either parts or alternative simplifications of the overall market model. Many economists do work with both market and macro economic models where the market model has never aggregated to the macro. The profession, however, has long worked on and lamented over this incongruence. The attempts and laments themselves document economists' belief in unitism(26).

Within ecology, population, food-web and other models are not thought to be simplifications or subsets of an accepted general model. Ecologists have tried, in what they term the "mechanistic" approach, to build systems models from basic biological principles(27). These attempts are analogous to the efforts by economists to build macro models from market principles. Ecologists, however, seem much more philosophical about the lack of progress in their efforts. At the same time, it is only fair to note that some economists have recently begun to advocate methodological pluralism(28).

The Problem of Context

The disciplines share two problems of context. The first is the problem of history. Both ecology and economics attempt to explain systems in terms of relationships between the parts in a particular time period, yet the parts and relationships stem from earlier phenomena that are not included in the model. Economics is not history; ecology is not evolution. Yet history and evolution steadily make the parameters of the models of each discipline obsolete such that they can only predict a little way into the future. The second is the problem of regional differences. Economic relations are affected by the types of consumption and production activities accepted or encouraged within particular cultures while ecological relations are affected by the characteristics of species compatible with particular physical environments and the coevolutionary path followed in particular regions. The differences in the ways in which ecology and economics have handled temporal and areal differences support the overall argument of this paper with respect to unity and pluralism.

Ecological and evolutionary theory have enriched each other. During the first century of evolutionary thinking, niche and hence fitness were primarily thought of in terms of the physical environment which

was presumed to change very slowly except during rare, catastrophic events. Niche in ecology, however, has always emphasized the other species with which a species interacts. To be sure, evolutionists have long been fascinated with how bigger teeth or longer legs might enhance survival viz a viz other animals, but it took ecological thinking to put individually adaptive responses into a systems context. Evolutionary thinking is also very common in ecological thinking. The two bodies of knowledge, however, are by no means formally integrated. Ecologists think like evolutionists and vice versa because each think in multiple patterns(29).

The major differences between ecosystems are noted in courses on principles of biology and stressed in courses on ecology. Indeed, there are separate courses on specific ecosystems. Students are expected to learn that succession is a reasonably productive concept for thinking about the dynamics of temperate forests while light gap is a more meaningful concept for tropical forests. Ecologists change concepts to fit the context. They can also be very concerned about the boundaries of their systems(30).

Economics used to be like ecology is now. Economists used to know sociology, political theory, history and geography and relied upon these disciplines to understand how societies worked, changed and were different from place to place. Economics helps link or describe the interactions between things, but the things of economics are largely historical and cultural artifacts. Max Weber argues(31):

“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.”

Frank Knight reaches a similar conclusion by a less tortuous route(32).

“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 than 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 gets to the same conclusions most concisely(33).

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

This understanding of theory and context, of how economics relates to the other social sciences, has been nearly lost over the past half century. In its place has grown quite the opposite understanding. Economics has now expanded its domain into history, politics, and sociology(34). Economic arguments have been developed to explain how history unfolds, politicians maneuver and people find partners. Economics students are still expected to have a general social science education, but very little more so than biologists. There are no social science prerequisites to economics comparable to the physics, chemistry, and microbiology that students of ecology are expected to know.

The dominant strain of economics treats other cultures as evolutionary misfits or as evolutionary precursors. Other cultures are undeveloped because they have institutions comparable to the feudal system of Europe which impede free choice by individuals(35). As these institutions are eliminated, as they will be and must be for development to proceed, economic explanations will fit better and better. This interpretation is no doubt too harsh, but economic prescription almost invariably consists of making societies more like the model. Neither history nor culture still present a contextual challenge to unitism in economics.

The ways in which economists and ecologists handle the problems of areal and temporal differences support the hypothesis that ecologists are conceptual pluralists while economists are unitists. Hirshleifer closes his essay on the expanding domain of economics with the following expression of faith in the eventual unity of science(36).

"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."

Responses to Logical and Empirical Inconsistency

The argument that economists are unitists while ecologists are pluralists is supported by the way each discipline has reacted to logical inconsistencies in their theory. Theories are simplifications on reality that should at least be logically consistent. In both economics and ecology, theories have been accepted which have subsequently been shown to be logically inconsistent. While the scientific challenges to the two disciplines have been similar, their responses have been decidedly different.

For perhaps half a century, ecologists accepted the theory that diverse ecosystems are more stable than simple ecosystems. A mixture of evidence and arguments supported the theory. 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(37). The controversy divided the profession, pitting the mathematically inclined against the pragmatic and field oriented. After intense rethinking and discussion the profession now has a much richer understanding of how theory might only be gospel and of how different types of diversity relate to different definitions of stability. Unfortunately, the new found knowledge does not generate universal prescriptions for ecosystem management.

Economists have also accepted theories for which there has been little testing for theoretical consistency. After much debate in the 1950s, the majority of the profession accepted during the 1960s the Hicks-Hansen-Samuels formulation of Keynes' General Theory. The 1970s proved this simplification inadequate, largely because it left too many important strings untied. As in the case of ecology, the economics profession is much more wary of macroeconomic pronouncements. This experience, however, is not quite equivalent to that of the diversity-stability controversy in ecology where a too simple theory was identified and rejected through questioning and debate within the discipline rather than by the march of history.

A more revealing challenge to economics has paralleled the development of the market model. Institutional, historical, and marxist economists have consistently argued that the assumptions and logic of the 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 - yet these conditions are rarely, if ever, actually investigated when economists prescribe free trade. As mathematical proofs became popular, Lipsey and Lancaster demonstrated that economic prescriptions must be tailored to the specific circumstances unless all of the assumptions, but the one for which a correction is being recommended, of market theory were true for the specific case(38). The point was logically irrefutable, but rather than respond as did ecologists, economists discussed its implications for a while and then effectively ignored it. Incorporating logically correct theory would have entailed digging into the specifics of each case, the nitty gritty of social and environmental systems, which would entail using knowledge beyond economics.

With the rise in environmental awareness during the 1970s, economists took market failure seriously, undertook extensive conceptual and empirical research, and published thousands of articles. Yet they continued to assume that markets existed for the vast majority of human interrelationship such that distortions overall were small and values for non-market goods could still be determined from how people behaved(39). Such an approach is illogical unless the distortions to market prices due to market failure are very small. It is difficult to imagine the appropriate empirical measure of the overall distortions of

non-market interrelations on market prices of what constitutes small, but in the literature of the mainstream, the questions has not been pursued(40).

Social Environments

What is accepted as knowledge within a discipline is as much influenced by the social environment of the discipline as by the training, epistemological beliefs and research style of its members(41). Several works emphasizing the sociology of economics have appeared in the past decade. They argue how individual economists' find it in their interest to contribute to the mainstream, ignore critiques thereof, reject alternative theories and isolate alternative theorists(42). With respect to academic economists, Peter Earl argues(43).

“Our analysis leads to two connected ways of explaining the dominance of neoclassical economics. One is that it is safer and more rewarding to be an equilibrium theorist of the conventional kind. The other is that upbringings affect the constructions young economists form of what it is that economists do and they then act in conformity with this image unless given an exceedingly strong cause to behave otherwise.”

Arguments with respect to risk or patterning, however, are also applicable to ecology, yet overall thinking is not nearly so channeled in ecology as in economics. In general, studies of the sociology of economics seem to presume that only one theory will ultimately prevail and merely question why the neoclassical paradigm has neither advanced significantly nor been replaced. Thus while this literature addresses models and knowledge in economics, it assumes the unitism that distinguishes economics from ecology.

There is, however, a major sociological difference that might explain why economists are unitists. Economists in great numbers work closely with policy makers in major international agencies like the World Bank, nearly every national agency and the major corporations and banks. Marxists argue that economists are “dogged” about neoclassical theory because it legitimizes and sustains those in power and thereby legitimizes and sustains the economics profession as well(44). Other economic theories would legitimize and sustain other power structures and thereby economics, but it would not be the same profession or professionals after the transition.

Economists, of course, see themselves as responsible merely to a cold logic that not infrequently confronts power(45). Confrontation certainly occurs, but confrontation is relative. To the extent that neoclassical economics, the dominant public philosophy, and the distribution of power are mutually reinforcing, confrontation is minimal. Alternative conceptual frameworks question so much of the status quo that they would confront every aspect of power and be of little use to policy makers. Alternative conceptual frameworks, in effect, are alternative images of how the economic and social order could be. Thus orthodox economists, in true positivist tradition, can easily portray alternative frameworks as unrealistic, untestable, and subjective and, hence, triply unscientific. Post-positivist philosophers see the positivists chasing their tail. What sort of “workable” relations might evolve between knowledge, social structure and action in a post-positive world, however, are by no means clear.

Policy makers and laymen often joke about how economists always give different answers. But the difference in the answers are due to differences in assumptions about the state of the economy rather than about assumptions with respect to how the economy should be modeled. I argued earlier that the policy prescriptions of economics tend to be with respect to how the economy should be constructed, i.e. free trade and the use of monetary rules by the Federal Reserve, rather than how much of a tax increase is needed to balance the budget. In this sense, the dominance of the belief in the neoclassical model generates considerable consensus on policy. The consensus gives the dominant practitioners in the profession power which is used against those who would erode the consensus.

Concluding Thought

One might describe the meshing of public philosophy, economic theory, the roles of economists and the economy using biological concepts and language. The meshing process can be portrayed as coevolution whereby each component increasingly reflects characteristics of each other. Each component

has evolved under the selective pressure of how the other components have responded to evolutionary responses and random changes in the other.

Ecosystems also evolve in response to how we think we know. Traditional agricultural systems reflect the cultural myths and social organization of their people. To a considerable extent, modern agricultural policy, agroecosystems, the agricultural chemical industry and the disciplines of agricultural chemistry and much of entomology have coevolved. This coevolution, however, appears to be leading toward the extinction of its participants. Relatively few transformations, however, stem from the application of ecological thinking. There have, of course, been feedbacks between ecology and public philosophy, but the dominant feedback has been a negative, corrective feedback to economic philosophy, rather than a positive, reinforcing feedback. Modern public philosophy, ecology, ecologists and ecosystems have not coevolved.

The mirroring of economics as public philosophy, theory, social organization and profession can be explained in several ways. From here I can only speculate. Is it just a coincidence that this coevolutionary, positive feedback process favored unity instead of disunity? Might a proliferation of theories lead to a proliferation of beliefs and forms of social organization that support a further proliferation of theories, beliefs and forms, or is there something special about unity? Is it not the essence of culture? And if so, if an ecological world view were to become dominant in our culture, would not the safety net of conceptual pluralism in ecology disappear?

Alternatively, perhaps we have learned something from the environmental and cultural destruction of the 20th century. Perhaps we are approaching the time when we can choose a global safety net by deliberately fostering conceptual, and hence organizational, diversity. Perhaps we could even facilitate the rise of new, local cultures in a process of retribalization that would allow the few remaining non-Western cultures niches of their own. In such a world, both economics and ecology would be non-positivistic and conceptually pluralistic by choice.

Notes

(1) Luten, D.B. Ecological Optimism in the Social Sciences, *American Behavioral Scientist*, v24 no 1 (Sept/Oct 1980) pp. 125-151 and Ehrlich, P.R. Environmental Disruption: Implications for the Social Sciences, *Social Science Quarterly*, v62:7-22, 1981.

(2) Rapport, D.J., Turner, J.E. Economic Models and Ecology. *Science* v195 (January 28): 367-73. Boulding, K.E. Economics and Ecology, in *Future Environments of North America*, Darling, F.F., and Milton, J.P. (eds.), Garden City, New York, Natural History Press, 1966.

(3) Environmental economists tend to deny this difference, ecologists use economic arguments strategically, and the World Bank is currently trying to incorporate environmental concerns within the economic paradigm that patterns their decision-making process. Nevertheless I still find the world views to be very different (Norgaard, Environmental Economics: An Evolutionary Critique and a Plea for Pluralism, *Journal of Environmental Economics and Management* 12 <December>: 382-94, 1985).

(4) Unitism and unitistic can be found in the *Oxford English Dictionary* (Compact Edition, 1971, p3516), though not in the index to the *Encyclopedia of Philosophy* (Paul Edwards <ed> New York, Macmillan, 1967). I also refer to those who hold such beliefs as unitists, which I have apparently "made up". Monism has had many meanings in philosophy some of which relate to my use of unitism. There has been a long tradition in philosophy of referring to the unity of knowledge or of science, the idea that the logics of the various disciplines will eventually prove congruous and complementary, and it is to this tradition that I tie unitism.

(5) Ernst Mayr structures much of a book on the history of biological thought around the antinomy generated by our understandings of essence and of change (*The Growth of Biological Thought*, Cambridge, Harvard University Press, see especially Chapter 6, 1982).

(6) Robert McNown (On the Uses of Econometric Models: A Guide for Policy Makers, *Policy Sciences* v19, pp 359-80) presents a recent summary of the literature in the practical difficulties and scientific shortcomings of economics, 1986.

(7) See the various articles published in the issues of the journal *Synthese* entitled Conceptual Issues in Ecology (volume 43, nos 1 and 2, January and February 1980); George W. Salt (ed.), *Ecology and Evolutionary Biology: A Round Table on Research* (Chicago, University of Chicago 1984); Elliott Sober (ed.), *Conceptual Issues in Evolutionary Biology: An Anthology* (Cambridge, Massachusetts Institute of Technology, 1984); T.F.H. Allen and Thomas B. Starr, *Hierarchy: Perspectives for Ecological Complexity* (Chicago, University of Chicago, 1982). There is a closely related evolutionary literature with a strong ecological basis: Ernst Mayr, *op. cit.* Elliott Sober, *The Nature of Selection: Evolutionary Theory in Philosophical Focus* (Cambridge, Massachusetts Institute of Technology, 1984); Richard Lewin and Richard Lewontin, *The Dialectical Biologist* (Cambridge, Harvard University Press, 1985); Sharon E. Kingsland, *Modeling Nature: Episodes in the History of Population Ecology* (Chicago, University of Chicago Press, 1985).

(8) Spiro Latsis (ed.), *Method and Appraisal in Economics* (Cambridge, Cambridge University Press, 1976) Sir John Hicks, *Causality in Economics* (New York, Basic Books, 1979); Mark Blaug, *The Methodology of Economics: Or*

- How Economists Explain* (London, Cambridge University Press, 1980); Lawrence A. Boland, *The Foundation of Economic Method* (London, George Allen and Unwin, 1982); Alfred S. Eichner (ed.), *Why Economics Is Not Yet a Science* (Armonk, New York, M.E. Sharpe, 1983); Donald N. McCloskey, *The Rhetoric of Economics* (Madison, University of Wisconsin, 1985); Gleen L. Johnson, *Research Methodology for Economists: Philosophy and Practice* (New York, Macmillan, 1986).
- (9) Douglas Torgerson, *Between Knowledge and Politics: Three Faces of Policy Analysis* (Policy Sciences, 19:33-59, 1986).
- (10) I use the term conceptual pluralism because I find it more descriptive for this paper than the philosophical terms "interpretative" or "hermeneutics". The major difference between the dominant epistemology, represented by my unity of science cluster, and hermeneutics is that the latter accepts that both what is and how we perceive are a product of how we have known and currently know. From this perspective, the long history of concern with reality and perception and objectivity and subjectivity appear naive. The implications of hermeneutical philosophy for the social sciences are developed by Charles Taylor (Interpretation and the Sciences of Man, *The Review of Metaphysics* 25<1> September 1971, reprinted in Paul Rabinow and William M. Sullivan, *Interpretive Social Science: A Reader*, Berkeley, University of California Press, 1979; see also the editors' introduction to this volume). Susan J. Hekman presents a history of hermeneutical thinking and its implications for the sociology of knowledge (*Hermeneutics and the Sociology of Knowledge*, Notre Dame, Indiana, University of Notre Dame Press, 1986). Marjorie Grene and Gunther Stent provide very interesting interpretations of this perspective for evolutionary biology (Grene: Perception, Interpretation, and the Sciences, and Stent: Hermeneutics and the Analysis of Complex Biological Systems, in David J. Depew and Bruce H. Weber <eds.>, *Evolution at a Crossroads: The New Biology and the New Philosophy of Science*, Cambridge, Massachusetts Institute of Technology Press, 1985). Charles K. Wilbur argues that the methodology of institutional economics is interpretive without reference to the hermeneutical literature (with Robert S. Harrison, *The Methodological Basis of Institutional Economics: Pattern Model, Storytelling, and Holism*, *Journal of Economic Issues*, vXII (March): 61-89, 1978).
- (11) Baumol, W.J., presents a less than sympathetic review of the methodology of these economists. (On method in U.S. Economics a Century Earlier, *American Economic Review*, 75 <December, 1985>: 1-12).
- (12) Pribram, K., *A History of Economic Reasoning*, Baltimore, Johns Hopkins University Press, 1983, pp 228-230. Susan Hekman, *op. cit.* pp 22-25. Joseph Schumpeter delicately, and productively, juggles the question of values and the dominant epistemological beliefs in economics. (Science and Ideology, *American Economic Review*, v39:345-59, 1949).
- (13) Mark Blaug (*op. cit.*) presents a defense of the dominant epistemological beliefs and a plea that they be followed. Alfred Eichner (*op. cit.*) has edited a collection of essays which also tend to critique economists for their epistemological insincerity. Boland (*op. cit.*), Johnson (*op. cit.*) and McCloskey (*op. cit.*) encourage economists to become familiar with and learn from the diverse methodologies they actually practice.
- (14) Greene, J.C., *Science, Ideology, and World View: Essays in the History of Evolutionary Ideas*, Berkeley, University of California Press, 1981.
- (15) Ernst Mayr (*op. cit.*) pp. 113.
- (16) Ernst Mayr (*op. cit.*) pp. 121, Clarence J. Glacken, *Traces on the Rhodian Shore*, Berkeley, University of California Press, 1967, and Donald Worster, *Nature's Economy: A History of Ecological Ideas*, Cambridge, Cambridge University Press, 1977.
- (17) Johnathan Roughgarden, Competition and Theory in Community Ecology, *The American Naturalist*, v122, 1983, reprinted as *Ecology and Evolutionary Biology: A Round Table on Research*, George W. Salt (ed.) Chicago, University of Chicago Press, 1984. Both the journal and the book include responses to Roughgarden by Arthur E. Dunham, Catherine A. Toft and Patrich J. Shea, Daniel Simberloff, Donald R. Strong, Jr. and Joseph H. Connell and a summary by the editor. Participants in this exchange made no reference to a methodological literature in ecology prior to this time. Richard Levins' "The Strategy of Model Building in Population Biology" (*American Scientist*, 54:421-31, 1966) might have been cited, as might have the much earlier debate surrounding the use of mathematics in population biology (Sharon Kingsland, *op. cit.*). It is interesting that while economists more rarely question their methodology than do ecologists, it seems economists have a much better sense of the history of their methodological debate.
- (18) Roughgarden, (*op. cit.* reprint) pp 17. George F. Oster and Edward O. Wilson express very similar views on the role of theory and mathematics (*A Critique of Optimization Theory in Evolutionary Biology, Caste and Ecology in the Social Insects*, Princeton, Princeton University Press, 1978).
- (19) Roughgarden, (*op. cit.*, reprint) pp 4.
- (20) See especially the response by Simberloff (*op. cit.*).
- (21) The tendency for ecologists to ignore people's influence on the ecosystems they study is emphasized by H. Ellenburg, Man's Influence on Tropical Mountain Ecosystems in South America, *Journal of Ecology*, v67:401-16, 1979.
- (22) John Perkins documents the tension between the stance of agricultural scientists and ecologists in: *Insects, Experts, and the Insecticide Crisis: The Quest for New Pest Management Strategies* (New York, Plenum Publishing Corp, 1981).
- (23) Norgaard, The Scientific Basis of Agroecology, Chapter 2 in Miguel Altieri, *Agroecology: The Scientific Basis of Alternative Agriculture*, Boulder, Colorado, Westview Press, 1987.
- (24) Paul Feyerabend (*Against Method*, London, New Left Books, 1974) makes this argument most forcefully. Michael Scriven (Explanation and Prediction in Evolutionary Theory, *Science*, v 130 no 3374 <1959>:477-82) argues that evolutionary thinking is clearly accepted as science yet clearly cannot possibly practice the dominant methodology of falsification. Stephen Toulmin (*Human Understanding: The Collective Use and Evolution of Concepts*, Princeton, Princeton University Press, 1972) describes the pragmatic eclecticism by which science really works.

- (25) Economists seem to fear their senses will delude them or that their use will interfere with objectivity. Economists have accused me, for example, in a written "peer" review for promotion, of letting my personal observations in the Amazon affect my research.
- (26) A short history and summary of the issues are presented by Daniel Bell and Irving Kristol in their editors' introduction to *The Crisis in Economic Theory* (New York, Basic Books, 1982).
- (27) See the collection of papers presented at the Symposium on Mechanistic Approaches to Natural Communities published in *American Zoologist* (v26<1986>:3-106). The paper in this symposium by Thomas Schoener (Mechanistic Approaches to Community Ecology: A New Reductionism? pp 81-106) reviews the history of difficulties in modeling ecological communities.
- (28) Boland (op. cit. chapter 12) and Johnson (op. cit.). McCloskey (op. cit.) argues that economists are really much more methodologically eclectic than they formally acknowledge.
- (29) A critical watershed in linking ecological and evolutionary thinking was crossed by Paul Ehrlich and Peter Raven (Butterflies and Plants, A Study of Coevolution, *Evolution* 18 <1964>: 586-608). Herbert G. Baker and Paul Hurd present a review of earlier thinking along these lines (Intrafloral Ecology, *Annual Review of Entomology* 13<1968>:385-414). Roger Lewin reviews more recent changes in thinking about species interaction and evolution (Punctuated Equilibrium Is Now Old Hat, *Science* 231 <February 14, 1986>:672-3). Stephen Jay Gould documents how ecology has enriched paleontology (*Paleontology Plus Ecology as Paleobiology, Theoretical Ecology: Principles and Applications*, Robert M. May <ed> Oxford, Blackwell, 1981 <2nd ed> pp295-317. Jonathan Roughgarden attempts a mathematical integration of evolutionary and ecological theory (*Theory of Population Genetics and Evolutionary Ecology: An Introduction*, New York, Macmillan, 1979).
- (30) Ricklefs, Robert E. Community Diversity: Relative Roles of Local and Regional Processes. *Science*, 235: 167-171, 1987.
- (31) Max Weber, Objectivity and Understanding in Economics, from *The Methodology of the Social Sciences*, translated and edited by Edward A. Shils and Henry A. Finch, The Free Press, 1949, as reprinted in *The Philosophy of Economics: An Anthology*, Daniel M. Hausman (ed.), Cambridge, Cambridge University Press, p.109, 1984.
- (32) Frank H. Knight. Economics. *Encyclopedia Britannica* (1951) reprinted in Frank H. Knight, *On the History and Method of Economics: Selected Essays*, (Chicago, University of Chicago Press, 1956).
- (33) John Hicks, *Causality in Economics*, New York, Basic Books, 1979, pp 12.
- (34) This trend is documented and defended by Jack Hirshleifer (The Expanding Domain of Economics, *American Economic Review*, 75 <December, 1985>:6:53-68). In my judgement, the domain is expanding because the original assumptions of the model are being forgotten. In the neoclassical model, technologies and institutions are among the "givens" which define supply and demand. In a well-received text by Yujiro Hayami and Vernon Ruttan, technologies and institutions change because of economic demand for changes and supply of new options (*Agricultural Development: An International Perspective*, Baltimore, Johns Hopkins, 1977, revised and updated 1985). Thus we now have meta economic models, but what determines these? Certainly Weber and Knight would characterize them merely as models pushing back facts.
- (35) The classic book in this mode is T. W. Schultz, *Transforming Traditional Agriculture* (New Haven, Yale University Press).
- (36) Hirshleifer (op. cit. p.66), 1964.
- (37) Accepted theory began to fall apart when May investigated whether mathematical analogies, to the extent he could define them given the terminological confusion, were stable (*Stability and Complexity of Model Ecosystems*, Princeton, Princeton University Press, 1973). David Goodman (The Theory of Diversity-Stability Relationship in Ecology, *Quarterly Review of Biology*, 50 <September, 1975>:237-66) and William Murdoch (Diversity, Complexity, Stability, and Pest Control, *Journal of Applied Ecology*, 12<1975>:795-807) review and assess the theory more broadly. Stuart Pim (The Complexity and Stability of Ecosystems. *Nature*, 307:321-26) presents a more recent assessment, 1984.
- (38) R. Lipsey and Kevin Lancaster, The General Theory of the Second Best, *Review of Economic Studies*, 24: 11-32, 1956.
- (39) Richard B. Norgaard, Environmental Economics: An Evolutionary Critique and a Plea for Pluralism, *op. cit.*
- (40) Most institutional economists avoid this complication by using market theory to describe and predict behavior but not to put values on non-market goods or prescribe policy.
- (41) Thomas S. Kuhn (*The Structure of Scientific Revolutions*, Chicago, University of Chicago, 1962), John Ziman (*Public Knowledge: The Social Dimensions of Science*, Cambridge, Cambridge University, 1968), and John C. Greene (*Science, Ideology, and World View: Essays in the History of Evolutionary Ideas*, Berkeley, University of California, 1981).
- (42) Eichner (ed.) op. cit.
- (43) Peter E. Earl, *A Behavioral Theory of Economists' Behavior*, Eichner (ed.) op. cit. pp 121.
- (44) See, for example: Marc Linder in collaboration with Julius Sensat, Jr. *Anti-Samuelson* (New York, Urizen, 1977), Gerald E. Peabody, "Scientific Paradigms and Economics: An Introduction" and John Weeks, "Political Economy and the Politics of Economists" (*The Review of Radical Political Economics*, v23 no 2 <July 1971> pp 1-16 and 75-84 respectively).
- (45) Charles L. Schultze, The Role and Responsibilities of the Economist in Government (*American Economic Review*, 72(2):62-66, May 1982) and Robert H. Nelson, The Economics Profession and the Making of Public Policy (*Journal of Economic Literature*, XXV: 49-91 March, 1987).