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PRACTICAL ECOLOGY AND FOUNDATIONS FOR ENVIRONMENTAL ETHICS

What you take as your starting point depends on where you want to go. If you want to sail due South to the Dry Tortugas, then you start with plenty of fresh water, some food, a good navigational system, and arguably a ship-to-shore radio for the long trip. But if you want to sail due West to nearby John's Pass, then you might need some fresh water, but no food, no sophisticated navigational system, and no radio. How you begin a journey depends on where you want to go. So it is with environmental ethics.

How you begin your environmental ethics depends on where you want them to go—whether you want them to guide naturalists or instead scientists or perhaps policy makers. If you want naturalists to use environmental ethics to encourage protective attitudes toward the biosphere, then you might begin with general goals and motivational ideals. These principles will inspire backpackers and birders, sailors and scuba divers, but they will have limited practical value in resolving environmental controversies. What such ethics gain because of their appeal and accessibility, they lose because of their generality and inapplicability. They are soft environmental ethics.

If you want scientists to use your environmental ethics to encourage accurate understanding of nature, then you might begin with general principles for avoiding erroneous claims. Such principles might enable experimenters to reduce false positives and to avoid claiming environmental effects where there are none, but they also will have limited practical value in protecting the environment. Because the most serious environmental conflicts concern situations of factual and probabilistic uncertainty, following scientific norms of avoiding false positives, in a context of uncertainty, often encourages

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false negatives, failing to recognize environmental damage when it occurs.[1] What such ethics gain because of their scientific credibility, they lose because of their generality. What they gain because of their epistemological conservatism, they lose because of their inadequate environmental protectionism. They are hard environmental ethics.

I shall argue that neither soft nor hard environmental ethics will take you where you want to go, if your destination is an ordered system of norms which will withstand courtroom challenges and which will support precise, often disputed claims over wetlands protection or development rights. The argument is that soft ethics—such as those of J. Baird Callicott, Aldo Leopold, Paul Taylor, Holmes Rolston, and Laura Westra[2]—have great heuristic and inspirational power, but they are more useful in preaching to the converted than in resolving controversy. Because they are so general, they fail to include precise second- and higher-order ethical principles that would make them operationalizable in decision making. Hard ethics—such as those of Dan Simberloff and Robert Henry Peters[3]—have great scientific credibility, but they are more useful in avoiding false claims than in discovering true ones. Instead, the ethics needed in practical policy making must be not only inspirational, but also complex and precise enough to help resolve controversy. They must be not only scientifically conservative, but also protective and specific enough to support particular environmental policies. They must avoid the philosophical ivory tower of soft ethics and the scientific ivory tower of hard ethics.

Although both hard and soft approaches are valuable in environmental ethics, their proponents appear to think that they are sufficient, not merely necessary, for solving environmental problems. In so doing, Rolston and others appeal to "soft ecology" to support their ethics based on inspiration rather than argument, preaching rather than offering second- and higher-order ethical analyses that

[1] Earl D. McCoy and my *Method in Ecology: Strategies for Conservation Problems* (New York: Cambridge, 1993), pp. 149–97; and "Statistics, Costs, and Rationality in Ecological Inference," *Trends in Evolution and Ecology*, VII, 3 (March 1992): 96–99.

[2] See Callicott, *In Defense of the Land Ethic* (Albany: SUNY, 1989); Leopold, *A Sand County Almanac and Sketches Here and There* (New York: Oxford, 1968); Rolston, *Environmental Ethics* (Philadelphia: Temple, 1988); Taylor, *Respect for Nature* (Princeton: University Press, 1986); Westra, *An Environmental Proposal for Ethics: The Principle of Integrity* (Lanham, MD: Rowman and Littlefield, 1994). See also note 27.

[3] Simberloff, "Simplification, Danger, and Ethics in Conservation Biology," *Bulletin of the Ecological Society of America*, LXVIII (1987): 156–57; and Peters, *A Critique for Ecology* (New York: Cambridge, 1991).

are capable of helping to adjudicate environmental controversies. Likewise, Peters and others follow "hard ecology" and search for grand, deductive ecological theories rather than modest rules of thumb that are operationalizable and applicable. My argument is that the science necessary to undergird practical environmental ethics requires that we avoid the extremes of either soft or hard ecology. Sound environmental ethics, at least at present, require a "practical ecology" based largely on case studies and rules of thumb.

A practical scientific foundation for environmental ethics must chart a middle course between the "hard," hypothetico-deductive ecology of persons like Peters and the "soft," largely qualitative ecology espoused by persons—such as Westra or Rolston[4]—who propose concepts such as ecosystem and integrity as the foundation for environmental policy making. The problem with using these concepts, as a proposed scientific foundation for environmental ethics, is that they underestimate ecological uncertainty and thus demand too little of ecology. Likewise, the more deductive concepts of Peters overestimate ecological uncertainty and thus demand too much of ecology. I shall show where both go wrong and propose an alternative.

I. PROBLEMS WITH DEDUCTIVE THEORIES AND "HARD ECOLOGY"

In an analysis that is both tough-minded and controversial, Peters argues that ecology is a "weak science" (*op. cit.*, p. 11). He claims that the primary way to correct this weakness is to judge every ecological theory "on the basis of its ability to predict" (*op. cit.*, p. 290). Peters's argument, that the main criterion for ecological theorizing ought to be its predictive power, is somewhat correct in at least two senses. Prediction often is needed for applying ecology to environmental problem solving. Peters also is right to emphasize prediction because, if scientists did not seek this goal, at least in some cases, they likely would foreclose the possibility of ever having any predictive scientific theories.

Despite the value of prediction in science, Peters's argument is misguided in at least four ways. For one thing, he is wrong to use prediction as a *criterion for*, rather than a *goal of*, ecological theorizing. Not all sciences are equally predictive. Economics and sociology, for example, are both more explanatory than predictive, yet it is

[4] Westra, *op. cit.*; Rolston, *op. cit.*,"Duties to Ecosystems," in Callicott, ed., *Companion to "A Sand County Almanac"* (Madison: Wisconsin UP, 1987), pp. 246–74, and *Philosophy Gone Wild* (Buffalo: Prometheus, 1986). For a discussion of ecologists who believe that ecosystem or community concepts and stability concepts can be used to ground environmental ethics and policy, see *Method in Ecology*, ch. 3.

not obvious that they are nonscientific by virtue of being so. Likewise, many geological phenomena—such as whether a given rock formation will be intact in 100,000 years—are not susceptible to precise, long-term prediction. We conclude from this predictive imprecision neither that geology is unscientific nor that we should reject the goal of precise geological prediction, but rather that geology probably deals with long-term phenomena that are less deterministic than those in other sciences. In overemphasizing the importance of *prediction* in ecology and science generally, Peters has erred in underemphasizing the role of *explanation*.

Peters's overemphasis on prediction and hypothesis deduction is also highly questionable in the light of the last three decades of research in philosophy of science, much of which has identified fundamental flaws in the positivistic, hypothetico-deductive paradigm for science. Thomas Kuhn[5]—and other critics of the positivist paradigm—have argued that science is likely based more on retroduction and good reasons than on deduction alone. One of the fundamental reasons that no sciences can be perfectly deductive in method is that they depend on methodological value judgments—about whether certain data are sufficient, about whether a given model fits the data, about whether nontestable predictions are reliable, and so on. Because such value judgments render strict deduction impossible, falsification and confirmation of hypotheses are always questionable, at least to some degree. Moreover, though all sciences depend on such value judgments, this dependence is particularly acute for ecology, because ecology is more empirically and theoretically underdetermined than many other sciences. In island biogeography, for example, there are many areas of underdetermination that require one to make choices among different methodological value judgments. These choices concern how to interpret data, how to practice good science, and how to apply theory in given situations, such as determining the best design for nature reserves. Such choices are evaluative because they are never wholly determined by the data.

Consider how value judgments are necessary in using the ecological theory of island biogeography in the nature reserve case. Ecologists must decide whether ethical and conservation priorities require protecting an individual species, an ecosystem, or biodiversity, when not all can be protected at once. Different design choices usually are required to protect a particular species of interest, as opposed to pre-

[5] *The Structure of Scientific Revolutions* (Chicago: University Press, 1970).

serving a specific ecosystem or biotic diversity.[6] Also, ecologists often must choose between maximizing present and future biodiversity. Currently, they are able to determine only which types of reserves, for example, contain the most species at present, not which ones will contain the most over the long term.[7] Moreover, in the absence of adequate empirical data on particular taxa and their specific autecology, ecologists frequently must decide how to evaluate the worth of general ecological theory in dictating a preferred reserve design for a particular case.[8] They also are often forced to assess subjectively the value of different reserve shapes. Besides, reserve shape, as such, may not explain variation in species number.[9] Ecologists likewise must frequently rely on subjective estimates and methodological value judgments whenever the "minimum viable population" size is not known in a precise area.[10] One of the most fundamental sources of value judgments in ecology is the fact that the island-biogeographical theory underlying current paradigms regarding reserve design has rarely been tested[11] and is dependent primarily on ornithological data,[12] on correlations rather than causal explanations (*ibid.*, p. 13), on assumptions about homogeneous habitats,[13] and on unsubstantiated turnover rates and extinction rates.[14] Hence, whenever ecologists apply this theory, they must make a variety of methodolog-

[6] C. Margules, A. Higgs, and R. Rafe, "Modern Biogeographic Theory: Are There Any Lessons for Nature Reserve Design?" *Biological Conservation*, XXIV, 2 (October 1982): 115–28, here p. 116; M. Soulé and D. Simberloff, "What Do Genetics and Ecology Tell Us about the Design of Nature Reserves?" *Biological Conservation*, XXXV, 1 (1986): 19–40; M. Williamson, "Are Communities Ever Stable?" *Symposium of the British Ecological Society*, XXVI (1987): 353–70, here p. 367; B. Zimmerman and R. Bierregaard, "Relevance of the Equilibrium Theory of Island Biogeography and Species-area Relations to Conservation with a Case from Amazonia," *Journal of Biogeography*, XIII, 2 (March 1986): 133–43, here p. 134.

[7] Soulé and Simberloff, pp. 24 ff.

[8] W.J. Boecklen and Simberloff, "Area-Based Extinction Models in Conservation," in D. Elliot, ed., *Dynamics of Extinction* (New York: Wiley, 1987), pp. 247–76; Margules, Higgs, and Rafe, p. 124; Simberloff and J. Cox, "Consequences and Costs of Conservation Corridors," *Conservation Biology*, I, 1 (May 1987): 63–71; Soulé and Simberloff, pp. 25ff.; see Zimmerman and Bierregaard, p. 135.

[9] M. Blouin and E. Connor, "Is There a Best Shape for Nature Reserves?" *Biological Conservation*, XXXII, 3 (1985): 277–88.

[10] Boecklen and Simberloff, pp. 252–55; Soulé and Simberloff, pp. 26–32; G.H. Orians, Committee on the Applications of Ecological Theory to Environmental Problems, *Ecological Knowledge and Environmental Problem Solving* (Washington, DC: National Academy, 1986), p. 231.

[11] See Margules, Higgs, and Rafe, p. 117; Zimmerman and Bierregaard, p. 134.

[12] See Zimmerman and Bierregaard, pp. 130–39.

[13] Margules, Higgs, and Rafe, p. 117.

[14] Boecklen and Simberloff, pp. 248–49, here p. 257.

ical—and sometimes ethical—value judgments. Some of these value judgments concern the importance of factors other than those dominant in island biogeography (for example, maximum breeding habitat), factors that often have been shown to be superior predictors of species number.[15] Making value judgments regarding reserve design is also difficult because corridors (an essential part of island biogeographic theory) have questionable overall value for species preservation.[16] Recommending use of corridors thus requires ecologists to evaluate subjectively their effectiveness in particular situations. Also, owing to the large variance about species-area relationships,[17] those who use island biogeographical theory are often forced to make subjective evaluations of nontestable predictions. Some of these subjective evaluations arise because islands are disanalogous in important ways with nature reserves.[18] As a result, ecologists who apply data about islands to problems of reserve design must make a number of value judgments about the representativeness and importance of their particular data.

Because of the empirical and theoretical underdetermination exhibited by ecological theories like island biogeography, and because of the resultant methodological value judgments necessary to interpret and apply it in specific cases, ecology does not appear to be fully amenable to hypothesis deduction. The included value judgments break the deductive connections of the theory. Of course, there are rough generalizations that can aid problem solving in specific ecological situations, as a prominent National Academy of Sciences Committee recognized (*op. cit.*). Nevertheless, it is unlikely that we shall be able to find many (if any) simple, general, hypothetico-deductive (HD) laws that we can easily apply to a variety of particular communities or species. A second reason—in addition to the underdetermined, value-laden theory—that such laws are unlikely is that fundamental ecological terms (like 'community' and 'stability') are

[15] *Ibid.*, p. 272; see Simberloff and Cox, pp. 63–71; Margules, Higgs, and Rafe, p. 120; Zimmerman and Bierregaard, pp. 136ff.

[16] Simberloff and Cox, pp. 63–71; see Orians, p. 32; H. Salwasser, "Conserving a Regional Spotted Owl Population," in *Ecological Knowledge and Environmental Problem Solving*, pp. 227–47.

[17] Boecklen and Simberloff, pp. 261–72; E.F. Connor and McCoy, "The Statistics and Biology of the Species-area Relationship," *American Naturalist*, CXIII, 6 (June 1979): 791–833; McCoy, "The Application of Island Biogeography to Forest Tracts: Problems in Determination of Turnover Rates," *Biological Conservation*, XXII, 3 (March 1982): 217–27, and "The Application of Island Biogeographic Theory to Patches of Habitat: How Much Land is Enough?" *Biological Conservation*, XXV, 1 (January 1983): 53–61.

[18] Margules, Higgs, and Rafe, p. 118.

imprecise and vague, and therefore unable to support precise empirical laws.[19] Likewise, though the term 'species' has a commonly accepted meaning, and though evolutionary theory gives a precise technical sense to the term, there is no general agreement in biology on an explicit definition of 'species'. There is consensus neither on what counts as causally sufficient or necessary conditions for a set of organisms to be a species, nor on whether species are individuals. Phenetic taxonomy has failed to generate a workable taxonomy, perhaps because species are not natural kinds and because facts cannot be carved up and rearranged in accord with the hopes of numerical taxonomists.[20]

Simple, general, hypothetico-deductive laws are also unlikely in ecology because of the uniqueness of ecological phenomena. If an event is unique, it is typically difficult to specify the relevant initial conditions for it and to know what counts as relevant behavior. One must often have extensive historical information in order to do so.[21] Hence, from an empirical point of view, complexity and uniqueness hamper the elaboration of a simple, general set of hypothetico-deductive laws to explain most or all ecological phenomena. And if so, then the "hard ecology" of Peters is not a reasonable foundation for environmental ethics because it does not appear achievable. HD may be an important ideal but, at present, it appears to demand too much of ecology and to overestimate its potential for certainty.

II. PROBLEMS WITH ECOSYSTEM INTEGRITY AND "SOFT ECOLOGY"

At the other extreme of proposed scientific foundations for environmental policy making, concepts like "integrity" demand too little of ecology because they are qualitative, unclear, and vague. They underestimate the ecological uncertainty associated with such fuzzy terms. Arne Naess[22] recognized this point when he claimed that the normative foundations provided by ecology are "basic intuitions." The problem with intuitions is not only that they are vague and qualitative but also that one either has them or does not. They are not the sort of things amenable even to intelligent debate, much less to

[19] See *Method in Ecology*, ch. 2.

[20] A. Rosenberg, *The Structure of Biological Science* (New York: Cambridge, 1985), pp. 182–87; P. Sokal and P. Sneath, *Principles of Numerical Taxonomy* (San Francisco: Freeman, 1963); D. Hull, *Science as a Process* (Chicago: University Press, 1988), pp. 102ff.

[21] See A. Kiester, "Natural Kinds, Natural History, and Ecology," in E. Saarinen, ed., *Conceptual Issues in Ecology* (Boston: Reidel, 1982), pp. 355ff.; James H. Fetzer, "On the Historical Explanation of Unique Events," *Theory and Decision*, VI, 1 (February 1975): 87–97.

[22] "The Shallow and the Deep, Long-range Ecology Movements: A Summary," *Inquiry*, XVI (1973): 95–100.

scientific confirmation or falsification. Hence, intuitions ask too little of ecology; their uncertainty causes us to come up short when ecologists need to defend their conclusions in an environmental courtroom.

To illustrate the difficulties with "soft ecology," consider some of the problems associated both with the scientific foundations of the concept of ecosystemic integrity and with its philosophical applications. Much of the scientific and ethical interest in integrity arose as a result of Leopold's famous 1949 environmental precept: "A thing is right when it tends to preserve the integrity, stability, and beauty of the biotic community. It is wrong when it tends otherwise" (*op. cit.*, pp. 224–25). Numerous persons—such as Callicott, J.D. Heffernan, Leopold, Rolston, Mark Sagoff, and Westra[23]—have done insightful analyses of the philosophical concept of integrity, but unfortunately these studies rely on problematic science or soft ecology. One of the major problems with the scientific concept of integrity is that one of the leading experts on integrity, Henry Regier,[24] has admitted that the term has been explicated in a variety of ways: to refer to open-system thermodynamics, to networks, to Bertalanffian general systems, to trophic systems, to hierarchical organizations, to harmonic communities, and so on. Obviously, a clear, operational scientific concept cannot be explicable in a multiplicity of ways, some of which are mutually incompatible, if one expects the concept to do explanatory and predictive duty for field ecologists and therefore philosophical and political duty for attorneys, policy makers, and citizens involved in environmental controversies.

A second problem with the integrity concept is that when persons attempt to define it precisely, often the best they can do is to specify necessary conditions, such as the presence of "indicator species" for ecosystem integrity. For example, the 1987 Protocol to the 1978 Great Lakes Water Quality Agreement formally specified lake trout as an indicator of a desired state of oligotrophy (*ibid.*). One difficulty—with using such species to indicate environmental integrity—is in part that tracking the presence or absence of an indicator species is imprecise and inadequately quantitative. A better idea

[23] Callicott, *Companion to "A Sand County Almanac"*; Heffernan, "The Land Ethic: A Critical Appraisal," *Environmental Ethics*, IV, 3 (Fall 1982): 235–47; Leopold, pp. 224–55; Rolston, "Is There an Ecological Ethic?" *Ethics*, LXXXV, 2 (January 1975): 103–09; Sagoff, "Fact and Value in Ecological Science," *Environmental Ethics*, VII, 2 (Summer 1985): 99–116; Westra, "'Respect', 'Dignity', and 'Integrity': An Environmental Proposal for Ethics," *Epistemologia*, XII, 1 (1989): 91–124.

[24] "Indicators of Ecosystem Integrity," in D.H. McKenzie, D.E. Hyatt, and V.J. McDonald, eds., *Ecological Indicators* (Ft. Lauderdale, FL: Elsevier, 1992), pp. 183–200.

might be to track the change in species number or taxonomic composition. Another recognized problem is that the presence or absence of an indicator species alone presumably is not sufficient to characterize everything that might be meant by 'integrity'; otherwise, persons would not speak of "ecosystem integrity" but merely of "ecosystem presence of lake trout." Hence, though the meaning of 'integrity' is not clear, defining the term via several indicator species appears both crude and inadequately attentive to the underlying processes likely contributing to the presence or absence of certain species and to the larger processes presumably possessing integrity.

The definition of 'integrity' is also methodologically suspect because it is based merely on opinions rather than on confirmed ecological theories or empirical generalizations. As Regier admits, though the aggregated form of the index of biological integrity (IBI) avoids reliance on a single indicator species, it provides an arbitrary definition of 'integrity'. It

> does not relate directly to anything that is observable by the nonexpert, nor to any encompassing theoretical or empirical synthesis. As a conceptual mixture put together according to judgments of knowledgeable observers, it is not 'understandable' in a theoretical sense. It is conceptually opaque in that it provides only a number on a scale; this number is then interpreted as bad or good according to practical considerations (*ibid.*, p. 191).

Indeed, the whole concept of ecosystem integrity seems to be conceptually opaque and vague. Regier admits, for example, that "general, qualitative, developmental tendencies of healthy organic systems...provide a basis for practical understanding, measurement, and management of *ecosystem integrity*" (*ibid.*, p. 191). But if general, qualitative judgments provide the basis for understanding ecosystem integrity, then it is arguable that they are likely to be insufficiently precise and quantitative to do the environmental work required of them if they are challenged in court by developers, polluters, or citizens asked to pay for cleanup. Also, if only experts can recognize integrity, and if 'integrity' is not tied to any publicly recognizable criteria, then the term seems incapable of uncontroversial operationalization. Hence, concepts like ecosystem integrity may be closer to "soft ecology"—or as Simberloff would put it, "theological ecology"—that preaches only to the converted (*ibid.*, p. 194). Soft ecology may be too uncertain to provide a firm foundation for the precise norms often required in environmental ethics and policy.

Admittedly, at least one branch of theory regarding ecosystems integrity—James Kay and Eric Schneider's[25] nonequilibrium thermodynamic account—is not "soft ecology" in the sense that it is not general, qualitative, and vague. Rather, it is specific, quantitative, and precise. It also yields a number of insights about ecosystem behavior. This thermodynamic version, however, assuming it might provide a correct definition of 'integrity', appears to be "soft ecology" in several other damaging senses. For one thing, the account is based on *defining* ecological phenomena in terms of a thermodynamic model rather than on discovering, confirming, or falsifying specific hypotheses about ecological phenomena. Because this account relies on definition rather than discovery, and because it does not show how at least two independent avenues function in advancing our explanation of ecological phenomena, the thermodynamic account appears to provide merely a stipulative definition, rather than a causal explanation, of ecological phenomena.

The thermodynamic account of integrity is also definitionally problematic in a second sense. On the thermodynamic model, ecosystem organization tends to increase degradation of energy, and measures of this organization rely in part on measures of energy utilization in the food web. Yet, because it is often difficult to assign organisms to a particular trophic level, it is difficult to measure ecosystem organization accurately. Linking the integrity of an ecosystem to its ability to maintain its organization, Kay and Schneider argue that there are certain situations in which an ecosystem would not maintain its organization. One such example is an ecosystem that is stressed by exposure to a 6°C increase in temperature of the water effluent from a nuclear power station.[26] If, as a result of this thermal stress, the size of the ecosystem were diminished, its trophic levels were decreased, it recycled less, and it leaked nutri-

[25] "Thermodynamics and Measures of Ecological Integrity," in *Ecological Indicators*, pp. 159–82, and "Life as a Manifestation of a Second Law of Thermodynamics," in *Advances in Mathematics and Computers in Medicine* (Waterloo, Ontario: Waterloo UP, 1993); Kay, "On the Nature of Ecological Integrity," in S. Woodley, J. Francis, Kay, eds., *Ecological Integrity and the Management of Ecosystems* (Del Ray Beach, FL: St. Lucie, 1993), pp. 201–14, and "A Nonequilibrium Thermodynamic Framework for Discussing Ecosystem Management," *Environmental Management*, xv (1991): 483–95; Peter A. Victor, Kay, and H.J. Ruitenbeek, *Economic, Ecological, and Decision Theories: Indicators of Ecologically Sustainable Development* (Ottawa: Canadian Environmental Advisory Council, 1991).

[26] R.E. Ulanowicz, "Community Measures of Marine Food Networks and Their Possible Applications," in M.J.R. Fasham, ed., *Flows of Energy and Material in Marine Ecosystems* (London: Plenum, 1985), pp. 23–47.

ents and energy, then Kay and Schneider claim that the ecosystem would not have maintained its organization. They claim that such effects are signs of "disorganization and a step backward in development."[27]

One problem with their argument is that there are many ecosystem responses to stress, and complex systems have multiple steady states. After stress, (1) the system could eventually continue to operate as before, or (2) it could operate with a reduction or increase in species number, or (3) it could exhibit new paths in the food web, or (4) it could take on a largely different structure with different species and food webs (cf. note 25). Because of the multiple steady states of complex ecosystems, a third definitional problem is that the thermodynamic model, as Kay and Schneider recognize, does not indicate which (if any) of these four changes is more or less natural or acceptable, in terms of maintaining integrity. Hence, the thermodynamic account, in itself, indicates different ways in which ecosystems respond to stress, but not which responses constitute a lack of integrity. And here is the rub. Either we must say, first, that any system maintaining itself at any optimum operating point has integrity—with the consequence that virtually any environmental change anywhere anytime is said to be consistent with integrity. This first position likely would cause environmental catastrophe and would delight many developers and polluters. Or we must say, second, that a system has integrity if it resists permanent ecosystem change—a position that does not fit the facts of dynamic and evolutionary ecosystems. This second position is inapplicable to the real world. Or, third, we must define, independently of the thermodynamic account, some type of change as a loss of integrity. Hence, the thermodynamic model reduces us, when using it for environmental policy making, to using science that is either (1) incapable of defining integrity in an environmentally protective way, or (2) inconsistent with evolution, or (3) dependent on some nonthermodynamic (arguably subjective) account of community structure. Thus, the thermodynamic model, despite its heuristic power, is definitional in at least three senses. It does not provide two independent avenues for explanation; it assigns organisms to trophic levels in a question-begging way; and it requires one to stipulate some change as a loss of integrity. For all these reasons, the account is fundamentally uncertain. It is obviously not an adequate ecological basis for environmental policy making. At best, it provides necessary, but not sufficient,

[27] "Life as a Manifestation of a Second Law of Thermodynamics," p. 21.

scientific grounds for environmental ethics and policy. Insofar as it is uncertain and requires us to fill in our knowledge gaps with subjective judgments, it leads to incomplete and soft ecology.

The objection, of course, is not to philosophical or ethical concepts of integrity which obviously may have heuristic and political power. Rather, the argument is that philosophers and soft ecologists do not call a spade a spade. They do not call soft science "soft" when it is soft, and they appear not to realize that soft science, in the absence of an environmental political consensus, is unlikely to be robust enough to support precise environmental policy decisions. When a consensus supports particular environmental values, then soft ecology is obviously valuable and heuristically useful. But situations of consensus regarding environmental values are not those in which we most need ecology.

III. A MIDDLE PATH

Given widespread controversy over environmental ethics and policy, soft ecology is unable to ground biocentric ethics on mere stipulative definition, just as hard ecology is unable to provide hypothetico-deductive theories to resolve environmental controversies. Because both types of ecology are uncertain, anyone who does environmental ethics needs both (1) a procedure for making ethical decisions under conditions of ecological uncertainty and (2) a method for using ecology, in a practical sense, to direct environmental policy. One procedure for dealing with ecological uncertainty, a procedure defended elsewhere,[28] is to minimize type II, rather than type I, statistical errors when both cannot be avoided. Contrary to current scientific norms, this rule of thumb places the burden of proof not on anyone who posits an effect, but on anyone who argues that there will be no damaging effect from a particular environmental action. One can defend this rule, despite its reversal of the norms of statistical practice, on straightforward grounds of protecting human welfare. Because of the uncertainty of both soft and hard ecology, one does not have the luxury of using them to ground purely biocentric arguments (not based on human welfare) for particular environmental decisions.

Another means of avoiding the scientific uncertainty of both soft and hard ecology is to develop a more reliable middle path, practical ecology. Based neither on stipulatively defined concepts nor on general theories lacking precise predictive power, practical ecology is

[28] McCoy and my "Statistics, Costs, and Rationality in Ecological Inference"; see also our *Method in Ecology*.

grounded on rules of thumb (like the norm regarding types I and II statistical error), on rough generalizations, and on case studies about individual organisms. A recent National Academy of Sciences (NAS) committee illustrated how case-specific, empirical, ecological *knowledge*, rather than an uncertain general ecological *theory* or *model*, might be used in environmental problem solving (*op. cit.*, pp. 1, 5). According to the NAS committee, ecology's greatest predictive successes occur in cases that involve only one or two species, perhaps because ecological generalizations are most fully developed for relatively simple systems. This is why, for example, ecological management of game and fish populations through regulation of hunting and fishing can often be successful (*op. cit.*, p. 8). Applying this insight to our discussion, ecology might be most helpful in undergirding environmental ethics and policy making when it does not try to predict complex interactions among many species, but instead avoids the uncertainties of both soft and hard ecology and attempts to predict what will happen for only one or two taxa in a particular case. Predictions for one or two taxa are often successful because, despite the problems with general ecological theory, there are numerous lower-level theories in ecology that provide reliable predictions. Application of lower-level theory about the evolution of cooperative breeding, for example, has provided many successes in managing red-cockaded woodpeckers.[29] In this case, successful management and predictions appear to have come from specific information, such as data about the presence of cavities in trees that serve as habitat (*ibid.*, pp. 506ff.).

Examples like that of the woodpecker suggest that, if the case studies used in the NAS report are representative, then some of the most successful ecological applications arise when (and because) scientists have a great deal of knowledge about the specific organisms investigated in a particular case study (*op. cit.*, p. 13). As the authors of the NAS report put it, "the success of the cases described...depended on such information" (*op. cit.*, p. 16). The vampire-bat case study, for instance, is an excellent example of the value of specific information when ecologists are interested in practical environmental problem solving (*op. cit.*, p. 28). The goal in the bat study was to find a control agent that affected only the "pest" species of concern, the vampire bat. The specific information that was useful in finding and

[29] J.R. Walters, "Application of Ecological Principles to the Management of Endangered Species: The Case of the Red-Cocaded Woodpecker," *Annual Review of Systematics*, XXII (1991): 505–23, here p. 518.

using a control, diphenadione, included the facts that the bats are much more susceptible than cattle to the action of anticoagulants; that they roost extremely closely to each other; that they groom each other; that their rate of reproduction is low; that they do not migrate; and that they forage only in the absence of moonlight.[30] Using this information, ecologists were able to provide a case study as a firm foundation for policy about controlling vampire bats and for the ethics of doing so. Rather than attempt to apply some general ecological theory "top down"—as hard ecologists might do—they scrutinized a particular case, "bottom up," in order to gain explanatory insights. Their case-study explanation was local or "bottom up" in the sense that it showed how particular occurrences come about. It explained particular phenomena in terms of collections of causal processes and interactions.[31] Their explanations do not mean, however, that general laws play no role in ecological explanations, because the mechanisms discussed in the vampire-bat study operate in accord with general laws of nature. Nor do they mean that all explanations are of particular occurrences, because we can often provide causal accounts of regularities. Rather, their case-study explanations, like the accounts of practical ecology that we wish to emphasize, are more inductive or "bottom-up" in that they appeal to the underlying microstructure of the phenomena being explained. They avoid both the hard ecology of more deductive or "top-down" explanation,[32] as well as the soft ecology based on stipulative definition of desired states.

IV. CONCLUSION

The success of the NAS case study, with its "bottom-up" approach to scientific explanation, suggests that—whenever ecology is needed to resolve environmental controversies—ecological method needs to avoid the uncertain and stipulative concepts of soft ecology, like integrity and stability. It also needs to avoid the equally uncertain, grand deductive theories of hard ecology. Reliable environmental actions seem to require case studies and autecology. Such a recipe for grounding environmental ethics and policy, however, provides no basis for purely biocentric concepts, laws, or theories. Rather, the modest practical ecology for which we have argued appears to

[30] C.G. Mitchell, "Vampire Bat Control in Latin America," in *Ecological Knowledge and Environmental Problem Solving*, pp. 151–64.

[31] McCoy and my "Applied Ecology and the Logic of Case Studies," *Philosophy of Science*, LXI, 1 (June 1994): 228–49.

[32] W. Salmon, "Four Decades of Scientific Explanations," in P. Kitcher and Salmon, eds., *Scientific Explanation* (Minneapolis: Minnesota UP, 1989), pp. 3–219.

rely on the practice of ecologists and on their individual cases. Genuinely biocentric ethics seem to require more certainty about underlying ecological concepts and theories than is currently available in the modest rules of thumb characterizing case studies and practical ecology.

Practical ecology is particularly needed in unique situations, like most of those in community ecology, where we cannot replicate singular events. If we can use the vampire-bat study as a model for future ecological research, and if the NAS committee is correct, then both suggest that accounts of ecological method might do well to focus on practical applications and on unavoidably human, but well substantiated and nonstipulative judgments about environmental management. Moreover, if ecology turns out to be a science of case studies, practical applications, and human-directed environmental management, it is not obvious that this is a defect. Ecology may not be flawed because it must sacrifice universality for utility and practicality, or because it must sacrifice generality for the precision gained through case studies.

Even with its case-study knowledge, ecology often can provide the insights necessary for sound preservation and environmental policy. This practical and precise knowledge, coupled with conceptual and methodological analysis, is a critical departure from the hypothetical deductive and general mathematical models of hard ecology and the untestable, definitional, or incomplete principles of soft ecology. Both soft ecology and hard ecology seem to fail to address the uniqueness, particularity, and historicity of many ecological phenomena. As a consequence, it likely will be difficult for either of them to provide clear directions for how to preserve the environment or how to guide environmental ethics and policy. For this we need a middle path—dictated in part by humans, not merely by biocentric theory. We need the practical ecology of case studies.

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