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APPLIED ECOLOGY AND THE LOGIC OF CASE STUDIES*

KRISTIN SHRADER-FRECHETTE†‡

Department of Philosophy
University of South Florida at Tampa

AND

EARL D. MCCOY

Department of Biology
University of South Florida at Tampa

Because of the problems associated with ecological concepts, generalizations, and proposed general theories, applied ecology may require a new "logic" of explanation characterized neither by the traditional accounts of confirmation nor by the logic of discovery. Building on the works of Grünbaum, Kuhn, and Wittgenstein, we use detailed descriptions from research on conserving the Northern Spotted Owl, a case typical of problem solving in applied ecology, to (1) characterize the method of case studies; (2) survey its strengths; (3) summarize and respond to its shortcomings; and (4) investigate and defend its underlying "logic".

1. Introduction. Two decades ago, Schoener (1972) warned that ecology has a "constipating accumulation of untested models", (p. 389), most of which are untestable, and Peters (1991) complained that the vast majority of models in the ecological literature do not describe the phenomena they purport to describe, or they contain internal mathematical problems, or both. One question such criticisms raise, apart from what sorts of claims are most appropriate to pure ecology, is whether applied ecology is ever likely to have any general theories or exceptionless laws to aid in problem solving. If not, then the logic and methods most appropriate for confirming general theories and exceptionless laws may not be those most suited to applied ecology. Sidestepping this issue of the types of causal claims most appropriate to pure ecology (if there is such a thing), we argue that

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‡Send reprint requests to Kristin Shrader-Frechette, Department of Philosophy, University of South Florida at Tampa, 4202 East Fowler Avenue, CPR 107, Tampa, FL 33620-5550, USA.

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some sort of "logic of case studies"—and an associated method—may be required in applied ecology. Although there are no sets of purely deductive inferences that one can draw from analysis of a unique, singular situation—and hence no applicable logic in the strictest sense—we argue that there is a "logic" of case studies in the sense of informal inferences (that give us a way to make sense of a situation), even though we cannot completely guarantee their soundness. Likewise we argue that there is a "method" of case studies in the sense of rules of thumb or a systematic plan for generating reliable case studies and hence for facilitating the relevant informal inferences.

2. Why We Might Need a Method of Case Studies. In community ecology we are unlikely to find many (if any) simple, exceptionless laws applicable to a variety of communities or species. One reason is that fundamental ecological terms (like "community" and "stability") are too vague to support precise empirical laws (see Shrader-Frechette and McCoy 1993, chap. 2). For example, although the term "species" has a commonly accepted meaning, and although evolutionary theory gives a precise technical sense to the term, there is general agreement in biology 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 (Cracraft 1983, 169–170; see also Ghiselin 1969, 1987; Gould 1981; Hull 1974, 1976, 1978, 1988, 102ff., 131–157; Kitcher 1985a; Mayr 1942, 1963, 1982, 273–275, 1987; Rosenberg 1985, 182–187; Simpson 1961; Sober 1981; Sokal and Sneath 1963; Van Der Steen and Kamminga 1991; Van Valen 1976). Such laws also appear unlikely because the apparent ecological patterns keep changing as a result of heritable variations and evolution (see, e.g., Mayr 1982; Rosenberg 1985; Ruse 1971, 1989; Sattler 1986, 186ff.; Simpson 1964; Sober 1988). Moreover, neither specific communities nor particular species recur at different times and places. Both the communities and the species that comprise them are *unique* (see, e.g., Norse 1990, 17ff. and Wilcove 1990, 83ff.). Of course, every event is unique in some respects (see Stent 1978, 219), and repetition of unique events is in principle impossible (Hull 1974, 98). Although—in terms of the covering-law model—initial conditions might be able to capture some of the uniqueness of an event, ecologists often do not have the historical information either to specify the relevant initial conditions or to know *what counts as* the unique event (see Kiester 1982, 355ff.). Consequently, instead of developing their own general theories and laws, ecologists are often forced to be content with a "user" science, a discipline based on borrowings and insights from other sciences.

Admittedly ecologists may apply useful findings about particular models to other situations, species, or communities. Nevertheless, such models are unlikely to help us develop general, exceptionless laws. One reason is that the ultimate units of ecological theory (e.g., organisms) are few in number as compared with the ultimate units in other scientific theories (e.g., molecules or subatomic particles), and they cannot easily be replicated. As a result, ecologists can rarely discount the random or purely statistical nature of events or changes; one disturbance in one key environment may be enough to wipe out a species. Model applications are also limited because we do not know the natural kinds. And if not, then perhaps the best paradigms of laws in ecology do not mention the species category at all. Eldredge (1985; see also Brandon 1990, 72ff.), for example, argues that because species are members of a genealogical hierarchy only, they do not take part in biological processes.

If exceptionless ecological laws are unlikely, and if there are problems with applying general ecological theory, given that species are not obviously natural kinds and that each individual in a population is unique, then apart from what sorts of claims are most appropriate to pure ecology (if there is such a thing), problem solving in applied ecology may require a new logic of case studies as well as a new method for helping to obtain reliable inferences. In this essay, we are more interested in describing, illustrating, and defending the methodological process leading to such inferences rather than in their epistemological status. Moreover, although there may be a variety of "logics" and associated methods able to encourage progress in ecology, there are four reasons that we are interested only in the "logic" and method characterizing case studies: (1) A discussion of the types of claims relevant to community, population, and ecosystems ecology is a difficult and massive undertaking, given the problems (already noted) with general ecological theory and concepts. (2) Others have already begun this undertaking (e.g., Van Der Steen and Kamminga 1991). (3) Our focus, instead, is primarily on the "logic" and method that might be most appropriate to the environmental problem solving of *applied* ecology. (4) Also, although a variety of logics and associated methods may be useful in applied ecology, our own field work (see Shrader-Frechette and McCoy 1993; McCoy et al. 1993), as well as the insights of a recent committee of the US National Academy of Sciences (NAS) and the National Research Council (NRC) (Orians et al. 1986) suggest that case studies may provide the best approach to applied ecology. Indeed, when it was asked to assess the use of ecology in environmental problem solving, the committee chose to illustrate how the practice of ecological science focused on case-specific ecological *knowledge*, rather than on the development or application of some general ecological *theory* (ibid., 1, 5). Faced with the absence of general ecological theory and laws available for environmental problem solving, the US NAS-NRC committee recognized that ecology's greatest predictive success occurs in situations having weak or missing general ecological theory and involving only one or two species (ibid., 8). These situations suggest that the success might be coming from sources other than the general theory: lower-level ecological theories and the natural-history knowledge of specific organisms (ibid., 13, 16; see also Gorovitz and MacIntyre 1976). As the authors of the National Academy report put it, "the success of the cases described . . . depended on such [natural-history] information" (Orians et al. 1986, 16).

3. The Method and "Logic" of Case Studies. The vampire bat research included in the NAS report is an excellent example of the value of specific natural-history and case-study information when ecologists are interested in practical problem solving (ibid., 28). Its goal was to find a control agent that affected only the "pest" species of concern, the vampire bat. The specific natural-history information useful in finding and using a control, diphenadione, included the following facts: The bats are much more susceptible than cattle to the action of anticoagulants, they roost extremely closely to each other, they groom each other, their rate of reproduction is low, they do not migrate, and they forage only in the absence of moonlight (Mitchell 1986). Rather than attempting to apply some general ecological theory, "top down", scientists scrutinized this particular case, "bottom up", in order to gain explanatory insights (see Kitcher 1985b; Salmon 1989, 384–409). The success of the NAS case study suggests that one important method of applied ecology, focusing on case studies, might be applicable in unique situations where we cannot replicate singular events. But what "logic" and method are appropriate to case studies? In subsequent paragraphs we shall attempt to answer this question. We shall use examples from the various analyses of Northern Spotted Owl conservation in the Pacific Northwest both to develop and illustrate our claims about the method of case studies and to motivate our discussion of the "logic" of case studies.

The survival of Northern Spotted Owls has been an increasing concern over the last two decades because timber harvests during this time have removed almost all the accessible lowland old-growth forest and forced the much-reduced Spotted Owl population to exist primarily in the rugged, mountainous old-growth forest of the Pacific Northwest. The basic problem facing applied ecologists studying the taxon is to determine how to protect resident populations of the Northern Spotted Owl so as to make policy recommendations that achieve both owl protection and the multiple uses of the forest required by law. To solve this problem, applied ecologists need to determine (1) habitat characteristics required for nesting and for successful survival; (2) successful owl dispersal and distribution; (3) owl population sizes able to withstand environmental fluctuations and random demographic changes; and (4) effective population sizes able to minimize genetic depression. Over the last 23 years, ecologists studying the Spotted Owl have made some progress in understanding these four issues. Regarding (1), for example, some ecologists concluded that Spotted Owls do not breed in young, second-growth forests (Salwasser 1986, 232). Using the framework of island biogeographic theory, the Interagency Scientific Committee to Address the Conservation of the Northern Spotted Owl drew a number of conclusions (typical of problem solving in applied ecology) regarding (1) through (4): that nesting and survival of the Northern Spotted Owl requires 191 habitat blocks of old-growth forest, each block 50 to 676,000 acres; that the blocks ought not be more than 12 miles apart, boundary to boundary; that the blocks (in Oregon, California, and Washington) need to be connected either by corridors or by "suitable forest lands" (with timber having an average diameter at breast height of at least 11 inches and with at least 40 percent canopy cover); and that habitat blocks need to contain at least 20 pairs of owls (Thomas et al. 1990). Congressional hearings (including examination of the key scientists involved in the Spotted Owl studies and recommendations), however, made it clear both that there is no confirmed general ecological theory to justify the conclusions and recommendations of the Interagency Committee and that even the best studies of the owl have been explicit neither about the logic underlying their conclusions nor about all of the methods used (US Congress 1990). Instead, the most prominent researchers on the Spotted Owl filled the gaps in their limited data with appeals to untested (often untestable) general theories such as island biogeography (Thomas et al. 1990; see US Congress 1990). In doing so, ecologists studying the owl came under attack for using general theories that were untested "in the real world", for employing inadequate "rigor", and for drawing conclusions unlikely to be supported by other reasonable persons (US Congress 1990, 260–296).

Even though neither a case-study method nor an associated "logic" is explicit and fully defended in any owl studies, we argue that by examining, evaluating, and making explicit various inferences in the best owl studies (for example, Dawson et al. 1987, Gutierrez and Carey 1985, Salwasser 1986, Thomas et al. 1990), we can "make sense" of many Interagency Committee conclusions (Thomas et al. 1990). In subsequent paragraphs, we develop and illustrate a method of case studies and a set of informal inferences ("logic") associated with it. Although space constraints prohibit our using Spotted Owl studies to illustrate this "logic" in more detail, we believe that our subsequent discussion may help to provide the rough outlines of a framework or "recipe" for using the method of case studies and its associated "logic" in other unique situations in applied ecology. In the following paragraphs, we shall outline a method of case studies, illustrating each step with examples from owl research, and then we shall discuss the informal "logic" associated with the method.

Campbell (1984, 8) claims that the method of case studies is "quasi-experimental"—an interesting choice of terms since ecologists sometimes classify their methods as "classical experimental", "quasi-experimental", and "observational" (see Parker 1989, 199). Classical experimental methods involve manipulation, a control, replicated observations, and randomization. Observational methods may not include any of these four components. Between these two methodological extremes lie quasi-experimental approaches, like the method of case studies, that embody some manipulations but lack one or more of the four features of classical experiments.

The method of case studies is (in part) "experimental"—as opposed to merely observational or descriptive—in that its goal is specification of cause-and-effect relationships by means of manipulating some of the variables of interest (Merriam 1988, 6–7). It is "quasi"-experimental, however, in that control of these variables often is difficult, if not impossible. In ecology, quasi-experimental methods often involve some manipulation and partially replicated observations. The interactions are complex (see, e.g., Levins 1968, 5ff.; McEvoy 1986, 83), and there are typically uncertainties regarding subject and target systems, boundary conditions, bias in the data and results, and the nature of the underlying phenomena (Berkowitz et al. 1989, 193–194). As a result, usually it is impossible to use either classical experimental or statistical methods or even to specify an uncontroversial null hypothesis (see Parker 1989).

In general, the case-study method aims at clarifying, amending, evaluating, and sometimes testing examples or cases. Unfortunately, in investigating particular cases, no simple logic, such as hypothesis-deduction, is applicable. Instead, one must follow a method, a set of procedures and rules of thumb, that help one to confront the facts of a particular situation and then look for a way to make sense of them through a set of informal inferences ("logic"). Often one knows neither the relevant variables nor whether the situation can be replicated. Indeed, in some of the best case studies performed (cited in the NAS report) ecologists remained divided even on the issue of the relevant variables. In the study of the Spotted Owl in the Pacific Northwest (Salwasser 1986), for example, some theorists claimed that limiting genetic deterioration is the most critical variable in preserving the owl and determining minimal population sizes. Other researchers, however, maintained that demographic (not genetic) factors are the most critical variables.

As a consequence of uncertainties about the relevant variables, researchers using the method of case studies have often been forced to use a "logic" of informal causal, inductive, retroductive, or consequentialist inferences in order to "make sense" of a particular example or situation (see Shrader-Frechette and McCoy 1993; Carson 1986, 36; Edelson 1988, xxxi–xxxii, 237–251; Gini 1985; Grünbaum 1984, 1988, 624ff.). In the Spotted Owl study from the NAS volume, for example, ecologists "made sense" of the situation by means of a number of inductive inferences based on observations about reproductive ecology, dispersal, and foraging behavior. As such, the inductive "logic" in the Spotted Owl case might be said to be "quantitative natural history" (see Ervin 1989, 86ff., 205ff.; Norse 1990, 73ff.; Salwasser 1986, 227). In using such informal inferences, the case-study analyst has two main objectives: to pose and to assess competing explanations for the same phenomenon or set of events and to discover whether (and if so, how) such explanations might apply to other situations (see Yin 1984, 16ff.). When they wrote *All the President's Men* (1974), for example, Bernstein and Woodward used a popular version of the method of case studies. They also used an informal "logic" to assess competing explanations for how and why the Watergate coverup occurred to suggest how their explanations might apply to other political situations (see Yin 1984, 24).

3.1. Five Components of the Method of Case Studies. In order to assess competing explanations of the same case, scientists must consider at least five factors: (1) the research design of the case study; (2) the characteristics of the investigator; (3) the types of evidence accepted; (4) the analysis of the evidence; and (5) the evaluation of the case study. The research design of the case study is a plan for assembling, organizing, and evaluating information according to a particular problem definition and specific goals. It links the data to be collected and the resulting conclusions to the initial questions of the study. Because the use of case studies is so new, however, no accepted "catalog" of alternative case-study research designs is available (see, however, Cook and Campbell 1979). Most research designs, nevertheless, appear to have at least five distinct components: (1) the questions to be investigated; (2) the hypotheses; (3) the units of analysis; (4) the "logic" linking the data to the hypotheses; and (5) the criteria for interpreting the findings. (See Edelson 1988, 278–308, 231ff.; Merriam 1988, 6, 36ff.; Yin 1984, 27, 29ff.)

In the Spotted Owl case study from the NAS volume, scientists addressed two main *questions*: (1) What are the minimal regional population sizes of owls necessary to ensure long-term survival? (2) What are the amounts and distribution of old-growth forests (the owls' habitat) necessary to ensure their survival? Although a given case study involves multiple *hypotheses*, one hypothesis in the Spotted Owl case was the following, "This particular Spotted Owl management area (SOMA) is supporting as many pairs of owls as expected on the basis of calculations of N_e, expected population size" (Salwasser 1986, 242). The *unit of analysis* in the Spotted Owl case study was the existing population of individual owls. The northwestern regional Spotted Owl population is currently estimated at approximately 2,000 in the US. In other case studies, the unit of analysis could be an individual organism. Or, there could be multiple units of analysis.

The most problematic aspect of the research design of a case study is the fourth component, the logic linking the data to the hypotheses. Essentially this "logic" is an informal way to assess whether the data tend to confirm the hypotheses. Because the auxiliary assumptions and controlling parameters in a case study frequently are not clear, and because a case study often represents a unique situation, scientists typically are unable to use hypothetico-deductive logic. Instead, they often are forced to use what Kaplan (1964, 333–335) calls "pattern" models of inference. Pattern models rarely give predictive power and instead enable us merely to fill in and extend data to formulate some hypothesis or pattern. For example, discussing the relationship between the annual number of traffic fatalities and automobile speed limit in the state of Connecticut, Campbell (1975) illustrated "pattern matching". Each of his two hypotheses—that the speed limit had no effect on number of fatalities, and that it had an effect—corresponded to a different pattern of fatalities. Although he was not able to formulate an uncontroversial null hypothesis and to test it statistically, Campbell concluded that there was apparently a pattern of "no effects" (Campbell 1969; see Yin 1984, 33–35). He simply looked at the number of fatalities, over nine years, and determined that there was no pattern, no systematic trend.

In using informal inferences to examine whether data are patterned, of course, one can always question whether an actual inference is correct. In the Spotted Owl case study, ecologists used a number of "patterns" from theoretical population genetics and ecology, including specific formulas for factors such as F, the inbreeding coefficient. Because some of the variables in the formula for F, for example, cannot be measured in wild populations, the ecologists' informal inferences about actual F are questionable (Salwasser 1986, 236). Likewise, although Campbell claimed, for example, that his data matched one pattern much better than another, it is not clear how close data have to be in order to be considered a "match". Campbell could not use a statistical test to compare his patterns because each data point in the pattern was a single number—fatalities for a given year—and he had five data points prior to the reduced speed limit and four after. These are an insufficient basis for reliable statistical testing. If one analyzes the literature on case studies, however, one can discover a number of criteria for assessing the quality of the case-study "logic", the informal inferences and the associated research design. Yin (1984, 35ff.) and Kidder (1981), for example, suggest construct validity, internal validity of the causal inferences, external validity or applicability of the case study, and reliability.[1]

One tests the reliability of the research design, for example, by using a protocol—an organized list of tasks, procedures, and rules that are specified ahead of time and that help one take account of all relevant variables and methods. In the Spotted Owl study from the NAS report, the protocol consisted of eight steps. One early step was to perform censuses (of the owl) on all national forest land. A subsequent step in the protocol was to perform a risk analysis of the demographic, generic, and geographical results of different management alternatives. The final step was to monitor the various SOMA in order to determine whether those managing the owls were achieving their goals (Salwasser 1986, 238–242). To test the reliability of the research-design "logic" requires developing, amending, and continually improving a data base against which the case-study findings can be reassessed. The final step in the Spotted Owl protocol, for example, described just such an updating and revision of the Spotted Owl preservation plan and conclusions. One also assesses the reliability, in part, by determining whether another researcher, evaluating the same case and the same evidence, would draw the same inferences or conclusions.

In analyzing the evidence used in the case study, the scientist employs three general analytic or methodological strategies. The first is developing a case-study *description* that is capable of organizing the data and hypotheses. In the Spotted Owl study the description emphasized the small size of the owl populations and their vulnerability as a result of habitat destruction, chance factors, and genetic deterioration. Formulating such a description presupposes both (1) developing categories that enable us to recognize and collect data; and (2) looking for regularities in the data. A second evidential strategy is *hypothesis formation*, using an inductive or retroductive "logic" to discover patterns or possible causal explanations for the data. Hypothesis formation often can be assisted by organizing inductive events chronologically, as a basis for time-series analysis, or by data-base management programs. In the Spotted Owl study, one important hypothesis assessed was that long-term protection of local populations of owls might require exchanging individuals among regional

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[1] One evaluates construct validity by employing multiple sources of evidence, attempting to establish chains of evidence and having experts review the draft of the case-study report. One tests for internal validity or causal validity by doing pattern matching, as exemplified in the Campbell case already noted, and attempting to provide alternative explanations (see Grünbaum 1984, 1988). One tests for external validity by attempting to replicate the case-study conclusions in other situations. To the extent that the case is wholly unique, however, replication will be impossible. Nevertheless, the findings of the case study may be valuable if they exhibit heuristic power.

populations. A third evidential strategy, *informal testing*, consists of using an informal "logic" to compare actual empirical results (e.g., survival of the owls) with the predictions generated by the case-study hypotheses and causal explanations. Available statistical techniques are not likely to be relevant here, because each data point in the pattern is probably a single point. Nevertheless, in the Spotted Owl case, for example, ecologists have been able to "test" their models of owl-population viability by using research on the long-term viability of other taxa. (See Lincoln and Guba 1985; Merriam 1988, 133ff., 13–17, 140ff., 147ff., 123ff., 163ff.; Yin 1984, 99–120; Salwasser 1986, 243, 232.)

After analyzing the evidence, the scientist can use an informal logic to draw conclusions and compose the case-study report. In the Spotted Owl case, one group of scientists concluded that internal factors (such as changes in fertility) and external stresses (such as habitat disturbances) increase the risk of extinction, that these factors can be offset only by immigration from other populations, and that regional populations of approximately 500 are necessary to protect the owls for several centuries (Salwasser 1986, 235–238). Of the main possible compositional forms of the case-study report—chronological, theory building, linear analytic, or comparative—the Spotted Owl report was a combination of theory building and linear analytic.[2]

The final component of the case-study method is to assess the report and conclusions. Often this can be accomplished by using the four criteria already mentioned for evaluating the "logic" underlying the research design: construct validity, internal validity, external validity, and reliability. One can also evaluate the case study in terms of the standard explanatory values, such as completeness, coherence, consistency, heuristic power, predictive power, and so on. Often these evaluations are best accomplished through outside review (see Merriam 1988, 170ff.). In the Spotted Owl study, the ecologists have an on-going plan of monitoring and research to evaluate critical assumptions in their conclusions, metapopulation models, and protocol (see Shrader-Frechette and McCoy 1993; Salwasser 1986, 242).

3.2. Shortcomings of the Method of Case Studies. As many scholars have noted, case studies can easily be biased by the practitioner (Grünbaum 1984, 1988; Callahan and Bok 1980, 5–62; Carson 1986, 37; Dalton

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[2] The chronological form consists of a case history in temporal order. Theory-building case studies provide an account of the case, usually in the form of causal claims. Linear-analytic case-study reports follow the format of research reports and grant requests. They cover a typical sequence of topics, such as the problem being studied, methods used, findings from the data, and significance and applications of the conclusions. Comparative case-study reports assess alternative descriptions or explanations of the same case (see Edelson 1988, 278–308; Merriam 1988, 185ff.; Yin 1984, 126–135).

1979, 17; Edelson 1988, 239–243; Gini 1985; Guba and Lincoln 1981, 377; Hoering 1980; Merriam 1988, 33ff.). Although there is no failsafe way to prevent all case-study bias, one way to deal with it is to realize that bias can enter the conduct of all science. Moreover, science does not require that scientists be completely unbiased, but only "that different scientists have different biases" (Hull 1988, 22). If they have different biases, then alternative conceptual analyses, accomplished by different scientists, will likely reveal these biases. Hence, it is important that practitioners of the method of case studies attempt to use an informal "logic" to confirm their results, in a partial way, by using independent data and other case studies. One also might avoid bias—or at least make it explicit—by developing rules for assessing similarities among system components, initial and boundary conditions, and by using multiple methods and multiple sources of evidence. (See Shrader-Frechette 1985, 68ff., 1991, chap. 4; Berkowitz et al. 1989, 195–197.)

Another response to possible bias in case studies is to realize that bias is possible only because of an asset: the flexibility of the method and its associated "logic" (de Vries 1986, 195). For example, in case-study work on the gopher tortoise, ecologists were able to discover a number of insights—such as the directional positions of entrances to tortoise burrows—even though they had neither algorithms nor a deductive logic to guide them (McCoy et al. 1993). Moreover, in certain situations that are unique and not subject to statistical testing, there is no alternative to the case method and informal logic. Because it is an *organized* means of obtaining information, because it can be criticized, and because it proceeds in a step-by-step fashion (problem definition, research design, data collection, data analysis, composition of results, and report), however, the method and its associated informal logic can be used in objective (i.e., unbiased) ways. For example, a number of scientists and philosophers of science have repeatedly argued that a given case study (a) does not illustrate what its practitioners claim; (b) does not fit the model imposed on it; (c) is factually deficient; (d) is a misrepresentation of the phenomena (see, e.g., Adelman 1974, Beckman 1971); (e) fails to take account of certain data (ibid.); or (f) has a "logic" that leads to inconsistency or dogmatism (Hoering 1980, 132–133) or that relies on faulty inferences—for example, the fallacy of affirming the consequent or the fallacy of assuming that two conjoint phenomena have a cause-effect relationship (see Edelson 1988, 255–266, 319ff.). Such criticisms indicate that, because use of the method of case studies, especially its associated "logic", is open to critical analysis and subject to revision, there are at least two ways in which it is rational and objective (in the Wittgensteinian sense of being "public"): (1) Expert practitioners are often able to distinguish a better application of the method and its logic from a worse one. (2)

Following the method, and thinking that one is following it, are not the same thing (see Baker and Hacker 1985, 150–185; 1986, 330–333; Wittgenstein 1973, s. 243ff.).

In arguing that the case-study method and its associated "logic" need not be subjective in a damaging sense, let us refer to the Wittgensteinian insight (see section 4 later) that objectivity is not tied to *propositions* but to the *practices* of people. As Wittgenstein puts it: "Giving grounds . . . is not a kind of *seeing* on our part; it is our *acting*" (1969, 204). Admittedly, our more traditional accounts of objectivity are tied to seeing, to mind-independent beliefs about the world, to impersonality, and to a set of judgements or logic. Typically we do not attach praise or blame, respectively, to *judgements* that fail to be objective in this traditional sense. The newer Wittgensteinian account of objectivity, however, is tied to actions, impartiality, and a method or procedure for behaving in a way that lacks bias. Usually we do attach praise or blame, respectively, to *persons* who fail to be objective in this sense (see Newell 1986, 63, 16ff., 23, 30). If a judgement is thought to be objective in the first sense, then obviously a single counterinstance can be enough to discredit it. Objectivity in this sense is not compatible with error. However, objectivity in the second sense is compatible with error. The upshot of distinguishing these two senses of objectivity is that the method of case studies—tied as it is to actions and practices rather than rules, propositions, or a deductive logic—is not infallible although it may be objective in a scientific sense.

Another problem with the method of case studies is that its associated logic provides little basis for scientific generalization (see Yin 1984, 21). In the Spotted Owl case study, for example, generalizations about habitat requirements were problematic because the owls' needs varied from place to place. In California, each pair of Spotted Owls used 1,900 acres of old-growth forest. In Oregon, the per-pair acreage was 2,264, and in Washington, 3,800 acres per pair (Wilcove 1990, 77). While concerns about the ability to generalize are well placed, this apparent problem with the "logic" of case studies is mitigated by two considerations. First, if Cartwright (1989), Fetzer (1974a,b; 1975), Humphreys (1989, 1991), and others are correct, then it may be possible to establish some reliable singular causal claims without first establishing regularities. Second, the single case study and the single experiment face the problem that both can be generalizable to theoretical propositions but not to populations or universes. Both face the problem of induction. Although the scientist must replicate a case or an experiment in order to generalize from it, mere replication is never sufficient for theorizing. The scientist's goal is not adequately accomplished merely by enumerating frequencies. Nevertheless, single cases and experiments, even in physics, are often sufficient for scientific theorizing. As Popper (1965, 28ff., 251ff.) pointed out, the severity of the tests, not mere replication, is important. Cases such as parity nonconservation (Franklin 1986, 100, 192ff.) and the Einstein-de Haas experiment (see Cartwright 1989, 349) likewise suggest that often controversies can be decided on the basis of a single convincing experiment. Hence it is not obvious that use of case studies is seriously flawed because its logic provides little basis for generalization.

The "logic" of case studies is also frequently criticized on grounds that it enables one to evaluate only those interpretations that use of the method of case studies already presupposes. However, any method of "logic" or confirmation is able only to evaluate hypotheses or interpretations that have already been discovered (see Hoering 1980, 135). Moreover, to the degree that the method of case studies, especially its associated logic, is open to critical evaluation, its conclusions are not merely begged and may, to some degree, be tested. M. Edelson and A. Grünbaum both provide insights regarding such testing. Recognizing that direct replication of a case study is typically impossible, Edelson argues persuasively that "partial replication", inference to the best explanation, and pitting a conclusion against rival hypotheses are all useful. Although Grünbaum claims that the data in an individual case cannot be used to *test* psychoanalytic propositions, for example, he maintains that one can use eliminative induction in experimental and epidemiological tests of the "logic" underlying the conclusions of a case study. One also can seek confirming instances or exclude plausible alternative explanations. Although Edelson and C. Glymour recognize that "testing" case-study conclusions is difficult, they appear unwilling to relegate the "logic" of case studies only to the context of discovery. Doing so likely would discourage rigorous argument about the relationship among hypotheses and evidence and might also force us to presuppose an account of testing that was rarely applicable in science. (See Edelson 1988, 120, 363, 231–265, 275–276; Grünbaum 1984; Glymour 1980; Meehl 1983.)

Another allegation against the "logic" used in the case-study method is that, because it follows no purely deductive scheme of inference, its practitioners often fall victim to uncritical thinking (see Francoeur 1984, 146; Hoering 1980, 135), erroneous inductive inferences, or the fallacy of false cause (see Grünbaum 1984; 1988). Community ecologists, for example, have been divided recently over the causal role of competition versus random chance in structuring biological communities. As a consequence of this division, different ecologists (using the same case study) often make incompatible and controversial inductive and causal inferences regarding alleged competition data (see, e.g., Diamond and Case 1986; Simberloff 1976; Simberloff and Abele 1976). The solution to such controversies, however, is not to abandon the method of case studies, but to subject its "logic", especially its problematic inferences, to repeated criticism, reevaluation, and discussion—to seek independent evidence and alternative analyses of the same case (see Edelson 1988, 237–251, 286ff., 319ff.). Grünbaum's (1984, 1988) criticisms of many of the central causal and inductive inferences of psychoanalysis, for example, provide an alternative to the case analyses provided by doctrinaire Freudians. Likewise, in evaluating evidence of a species' shared genealogy, Sober (1987, 466; 1988) has discussed in detail which sorts of causal inferences are justified and which are not. In general, much of the literature (like Sober's) that discusses problems with the "principle of the common cause" (see Reichenbach 1956, Salmon 1984) or with inductive inferences helps us avoid questionable inferences in assessing case studies.

4. The Scientific Status of the Method of Case Studies, Especially Its Underlying Logic. Although the method of case studies typically employs a "logic" of informal inferences that are difficult to evaluate, the method has a number of assets. (1) It enables scientists to gain a measure of practical control over real-world problems, like pest management. (2) Its associated "logic", a set of informal inferences, often allows us to make rough generalizations (that often suffice for sensible explanations), for example, about a taxon's susceptibility to anticoagulants, even though exceptions cannot be treated in a systematic way (see Van Der Steen and Kamminga 1991). (3) By facilitating such inferences, the method shows us how to use descriptions of a particular case in order to study different, but partially similar, cases. Apart from practical benefits, the method of case studies is also important because its systematic procedures and "logic" are applicable to unique situations that are not amenable to replication, statistical testing, and the traditional logic associated with hypothesis testing. The method provides an organized framework for consideration of alternative explanatory accounts, for doing science in a situation in which exceptionless empirical laws typically are not evident or cannot be had. Case studies enable us to learn about phenomena when the relevant behavior cannot be manipulated, as is often the situation in ecology (Yin 1984, 19). Case studies are also valuable for some of the same reasons that Kuhnian "exemplars" are important. They show, by example, how the scientific "job" is to be done, how one problem is *like* another (see Kuhn 1970, 187–191). A final benefit of the method is that, although its "logic" is often unable to provide information about regularities or to confirm hypotheses, it does enable us to see whether a phenomenon can be interpreted in the light of certain models or assumptions (see Mowry 1985). More generally, as the authors of the NAS study on applying ecological theory put it: "The clear and accessible presentation of the [case-study] plan . . . focuses the debate and research" (Orians et al. 1986, 247). It enables us to deal with a full range of evidence in a systematic, organized way and therefore to uncover or illustrate crucial details sometimes missed by more formal methods of science.

Despite these benefits of the method of case studies, critics are likely to object that there is no rationale for claiming that either its method or its "logic" is *scientific*. Moreover, any comprehensive defense of the "logic" of case studies would require us to defend some causal account of explanation as well as some account of the type of generality we expect in causal claims in applied ecology. We need to have an account, both of how causality operates and of how at least two independent avenues function, in causal explanation, so as to advance our understanding of phenomena. Nevertheless, no fully developed, specific account of causality is yet available (Salmon 1989, 409). Because it is not, we shall not provide much insight into the sorts of claims most appropriate to applied ecology: causal claims made on the basis of inductive and retroductive inferences and rough generalizations made on the basis of descriptions of natural history. Instead, we shall attempt to show merely that a causal account of scientific explanation—focused on the singular claims and unique events of case studies—is prima facie plausible for at least four reasons. First, pragmatically speaking, many complex situations (like those in ecology) have no obvious other "logic" that appears applicable. Second, there is no recipe for moving from singular claims to abstract regularities. Third, even general laws rely in part on scientific practice to specify how to apply them. Fourth, even general causal laws require reference to singular claims, if they are to work.

One of the strongest arguments that the "logic" of case studies is scientific is pragmatic. The "logic" is more useful, appropriate, and workable than others—such as hypothesis-deduction—when dealing with unique situations in which testing and experimental controls are impossible (see Merriam 1988, 20–21). In other words, in many situations, we have no reasonable alternative to the logic of case studies.

A second, Wittgensteinian-Kuhnian justification for the logic of case studies is that it is scientific by virtue of being embodied in the practices (the relevant actions and dispositions) of the scientific community, such as its ability to "see" situations as exemplars, as like each other (see Kuhn 1970, 191–204). As both Kuhn and Wittgenstein showed, there is room in scientific method for the rationality of *practice*, for a way of grasping a rule that is exhibited in obeying the rule, rather than in being able to formulate it (see Baker 1986, 255). Moreover, because this behavior involves an implicit reference to a community of persons (as Bloor 1983, Kripke 1982, Peacocke 1986, and Smith 1988 suggest), case-study practices need not be purely subjective (see Newell 1986). Another Wittgensteinian criterion for the correctness of case-study practices is whether they enable us "to go on" to "make sense" of further practices and to see likenesses among different cases (Ackermann 1988, 131; Eldridge 1987; Wittgenstein 1973, 47–54, 1979, 61, 77–79).

A third prima facie reason for believing that the method of case studies and its associated "logic" need not be dismissed as nonscientific (because it has no exceptionless rules to guide practices) is that no science relies solely on exceptionless rules. This is because, as Kripke (1982) and Cartwright (1989) point out, no rule can determine what to do in accord with it, because no rule for the application of a rule can "fix" what counts as accord. Therefore, every rule generates the same problem: how to apply it. If the practices of experts ultimately guide scientists in applying rules, then it is reasonable to believe that practices can also guide scientists in applying the "logic" of case studies (see Baker 1986; Baker and Hacker 1985, 1986; Picardi 1988; Smith 1988; and Wittgenstein 1973). Hence the "logic" of case studies may be appropriate to science if one conceives of scientific justification and objectivity in terms of method, in terms of *practices* that are *unbiased*—rather than in terms merely of a set of inferences, *propositions* that are *impersonal*. For Wittgenstein, practices are normative and not purely subjective in part because their existence requires multiple (not unique) occasions (Baker and Hacker 1985, 151). Although the "logic" of case studies is typically applied to a unique phenomenon, however, even accounts of unique events may be "tested" indirectly (see Olding 1978) on the basis of past scientific practices, heuristics, or "rules of thumb". Moreover, as mentioned in section 2, even unique events sometimes may be explained if one is able to supply appropriate initial conditions (see, e.g., Fetzer 1975, Van Der Steen and Kamminga 1991).

Fourth, general causal laws require reference to singular claims, if they are to work. All forms of inference, whether deductive or inductive, presuppose the recognition of regularities, parallel cases, and this recognition presupposes recognition of similarities and differences (see Bambrough 1979, chap. 8; Dilman 1973, 115–120; Fetzer 1975, 95–96; Kuhn 1970, 1977; Newell 1986, 88–94; Wisdom 1965). Deduction addresses *all* possible singular instances that support a particular claim, whereas case "logic" addresses only *some* of the singular instances, often one at a time. Both may transmit truth, but neither is capable, alone, of initiating it. Initiating truth requires some sort of tacit knowledge in the form of both ultimate premises and knowing what explains a particular case or cases. Indeed, all knowing requires some appeal to tacit knowledge.[3] Whenever one *ap-*

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[3] Tacit knowing is required, for example, when we discover novelty, reorganize experience, understand symbols, distinguish what is meant from what is said, and understand in a gestalt or wholistic way (Polanyi 1959, 18–29). Tacit knowledge is also required when we value something, know reasons and not merely causes, understand subsidiary

plies a generalization, law, or theory to a particular case, as Kripke (1982) realized, one must use tacit, not explicit, knowledge. Likewise tacit knowledge tells a scientist what needs to be explained and what counts as a criterion for justification. As Wittgenstein pointed out in discussing the foundations of mathematics, even mathematical proofs proceed by means of analogy, by means of the tacit knowledge that one case is like another. Hence, if there is a problem with the tacit knowledge that characterizes the "logic" of case studies, then there is likewise a problem with the tacit knowledge that underlies all science. (See Newell 1986, 92–110; Polanyi 1959, 13, 26, 1964; Adelman 1974, 223; Gutting 1982, 323; Bloor 1983, 95.)

Admittedly one might object that, although tacit knowledge is necessary for all science, it is not sufficient. In other words, in contrast to deductive scientific logic which employs tacit knowledge, the "logic" of case studies bears the additional burden of being able, at best, only to show the rationality of a particular scientific conclusion, not to confirm it (see Plantinga 1974, 220–221). Merely being able to show that particular conclusions are rational, however, need not count against a scientific "logic". For the "logic" to be defective, confirmation—or *something more* than an illustration of rationality—must be possible in the situations in which it is used. As with the Spotted Owl case, it is not obvious that there are deductive methods and associated logic able to confirm hypotheses in situations in which the method of case studies and its associated logic are used.

5. Conclusion. Obviously the best way to defend a "logic of case studies" is to illustrate what it can do in a real scientific situation, such as the case of preserving the Pacific Northwest Spotted Owl or controlling the vampire bat. Both studies—although merely sketched here—showed that practical and precise knowledge of particular taxa is often important to the practice of applied science when no general ecological theory is available. This practical and precise knowledge—rules of thumb and informal inferences based on natural history—coupled with the conceptual and methodological analysis typical of the case study, is an important departure from much earlier ecological theorizing based on untestable principles and deductive inferences drawn from mathematical models. Moreover, informal inferences based on natural history are often more capable of being realized in contemporary community ecology than are hypothetico-deductive inferences based on exceptionless general laws. Important ideals for ecological method and its associated "logic", clas-

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rather than focal points, grasp unique things, and make methodological value judgements in science—e.g., "reagent x is the best one for the next test" (ibid., 38–93).

sical testing and use of null models sometimes fail to address the uniqueness of many ecological phenomena and the ambiguity of many ecological concepts. Hence, in addition to a logic of justification, applied ecology—and perhaps other areas of science—need a "logic" of case studies.

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