Conceptual exploration: An autobiographical narrative

from

UNRULY COMPLEXITY

ECOLOGY, INTERPRETATION, ENGAGEMENT

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PROLOGUE

My decision to study ecology during the early 1970s stemmed from environmental activism in Australia that ranged from a collaboration with trade unionists opposing the construction of an inner-city power station to street theater exposing fraudulent, industry-sponsored recycling plans (Whole Earth Group 1974). Ecology-the-science was the recommended choice for college students who sought programs of study in which to pursue their interests in ecology-as-social-action—if indeed any other choices were available. I hoped my studies would lead to some kind of career that would take me beyond responding to one environmental issue after another and instead allow me to help in planning that prevented future problems from emerging. I also hoped that understanding how to explain the complexities of interactions in life would lend support to less hierarchical and exploitative relationships, both within society and among humans and other species.

I had brought a mathematical disposition to my studies in ecology, so I undertook projects that advanced my skills in quantitative analysis and mathematical modeling. I was excited to learn that some biologists and mathematicians were creating a specialty called theoretical biology (Waddington 1969). This discovery was still fresh when I took a course for which E. C. Pielou's (1969) text on mathematical ecology was assigned. In the introduction she noted that organisms come from a range of species; within any species they differ in age, sex, genetics, experience, and so on; and any particular individual changes over its lifetime. Any situation an ecologist might study is continually altered by births and deaths, by migratory exchanges with other places, and by seasons and climatic change. Even so, ecological regularities persist long enough for most people to recognize some order, such as, an oak-maple forest or the sequence of plants encountered as one moves inland from the seashore (Pielou 1969, 1). The processes could be simply described, yet the combination of them seemed theoretically challenging—how could ecologists account for order arising out of such complexity?…(to be continued)

[the dashed line indicates that a body of text stands in the book between the preceding and following sections of the narrative]

My undergraduate studies had raised the theoretical question of how ecologists could account for order arising out of the complexity, but the jobs I applied for after graduation were more practically oriented. Environmental planning scarcely existed in Australia in the mid 1970s and I found employment in agricultural research. My first job was to extract patterns from data about the complexity of interactions between plant varieties and field conditions in large crop trials. My second job involved modeling the economic future of an irrigation region suffering from soil salinization (a project analyzed in chapter 4). To my frustration, the government sponsors of the salinization study turned out to be interested only in a small subset of the factors and policies potentially relevant to the region's future. This experience in analysis and planning led me to seek opportunities for self-directed inquiry in ecological theory. At the same time, the experience motivated me to explore ways that social influences could shape ecology and environmental science in less constraining ways.

My interest in understanding science in its social context had already been stimulated by the advisor of my undergraduate thesis in ecological modeling, Alan Roberts, a physicist who also wrote about environmental politics and the need for the self-management of society (Roberts

1979). From Roberts and others I was learning that through the course of history all kinds of social lessons had been read from nature (Williams 1980). It would be better to argue directly for, say, cooperative, decentralized social relations than to put forward some account of ecological complexity to justify them. Neverthless, I could still envisage research on complexity challenging the simple scientific themes that were often invoked in support of social inequalities and exploitation of nature (Science for the People 1977). As I was finishing the salinization study in 1979 I learned that two biologists in the United States whose theoretical work I already knew and valued, Richard Levins and Richard Lewontin, saw their scientific work as a political project (Levins and Lewontin 1985; Taylor 1986). I sought an opportunity to study with them. This would draw me away from environmental activism in Australia, but this leave—which has extended longer than I could have imagined—would provide space to focus on questions around conceptualizing life's complex ecological context and to begin to take up questions of conceptualizing science's complex social context…

PART I
MODELING ECOLOGICAL COMPLEXITY

Ecological complexity poses challenges to conventional scientific ways of knowing. Ecology is not like thermodynamics, in which complexity can be simplified through statistical averaging of large numbers of identically behaving components. Moreover, whereas progress in the physical sciences depends greatly on controlled experiments in which systems are isolated from their context, this strategy is not so clearly appropriate for understanding organisms in a context of interactions with multiplicity of hazards and resources distributed in various ways across place and time. At the same time, analysis of observations from non-experimental situations is beset by circularity—ecologists need to be know a lot in advance about the causal factors before they can design methods of multi-variable data analysis capable of revealing the effects of those factors.

If ecological complexity does not lend itself well to statistical simplification, experimental control, and multi-variable data analysis, it was fair to ask whether any general theories of its structure could apply. During the 1960s and 1970s many academic ecologists, especially in the United States, had thought such theories were indeed possible. In systems ecology, complexity was analyzed in terms of the nutrient, energy, and information flows among the living and non-living entities that make up the entire ecosystem (to be discussed in Chapter 3). In community ecology, analyses focused on a part of the ecosystem, namely, on some group of interacting populations or ecological community. Theoretical propositions concerned population sizes and distributions and their regulation through inter-species interactions—chiefly competition for limiting resources. Elegant, widely applicable principles of ecological organization were sought. Robert MacArthur, following the lead of his teacher G. Evelyn Hutchinson, was a leading proponent of expressing such principles as verbal or mathematical models: "Will the explanation of these facts degenerate into a tedious set of case histories, or is there some common pattern running through them all?" (MacArthur 1972, 169). By using mathematical equations to focus attention on certain entities and relationships in ecology, MacArthur, Hutchinson, and other ecologists encouraged mathematicians, including a number of my teachers in Australia, to try their hand at ecological theorizing.

In the early 1980s—the time when I began doctoral studies in the United States—ecologists of a particularistic bent were vigorously questioning ecological principles and expressing skepticism about the possibilities of general ecological theory. Daniel Simberloff (1982), for example, argued as follows: Many factors operate in nature, and in any particular case at least some of them will be significant. A model cannot capture all the relevant factors and still have general application. Instead, Simberloff contended, ecologists should intensively investigate the natural history of particular situations and test specific hypotheses about these situations experimentally. Ecologists may be guided by knowledge about similar cases, and they may end up adding to that knowledge, but they should not expect their results to be extrapolated readily to many other situations.

The tension between the MacArthurian tradition and the newer critiques stimulated me to clarify relations intended between models and the reality to which they refer—this is the subject of chapter 2. Yet, despite the particularistic challenge to models, I remained interested in fundamental questions in ecological theory. One such question was how explanations that involve interacting causes can be extracted from ecological patterns and data. Another question—which underlies the two parts of chapter 1—concerned the consequences of defining boundaries between the outside and inside of a system when ecologists attempt to account for ecological structure or organization…

CHAPTER 1

PROBLEMS OF BOUNDEDNESS IN MODELING ECOLOGICAL SYSTEMS

Two conclusions had emerged from my exploration of the consequences of ecologists defining boundaries between the outside and inside of a system when they attempt to account for ecological organization:

— Ecologists interested in explaining the persistence of complex communities need to examine not only the current configuration or "morphology" of that complexity but also its construction over time—its contingent history of becoming structured and its ongoing restructuring in a wider spatial context or "landscape."

— Ecologists need to recognize that principles derived from analyzing simple sub-communities can be confounded by the dynamics of populations with which those sub-communities interact in naturally variable and complex ecological situations.

It seemed that construction over time together with embeddedness in a dynamic context, which links the system that is the focus of research with the backgrounded processes, have potentially profound implications for knowledge-making: Theorists should not assume that ecological complexity can be partitioned into communities or systems that have clearly defined boundaries, coherent internal dynamics, and simply mediated relations with their external context.

Around this time—the mid-1980s—I became aware of the work of the anthropologist, Eric Wolf, which primed me to look in areas other than ecology for ways to think about problematic boundaries: "'Societies' emerge as changing alignments of social groups, segments, and classes, without either fixed boundaries or stable internal constitutions." If anthropologists observe "transgenerational continuity, institutional stability, and normative consensus," they should seek "to understand such characteristics historically, to note the conditions for their emergence, maintenance and abrogation. Rather than thinking of social alignments as self-determining... we need... to visualize them in their multiple external connections" (Wolf 1982, 387). In other words, whenever theory has built on the dynamic unity and coherency of structures or units—in Wolf's case, societies or cultures—researchers could invert this and consider what would follow if those units were to be explained as contingent outcomes of intersections among processes that implicate or span a range of spatial and temporal scales. As will emerge later (Chapter 5, section C; Chapter 6, section A), socio-environmental studies proved to be a more fertile field than ecology proper for me to elaborate on Wolf's conceptual inversion and paint a picture of intersecting processes (see, e.g., Little 1987; Peet and Watts 1996; Taylor and García-Barrios 1995). Nevertheless, even within ecology proper, my inquiry into the relations between models and the reality to which they refer (Chapter 2) would lead me again to the core conceptual issue of embeddedness in a wider context…

CHAPTER 2

OPEN SITES IN MODEL BUILDING

Boundaries become problematic when they discount history, embeddedness in a spatial context, or the dynamics of variables not explicitly included in the models —such was the conclusion I drew from the modeling presented in Chapter 1. This perspective is especially challenging for mathematical modelers because the assumption of a fixed, delimited set of components is almost required for formulating and analyzing a mathematical model. Recognition of this fundamental limitation of ecological modeling started me thinking about the need for some ongoing process of assessment, reformulation, and reassessment. I was primed to notice analogous problems when I ventured into the interpretation of science and other fields (Chaps. 3-6).

I did not, however, abandon modeling and adopt the view that ecology should consist of particular case histories. Models had proved valuable; my understanding of problematic boundaries had been derived through theorizing that centered on models. With regard to the construction of complexity, I had followed the tradition in which simple models are used to seek qualitative and general insights (Chapter 1, section A). To understand the sensitivity of principles ecologists derive about sub-communities to the dynamics of the other populations in the community I had explored a model world in which I knew the complete dynamics, but the hypothetical ecologists analyzed data from the sub-community only (Chapter 1, section B).

These two modeling exercises were complemented by earlier experience in agricultural research. When I had to extract patterns from data on the complexity of interactions between plant varieties and field conditions in large crop trials I followed the lead of certain vegetation ecologists. These researchers had used models to generate artificial data, which allowed them to examine the sensitivity of their multivariate data analysis techniques to built-in assumptions about the nature of the causes that the techniques are intended to expose.

My awareness of the tension between the productive potential of model-based theorizing and its limitations led me to try to make sense of the positions that philosophically minded ecologists were staking out during the 1980s. What do models model? This is the subject of chapter 2...

My approach to philosophy of modeling depended on first classifying different things that ecologists do when they build models to represent ecological complexity. There was an important place in this classification for exploration of concepts and the generation of theory; ecologists discount this dimension of science when they emphasize testing specific hypotheses about particular situations. I was also able to identify several *open sites*, in which considerations other than analysis of a model's correspondence with evidence must come into play. In other words, knowledge-making must always extend beyond the dialogue between models and evidence. By analogy with a theme from my account of apparent interactions in ecology (Chapter 1, section B), the dynamics of the wider influences on knowledge-making may confound any philosophical analysis of modeling and theory-building that leaves those influences hidden. My recognition that dialogue is embedded in a dynamic social context opened up the larger project that is the subject of Parts II and III...

PART II
INTERPRETING ECOLOGICAL MODELERS IN THEIR COMPLEX SOCIAL CONTEXT

The motivation for the modeling efforts described in Part I was to account for order in ecological complexity. However, after sites are identified where considerations other than explicit analysis of a model's correspondence with evidence must come into play (Chapter 2), a wider exploration of ecological theorizing is opened up: What factors influence the decisions that ecologists make about which questions to put to nature, categories to use, observations to construct, analyses to perform, degree of confirmation to require, and ways to revise models? Is the effect of these factors on ecological science merely idiosyncratic and transient, or are there systematic patterns? Could awareness and discussion among ecologists of any systematic effects influence their subsequent science in productive ways? In particular, could such a wider contextualization of ecological theorizing help ecologists address the challenge of making sense of ecological complexity that involves ongoing restructuring and embeddedness (Chapter 1)?

I had the opportunity to examine these new questions during research fellowships in two interdisciplinary programs. The first step I took was to relate my interests to the existing approaches in the interpretation of science. Some scientists and philosophers concerned with scientific method identified the different theoretical heuristics applied in science and compared their effectiveness in establishing knowledge (Bechtel and Richardson 1993). This approach focused on the dialogue between theories and the evidence about reality to which they refer. Historians and sociologists of science, on the other hand, tended to focus on interactions among members of scientific communities during disputes and dialogue around methods, observations, and conclusions. The resulting interpretations of science invoked a wide range of social factors—mentoring and favoritism, competition for prestige and publicity, government or corporate funding decisions, gender relations and class interests, and so on.

It was clear that scientists and philosophers of science tended to assume—as do some historians and sociologists of science—that scientists' contributions to the dialogue between models and evidence they refer to can be separated from their dialogue with other scientists to establish what counts as knowledge. This separation of the *referentiality* of science from its *socialty* might occur in a number of ways: The effect of decisions made at what I called the open sites (Chapter 2) leads some scientists to tackle anomalies that others had dismissed as negligible and thus ensure that science progresses. In any community of scientists, disputes are resolved when one scientist's biases are countered by those of others, thus science self-corrects. Social influences, such as research funding, merely inhibit or accelerate improvement in scientific knowledge. In short, although science is a social endeavor, its referentiality still determines what counts as knowledge—if not immediately, at least in the long run.

I was more interested, however, in a deeper sense of sociality that makes it harder to keep socialty and referentiality separate. All scientists engage in various arenas of social activity—they build careers and institutions, use and transform language, facilitate policy formulation, and so on. This context means thatscientists select problems, define categories, collect data, and present findings not only to develop models of their subject matter, but also to secure the support of colleagues, collaborators and institutions, and enable others to act upon their conclusions. I realized that this might happen in idiosyncratic ways, but it was also possible that the simultaneous pursuit of referentiality and social support could sometimes lead to systematic and enduring effects on the content of scientific knowledge. I wanted to attempt to demonstrate and analyze such effects—the subject of Part II—for two reasons: It would be harder to dismiss as “insignificant in practice” the conclusion from Chapter 2 that considerations other than explicit analysis of a model's correspondence with evidence must come into play. It would also open up the possibility that ecologists who theorize about ecological complexity—or researchers more generally—might be encouraged to use awareness of such effects to modify their own work. This possibility is taken up in Part III...

CHAPTER 3

METAPHORS AND ALLEGORY IN THE ORIGINS OF SYSTEMS ECOLOGY

I began to explore the effects of the sociality of science on its content in the field of systems ecology, in particular, in the work of H. T. Odum, a pioneer in systems ecology in the United States. Although this field emphasizes nutrient and energy flows, which were not often examined by the modelers in the stability-complexity debate stability was a central concern of Odum. Moreover, the shift to a field that explicitly considered entire complex systems allowed me to focus on the implications of partitioning complexity into systems assumed to have clearly defined boundaries, coherent internal dynamics, and simply mediated relations with their external context.

As an interpreter of science I looked for correlations between scientific ideas and the scientist's social context and personal history. The scientist in me, however, was interested not only in correlations, but also in the mechanism or dynamics producing them. As I interpreted Odum's work I also sought a plausible model of the dynamic relationships among his social and scientific ideas and practices…

CHAPTER 4

RECONSTRUCTING HETEROGENEOUS WEBS IN SOCIO-ENVIRONMENTAL RESEARCH

After considering Odum's practice—his methods and organization of research as well as his concepts and production of theory—I saw him as a person or *agent* working to make the overlapping realms he inhabited—the social, personal, and scientific—reinforce each other, so that efforts made and directions pursued in one realm did not undermine those in the others. In my interpretation, many aspects of the post-war setting for Odum's early research enabled him not only to think that ecosystems were like well-designed feedback systems (or circuits), but also to *act as if* they were—He was able to find in nature a special role for systems engineers, such as himself, working in the service of society.

Yet I wondered about the generality of this model of scientific and social agency. Could I show reinforcement across realms in cases where the social-personal-scientific correlations were less obvious or less consistent over time than in the case of Odum's scientific work? In this line of inquiry about scientists as social agents I followed the lead of sociologists of science, especially sociologists of scientific knowledge, who had been formulating vocabulary and propositions about how scientists in practice establish knowledge (Collins 1981a).

Another challenge remained after interpreting Odum's work. I had shown that the sociality of science could affect the content of scientific knowledge, but my original motivation was to bring such interpretations to bear productively on subsequent research. In this regard, the case of Odum provided limited guidance. Personal, scientific, and social considerations reinforced each other so consistently in Odum's life and work that it was difficult to see how he could have done anything differently. At best, I could have used my interpretation of Odum to suggest a very broad lesson: Scientists opposed to technocratic rationality should not treat ecological complexity as if it were made up of well-bounded systems that could be analyzed in terms of a single currency. Yet, any scientists who wanted to heed such a lesson would still need specific ways to arrange or alter the personal, scientific and social facilitations of their work. To provide insights about how that might be achieved, a finer-grained analysis than the broad historical interpretation of Odum seemed to be called for.

With these two challenges in mind I chose to consider two projects of *socio*-environmental assessment likely to be governed by more complex and contested pragmatics. The first case was the modeling work I had undertaken in Australia, in a project analyzing the future of a salt-affected agricultural region (section A); the second involved U.S. researchers in the mid 1970s building computer models of nomadic pastoralists (lifestock herders) in drought-stricken sub-Saharan Africa (section B)…

My investigations of how the models in the two socio-environmental modeling projects came to be established as knowledge centered on assessment of what would be entailed *in practice to modify that knowledge*. By identifying alternatives to specific aspects of the modeling projects and teasing out their practical implications I was able to trace diverse interconnections between the various so-called technical tasks of scientists and the social considerations that influence how scientists perform these tasks. The terms and themes I formulated emphasized that scientists harness many *diverse resources* in establishing knowledge. This process of *heterogeneous construction* is always, *in practice*, bound up with construction of lives, careers, institutions, language, ideologies, societies, that is, with a range of actions and *engagements*. Scientists are simultaneously *representing and engaging*. In this sense, the work of modelers embedded in a social context became a variant of *intersecting processes* (see the narrative at end of Chapter 1 and Chapter 5, section C)—a variant whose interpretation requires special attention to the agency of the modelers.

At this point I saw these scientists' agency as something *distributed* beyond their persons, depending on webs of resources, such as the available computer compiler, published data, length of study time set by the sponsors, and so on. This view, which extends the themes of heterogeneity and embeddedness (Prologue and Chapter 1), contrasts with the idea of agency as something *concentrated* inside scientists' minds in the form of motivations, beliefs, perspectives, biases, or ideology. Concentrated agency steers attention towards verbal and textual discourse, but I was choosing to emphasize the diverse material aspects of practice relevant to constructing knowledge.

The shift I had made away from overall correlations between scientific content and social-personal context to the picture of heterogeneous construction had been accompanied by three other shifts of emphasis. The first was that the ecological theorizing discussed in part I had given way to a focus on research on socio-ecological complexity—first Odum's "systems of man and nature" then projects of socio-environmental assessment. I had not left my interest in ecological theory behind; indeed, this new focus had provided material in which I could explore not only the sociality of science but also the problematic boundaries of ecological or environmental complexity. My analysis of alternatives to system dynamics modeling of nomadic pastoralists had acquainted me with the field of political ecology, in which cases of environmental degradation were explained by linking local changes in agro-ecologies, labor supply and the organization of production with wider political-economic conditions (Peet and Watts 1996a). In short, this was an area giving substance to Wolf's image of intersecting processes that involve diverse components and span a range of spatial and temporal scales (Wolf 1982, 387; see narrative at the end of Chapter 1).

I noticed some affinity between this first shift and steps being taken by ecologists developing an approach called Adaptive Environmental Management (AEM) (Holling 1978). AEM promoted use of multiple models and their ongoing revision in recognition that any ecological situation is a moving target—not the least because management practices produce continuing changes. In my terms, AEM was addressing the ongoing restructuring and embeddedness of ecological situations. (Subsequently AEM has evolved into a field that advances models of the social or institutional embeddedness of research and policy.) At that time, however, I was less enthusiastic, about AEM's orientation toward environmental *management*. My critical perspective on the technocratic orientation of the socio-environmental research of Odum, Picardi, and the Kerang Farm study (Chaps. 3 and 4) motivated a second shift in emphasis, namely, to look for examples of representing and engaging in ecological situations that were less technocratic—cases in which researchers bridged the divide between outside analysts and the subjects whose social and ecological situation was being analyzed. In this vein, I was inspired by cases of participatory action research (PAR) in which the researchers shaped their inquiries through ongoing work with and empowerment of the people most affected by some social issue (Adams 1975; see Epilogue, sections A and B).

The final shift of emphasis was that assessment of what would be entailed in practice to modify the knowledge produced by the modelers primed me to reflect on the social considerations that shaped my own research as an interpreter of science. My efforts at self-conscious or reflexive engagement with ecological and social complexity, which is the subject of Part III, were informed by the PAR ideal…

PART III
ENGAGING REFLEXIVELY WITHIN ECOLOGICAL, SCIENTIFIC, AND SOCIAL COMPLEXITY

One motivation for the efforts interpreting the sociality of science presented in Part II was the possibility that awareness and discussion among ecologists of such interpretations might influence their subsequent work in productive ways. It was still an open question how best to feed interpretation back into science. On one hand, interpreting the two socio-environmental projects (Chapter 4) had led me to identify alternatives to specific aspects of the modeling methods. These alternatives pointed to the possibility of representing complexity without assuming the existence of well-bounded systems, an assumption that my earlier modeling and historical work had also called into question (Chapters 1 and 3). I knew that scientists—including the scientist in me—would like to see what modeling built around the alternatives would look like. On the other hand, interpreting those modeling projects had also heightened my awareness of the diverse practical considerations and interactions among diverse social agents involved in establishing what counted as knowledge. I decided, therefore, not simply to focus on ways ecological and socio-environmental complexity could be modeled so as to capture ongoing restructuring, heterogeneity, and embeddedness. I also needed to explore the potential of heterogeneous constructionist interpretations to expose many specific sites of scientific practice at which different researchers—interpreters of science as well as scientists—could engage with a view to modifying the science (Chapter 4). I would need to get more interpreters of science interested in analyzing scientists' diverse resources and making sense of their distributed agency. I would also need to get scientists to pay attention to such interpretations—even better, to become interpreters of the construction of their own work, that is, to become *practically reflexive*. And I would need to follow through the implications of such reflexivity, which included modeling what I wanted for scientists in the ways I interpreted and engaged with science…

CHAPTER 5

REFLECTING ON RESEARCHERS' DIVERSE RESOURCES

I had developed my interpretation of the computer modeling of nomadic pastoralists in the context of making a contribution to a set of sociological papers on the tools used by scientists. In the course of this work I had already begun to explore ways to engage others in analyzing scientists' diverse resources (section A). During the same period I organized some workshops in which researchers reflected explicitly on their own sociality and how it affected their work, and were encouraged to identify for themselves potential sites of engagement and change (section B). Soon after I took up a position teaching about biology and environmental science in their social context. This provided more opportunities to attempt to distribute the work of interpretation and engagement to others (section C)…

In the course of working to stimulate more interpreters of science and scientists to become interested in analyzing scientists' diverse resources and paying attention to their distributed agency, I had formulated two further themes about interpreting science:

1. When interpreters of science deal with scientists' webs of heterogeneous resources—even if only to discount the webs' complexity—they must be building their own webs. That is, all research involves heterogeneous construction (section A).

2. Those who interpret research as heterogeneous construction should try to distribute to others the work of interpreting and engaging with that research (section B). That is, they should lessen the pressure on themselves or any one person to convey the full complexity of the researchers' resources. A single individual should not even be relied on to deliver the resources needed for *others* to expose this complexity. Indeed, when I introduced the terms *unruly complexity* and *intersecting processes* to students and colleagues (section C), I hoped this would help them conceptualize directions that would address more complexity in the situations they studied, but I relied on them to take initiative in mobilizing new resources and organizing them to support new directions in their work.

A tension had become apparent. In recognition of the heterogeneous construction both of science and of its interpretation I was working to stimulate others to identify the diverse resources mobilized by particular agents who span different domains of social action. As I did so, however, I made conceptual and methodological choices that, to varying degrees, pushed the complexity of my own and my audience's sociality into the background. In principle, the practical conditions behind the interpretive choices made by researchers such as myself can always be opened up for reconstruction. The complexity that has been hidden can be brought back into the foreground. But when, in practice, is practical reflexivity worth pursuing? This remained a matter for further investigation and experimentation…

CHAPTER 6

REASONED UNDERSTANDINGS AND SOCIAL CHANGE IN RESEARCH ON COMMON RESOURCES—INTRODUCING A FRAMEWORK TO KEEP TENSIONS ACTIVE, PRODUCTIVE, AND EVER-PRESENT

As a conceptual matter I had not finished exploring practical reflexivity in relation to the heterogeneous construction of research and its potential reconstruction. Yet as a practical matter it was necessary to reach audiences comfortable with the convention of presenting scientific accounts or interpretations as if they could stand independently of the author's and the audience's particular situatedness. As I continued to wrestle with this tension, my teaching of socio-environmental studies and interpretation of science suggested a lead worth following.

In most of my interdisciplinary classes students lacked the sustained research experience that could be shared in mapping workshops, but they were also usually free of commitments to any specialized area of ecological or interpretive research. This combination of constraint and opportunity led me to formulate themes that I could introduce through cases accessible to a wide range of students, which, although simple to convey, would point to the greater complexity of particular cases and to further work needed to study them. I described these as *opening-up themes*.

It seemed that the same basic approach might be tried out on non-student audiences in which no one area of specialization predominated. The idea was to formulate themes that stimulated members of the audience to examine the particularity in practice of their own contributions to changing knowledge, society, and ecology. If this were effective, I could afford to push situatedness into the background of my presentations without abandoning my perspectives on heterogeneous construction and on representing-engaging with ecological complexity. Another way of expressing this challenge was that I wanted to acknowledge the tension between, on one hand, the multiplicity of particular situated complexities of my audience's knowledge-making and, on the other hand, the simplicity and apparent generality of the themes. My aim was to keep that tension active, productive, and ever-present. To this end I developed the multi-part framework that I introduce in this final chapter, which I illustrate using case material from research on people who manage natural resources that are held in common. (See also Taylor 2001d for an earlier application in the context of population-environment research.) Along the way I draw together many of the themes and some of the cases of the earlier chapters...

EPILOGUE: THREE STORIES

The conceptual exploration presented in this book began with the question of how ecologists could account for order arising out of the complexity of situations that build up over time from heterogeneous components and are embedded within wider dynamics, and in which there is an ongoing restructuring—what I have come to call unruly complexity. An important aspect of the progress I have made towards answering this question is to have shifted emphasis from the word “account” to the word “how”—from representations of complexity to representing-engaging—from product to process. At the beginning of the journey I envisaged that an answer would take the form of a theory or models that provide an explanation of ecological complexity. By the end I am inviting researchers who want to reconstruct the unruly complexity of ecological and social situations to become more self-conscious about their engagement within the complexity of the situation studied and of the social situations that enable them to do their research. The intersecting ecological, scientific, and social processes in the work of researchers involve diverse components and agents and span a range of spatial and temporal scales—the boundaries of unruly complexity are problematic. As both a conceptual and practical matter the framework of the last chapter had to leave as an "exercise for readers" the challenge of using your knowledge, themes, and other awareness of complex situations and situatedness to contribute to “a culture of participatory restructuring of the distributed conditions of knowledge-making and social change."

With this ending the book as a whole becomes an opening-up theme. The book does not provide a theory to explain unruly complexity in any specific field or situation, but opens up issues about addressing complexity in ways that point to further work that needs to be undertaken to deal with particular cases. On occasions I have attempted to motivate this theme in the space of a single lecture through a rapid presentation of the framework of the last chapter. On other occasions, however, I have found myself adopting an approach that amplifies the moves in section C1 of Chapter 6, namely, to use certain stories to convey some meaningful things that researchers might work on with and within the framework. Although I have been wary of ways that the narrative form tends to reinforce our experience of ourselves as concentrated agents, I am learning that stories like the those that make up this epilogue can keep distributed agency in view as we seek to grapple conceptually and practically with unruly complexity...