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Unruly Complexity: Ecology, Interpretation, Engagement

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UNRULY COMPLEXITY
ECOLOGICAL MODELING, INTERPRETATION, ENGAGEMENT

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PROLOGUE

Overview

Simply put, this book explores concepts about complexity and change. To be more specific—although at this point very abstract—I am interested in situations that do not have clearly defined boundaries, coherent internal dynamics, or simply mediated relations with their external context. Such *unruly complexity*, as I call it, arises whenever there is *ongoing change in the structure* of situations that have built up over time from *heterogeneous components* and are *embedded* or *situated* within wider dynamics. In the chapters ahead I explore the significance of unruly complexity in three realms: in ecology and socio-environmental change (in which social and ecological processes are interwoven); in the interactions among researchers and other social agents as they establish what counts as knowledge; and in efforts to feed interpretations of those interactions into ecological research—or, more broadly, to link knowledge-making, interpretation, and social change.

I construe social change broadly. It may be as ambitious as stemming the degradation of some natural resource or redirecting government policies that allocate funds to different scientific fields. But it may also be as local as focusing an audience's attention on certain themes, which is closer to the spirit of this book. I do not provide possible solutions to pressing environmental, scientific, or social problems, or a comprehensive theory of their causes. My more modest goal is to stimulate scientists who deal with ecological complexity, as well as researchers who interpret the ecological-like complexity of scientific change, to become more self-conscious and systematic about the ways they deal with the unruliness of complex situations. This shift would include researchers reflecting *critically*—that is, in relation to alternative possibilities—on their efforts to modify the social and technical conditions in which their research takes shape.

The way I promote critical reflection on concepts and practice is to introduce questions and themes intended to disturb various conceptual boundaries used by researchers when they focus attention on (supposedly) well-bounded systems and push complicating dynamics or processes out of view. I develop these questions and themes

through concrete cases from my own work; these cases open up one to the next in a way that mirrors to some degree the critical reflection I favor. The cases all involve ecological or socio-environmental situations, but their style and content differ according to the intellectual field in which each case is centered—first, theoretical ecology, then philosophy of science, history and sociology of science, and socio-environmental studies, and eventually, facilitation and teaching of reflective practice and critical thinking (from hereon, *critical reflective practice*).

The sequence of cases should help researchers and students in this wide range of fields appreciate more acutely the limitations of assuming that ecological, scientific, and social complexity can be delimited into well-bounded systems. My hope is that readers will then take steps, on their own and in collaboration with others, to reconstruct the unruliness of complexity without suppressing it, to link knowledge-making to social change, and to wrestle with the potential and limitations of critical reflection as a means to redirect practice. In the words of Raymond Williams (1980, 83), I want to encourage others not to "mentally draw back [and be] spared the effort of looking, in any active way, at the whole complex of social and natural relationships which is at once our product and our activity."

Historical origins

Why undertake a project that addresses complexity and change across the different realms of science, interpretation of science, and critical reflective practice? One answer would be that the realms are already always connected, which makes it interesting to inquire how concepts and practice are shaped to make the realms seem separate. But this is a position that can only emerge after the book has worked its way through many steps. A shorter answer is historical: the origins of the project can be located in the intersection of two kinds of ecology during the 1970s.

A century earlier Ernst Haeckel had defined "ecology" as the study of the complex interrelations among animals, plants, and their living and non-living environments (Allee et al. 1949). The meaning of the new term soon stretched to refer to the complex interrelations themselves as well as the scientific study of them. Starting around 1970, however, "ecology" (and the prefix "eco-") also became associated with

actions responding to the degradation of the environment of humans and other species. The array of endeavors that have come under the umbrella of ecology-as-social-action is vast: preventing pollution, ozone holes, global climate change, future catastrophe; advocating radical social change, environmental activism, recycling, simpler lifestyles, unrefined foods; preserving nature, biodiversity, endangered species; promoting balance and interdependency.

From a number of angles ecology-the-science promised to help address ecological concerns. Researchers competent in using tools of ecological research could provide technical assistance on particular environmental problems. Systematic environmental analysis and planning might be established so problems could be managed before they became the crises that provoke environmental campaigns. General theories of ecological complexity might enlighten humans about the conditions for more harmonious relations among people and with other organisms sharing our environment.

The rise of ecology-as-social-action, however, also involved a serious critique of the scientific enterprise. The presumption that advances in science constitute Progress was challenged by peace and environmental activists, among others. The destructive effects of science applied, for example, in military technologies and synthetic agro-chemicals made it hard to justify the pursuit of knowledge as a good thing for all. The pertinent question was: Who benefits from scientific research, and who does not? Such probing exposed science's role in many forms of domination: developed nations over former colonies, military and security branches of the State over dissenting citizens, managers over workers, whites over other races, men over women, and humans over non-humans. Some people saw science in the service of domination as abuse, not use, of science, but other critical commentators associated these tendencies with the nature of scientific inquiry itself. Either way, the view of science was not as unfettered inquiry; instead, specific developments in scientific knowledge began to be interpreted in terms of the social priorities of the governmental bodies, military agencies, corporations, and individuals who sponsored, created, or applied them.

The critique of science also involved positive proposals for alternative processes of inquiry and alternative applications of the products of science. To counter the inherent tendencies of science towards domination—or the recurrent abuses of science

in that direction—these alternatives should revolve around cooperation and not take for granted the contributions of other people or species. Scientists should also accept local, democratically formulated input to their research. Even if scientists held onto their freedom of inquiry (albeit within parameters set by their funding sources), it was widely recognized that they needed to take more responsibility for how the knowledge they made would be applied.[1]

In short, ecology-as-social-action challenged ecological researchers not only to attend to ecological concerns through technical assistance, analysis, or general theories, but also to shape their scientific practices and products self-consciously so as to contribute to transforming the dominant structure of social and environmental relations. In retrospect, I read in the broad terms of the critique of science an overoptimistic assessment of the potential, on one hand, for the social movements of the 1960s and 70s to bring about radical restructuring of social relations and, on the other hand, for people to transform their lives accordingly—including, in this context, for scientists to redirect their research. Yet the 1970s critique of science was a key aspect of the context in which I first began to engage with the complexities of environmental, scientific, and social change together, as part of one project. The challenge I take up, then, is to build on the historical and personal origins of the project and to convey its subsequent evolution in terms that help other researchers engage with such complexities in the context of the early 21st century.

Conceptual exploration: An autobiographical narrative

My decision to study ecology in Australia during the early 1970s stemmed from environmental activism 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 more than in fieldwork techniques. 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?…

Framework, audiences, and positioning

My exploration of this question about ecological theory has opened out into inquiries in socio-environmental studies, interpretation of science, and critical reflective practice. I have examined the ways that researchers in diverse intellectual fields address—or discount—the heterogeneity, embeddedness, and ongoing restructuring that make complexity unruly. I have questioned the assumption that environmental, scientific, and social complexity can be partitioned into well-bounded systems and understood or managed from an outside vantage point. Instead, I have looked for concepts and practices that would help researchers treat boundaries of many kinds as

problematic—including the boundaries between science, interpretation, and engagement.

The chapters ahead represent steps in the development of a framework, made explicit in the last chapter, that integrates conceptual, contextual, and reflexive angles on the practice of researchers. In brief, the framework:

- exposes the hidden complexity of the simplifications various fields use to focus attention on supposedly well-bounded systems;
- takes an expanded view of ecological and socio-environmental research in which inquiry is embedded in interactions among researchers and other social agents as they establish what counts as knowledge;
- extends that perspective on the embeddedness of knowledge-making so it applies also to research that interprets science (in the fields, for example, of philosophy, history, sociology);
- locates interactions to establish knowledge as part of a larger realm of researchers pursuing social change through diverse and often modest practical choices;
- invites researchers to address self-consciously the complexities of the situations studied *and* the social situations that enable them to do their own work; and
- makes space for *conceptual exploration*, that is, for playing with themes and models so as to open up questions in broad terms that might transfer across different fields while, at the same time, keeping the limitations of such themes and models in view.

I hope that an exploration of "unruly complexity" resonates with some of the concerns of a diverse range of readers: ecologists and socio-environmental researchers; modelers and theorists of complexity in biological and other systems; interpreters of science; and educators and activists in environmental or scientific politics. Yet the audience I envisage is defined not by field or discipline as much as by three qualities: an interest in exploring new propositions, themes, questions, or framings and seeing how these might adapt to their own inquiry; a sense that disciplinary boundaries (for example, between science and interpretation of science) give them trouble in their

work; and a disposition to reflect on the the conceptual and practical choices they have made in relation to alternative possibilities, past and future.

I use a number of expository devices to draw readers into issues outside their field and to keep them moving through unfamiliar terms and shifting expository styles. Each case opens with a problem or puzzle. This leads into discussion that exposes issues that I believe were not well-resolved or recognized when researchers in that field moved on from the episode or period in question. I use an autobiographical narrative to bridge the chapters and highlight the conceptual connections between cases from different fields. (Readers are welcome to read this story in its entirety first if they want to a preview of how the diverse components of my project come together.)

As an expository device the autobiographical narrative has a number of functions. It allows me to downplay side branches and intersecting storylines—as most storytellers do—so readers can see the logic of the book's overall progression even when the cases take them beyond their specialties. (For additional help in this regard a summary of themes and questions is included at the end.) The narrative also serves as a reminder that the episodes are drawn from *certain periods of time* in the work of *one* person. This caveat means readers should not expect a direct payoff in relation to the current problems in any of the fields addressed—I would not have arrived at a framework spanning the three realms of complexity if I had followed the pathways that branched off after each episode or tried to translate my cases into the present terms and inquiries of each field. (The glossary and extended thematic endnotes, which acknowledge subsequent developments and related projects, should help readers relate my approach to those of other authors.)

The most important function of the narrative, however, is to model the journeying, opening up of questions, and critical reflection that I aim to stimulate in others (Taylor 2002b). Readers could well ask me for evidence or argument that conceptual exploration and critical reflection produces better or more relevant answers in their field, but they could also view a narrative that highlights the partiality, particularity, and developmental character of my contribution as an invitation to reflect on their own paths and positioning over time. In this spirit, readers might bounce off the themes I develop in the book and articulate the contrasting or similar themes they

have they applied when addressing the complexities of the situations they study and the conditions that enable them to do their work.

About my own positioning: I remarked earlier that ecology-the-science could address ecological concerns at three levels: through technical assistance on particular environmental problems; systematic analysis and planning; and general theories of ecological complexity. Interpretation of science can also be arrayed along an equivalent spectrum. Clearly this book lies at the general end, with an emphasis on questions and themes that I believe can help researchers address complexity in ecology, interpretation of science, and critical reflective practice. Systematic analysis and planning is also considered, but mostly in ways that are critical of technocratic ambitions and that promote reframing of analysis and planning along participatory and reflexive lines.[2] The demands of assisting on particular environmental and scientific problems are not the subject of this book.

I believe that conceptual exploration is valuable for researchers trying to deal with environmental, scientific, and social complexity. But let me acknowledge at the outset that contrasting approaches, especially learning from long-term engagement in particular controversies or in larger social mobilizations,[3] keep me questioning my emphasis on abstract concepts such as heterogeneity, embeddedness, and ongoing restructuring. Conceptual exploration is the area in which I have been best able to make a contribution, but I look forward to dialogue that keeps such tensions active and productive—that stretches what I am able to offer in the text of a book.[4] Indeed, the framework I develop in the chapters ahead makes a special place for conceptual moves that open up issues about addressing complexity but do so in ways which point to further work that needs to be undertaken to deal with particular cases.

Conceptual exploration (continued)

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. Later I modeled 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 and, at the same time, 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, historically, 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 justify them with some account of ecological complexity. Yet 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…

Notes

The main body of the text is written to engage a range of audiences, so I use thematic endnotes to elaborate on some technical issues and specialized debates, and on developments subsequent to the period in which the cases originated. In these notes I also acknowledge some related projects and position my approach with respect to them. My intent is to stimulate dialogue, not to provide a comprehensive bibliography or overview of any issue.

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[1] The critique of science during the 1970s

During the 1960s Bookchin (1962), Carson (1962), and Commoner (1963; 1971) linked ecology-as-social-action to criticisms of the dominant directions of scientific research. Social responsibility in science was promoted by the British Society for Social Responsibility in Science, the Union of Concerned Scientists (in the U. S. A.), and Pugwash (which focused on reducing the danger of armed conflict in a nuclear age). During the 1970s more radical, anti-capitalist critiques of science and social relations were developed in the U. S. A. by Science for the People (see especially the critiques of biological determinism in Science for the People 1977 and the related work of Chase 1977) and in England by the Radical Science Journal (see Levidow 1986, especially Young's introduction on the origins of the "Radical Science" movement; Levidow and Young 1981; and Radical Science Editorial Collective 1997). In this context Werskey completed his illuminating history of an earlier generation of left-wing scientists in England who saw "science, progress, socialism as equivalent concepts" (Young, p. xiv in the forward to the 1988 reprint of Werskey 1978; see also Werskey's preface to the reprint, which reflects on the way the 1970s critique of science shaped his account.) In contrast, left-wing scientists of the 1970s who saw their science as a political project recognized that science could bolster domination and inequality (Roberts 1979; Rose 1982; Levins and Lewontin 1985; see also Illich 1973 and 1976 for advocacy of de-professionalization and of "convivial" technology and medicine).

The critique of science also stimulated interpretation of science in relation to the historical and social context in which it was formed. Kuhn (1970) was widely cited as

opening up science to such contextualization, but younger historians and sociologists began to take the social interpretation of science much further than Kuhn had (see, for example, Young 1985).

[2] **Intellectual trajectories away from comprehensive quantitatively-based analysis and planning**

Many researchers who during the 1960s and 70s were interested in comprehensive, quantitatively based analysis and planning have since left this ambition behind. Based on personal conversations I believe a "Modelers Anonymous" organization would have many potential members. David Harvey, a geographer who features in the Epilogue, section C, might be one example (compare Harvey 1969 with 1995; see autobiographical essays by Harvey and by Olsson in Gould and Pitts 2002).

[3] **Learning about ecological and scientific complexity through long-term engagement in particular settings or in larger social mobilizations**

Examples of texts that address the intersections of environmental and social change through long-term engagement in particular settings or in larger social mobilizations, which thus stand in potentially productive tension with this book's conceptual exploration, include Boal (forthcoming); Brown & Mikkelson (1990); Gibbs (1992); Hofrichter (2000); Martin (1996a); Pellow (2002); Sclove (1995); and Williams (1990; 1992). Some relevant organizations in the United States include the Community Research Network (associated with the Loka Institute, http://www.loka.org/), the Highlander Education and Research Center (http://www.highlandercenter.org/), Center for Health, Environment and Justice (formerly, Citizens Clearinghouse for Toxic Wastes, http://www.chej.org/). See also the Epilogue.

[4] **Studies of complexity in ecology, socio-environmental studies, and interpretation of science**

In addition to the work identified in the previous note, the following texts would be on my provisional map of positions that complement or stretch this book's approach to complexity in ecology, socio-environmental studies, and interpretation of science:

ecology—Haila and Levins (1992), Levin (2000), Pimm (1991)

socio-environmental studies—Guyer (1991), Peet and Watts (1996b), Roe (1998), Turner and Taylor (2003)

history of ecology—Bocking (1997), Hagen (1992), Kingsland (1995), Kwa (1989)
social studies of science, especially complexity and modeling—Edwards (2003), Fausto-Sterling (2000), Gieryn (1999), Haraway (1988), Helmreich (1999), Latour (1987), Law and Mol (2002), Pickering (1995), Traweek (1994) . (In Law and Mol 2002 see especially the editors' introduction and essays on ecological themes by Kwa and Thompson.)
philosophy and conceptual studies of science—Bohm (1995), Krieger (1994), Longino (1990), Oyama et al. (2001), Wimsatt (2001). (In this area there is room for dialogue around epistemology construed broadly as inquiry about what makes it possible for social agents to make reliable knowledge; see also notes 17 and 20).
studies of gender or race in relation to complexity—Eglash (1999) and the works cited above by Fausto-Sterling, Guyer, Haraway, Krieger, Law and Mol, and Traweek. (These are important directions of stretching given that almost all the researchers in the book's cases are men of European descent, as I am also; but see note 29.)
critical, reflective practice—See Epilogue and notes 30 -32.