Reconstructing Unruly Ecological Complexity: Science, Interpretation, and Critical, Reflective Practice

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Involvement in environmental issues in the 1970s led me, as it did many fellow activists, to study the science of ecology. Having a mathematical disposition, I chose to focus less on field studies and more on quantitative analysis and theoretical modeling. I soon developed an interest that continues to this day in the challenge that ecological complexity poses to conventional scientific ways of knowing. As I explored this challenge, my work in ecology and socio-environmental studies opened out to interpretive studies of science and then to facilitation of critical, reflective practice. Within each of these realms as well as in moving among them, my interest became to problematize boundaries used by researchers to partition of complex situations into well-bounded systems and backgrounded or hidden processes.

When researchers assume that there are systems with clearly defined boundaries, coherent internal dynamics, and simply mediated relations with their external context, they can locate themselves outside the systems and seek generalizations and principles affording a natural or economical reduction of complexity. A contrasting image is that well-bounded systems, when they are encountered, require explanation as special cases of unruly complexity, in which boundaries and categories are problematic, levels and scales are not clearly separable, structures are subject to restructuring, and components undergo ongoing differentiation in relation to each other. Control and generalization are difficult and no privileged standpoint exists. The position I have come to is that researchers who want to discipline unruly complexity, but not to suppress it, have to pay more attention to their own agency within the participatory restructuring of knowledge making and social change.

This essay reconstructs my intellectual journey towards this position, one that resonates with de Sousa Santos's (1992) Discourse on the Sciences. The episodes are less about participatory restructuring, however, than they are about exploring concepts and eventually coming to articulate my project in terms of intersections among three strands: disciplining unruly complexity; linking knowledge making to changing diverse social relations; and wrestling with the potential and limitations of conceptual exploration. With respect to this last strand, the detail I present will be specific to my own inquiries, but the themes developed are meant to stimulate readers to problematize analogous boundaries and address analogous complexities in their own fields of inquiry and practice. I believe that the concepts and issues I raise should be taken up more broadly, but I have no hesitation admitting the heuristic intent of the essay. I recognize that analogies can be applied in circumstances for which they do not serve well or can misguide the theorist. As will become evident, however, I am especially interested in conceptual moves that open up issues about addressing complexity, but do so in ways that point to further work that needs to be undertaken to deal with particular cases.

1. PROBLEMS OF BOUNDEDNESS IN MODELING ECOLOGICAL SYSTEMS, TWO CASES

a. The construction of complexity

My undergraduate mathematical ecology text conveyed succinctly the quality of ecological complexity (Pielou 1969, 1): 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.

How could ecologists account for such ordered complexity? Ecology is not like thermodynamics; it does not yield the statistical complexity of large numbers of identically behaving components. Moreover, 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 their ecological context, where they respond to a multiplicity of hazards and resources distributed in various ways across place and time. Yet, analysis of observations from non-experimental situations is beset by circularity—much needs to be known in advance about the causal factors involved in order to design methods of data analysis capable of exposing those very factors (Austin 1980, 1987).

Given the character of ecological complexity and its analysis by ecologists, could any general theories of its structure apply? During the 1960s and 1970s many academic ecologists, especially in the United States, thought such theories could be developed (Kingsland 1995). In this

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tradition, the age-old idea of a balance of nature (which many environmentalists still invoke) led to a search for a significant relationship between the complexity of ecological communities and their stability. However, mathematical ecologists, using computers to analysze large samples of possible model communities, discovered that the larger the commuity and the stronger the interactions among populations, the smaller the proportion of the sample would be stable. If complexity in itself does not promote stability, May (1973, 174) asked, what are the “devious strategies which make for stability in enduring natural systems”? A considerable body of work then explored this line of thinking or disputed the idea that complex natural communities were stable, however that was defined (DeAngelis and Waterhouse 1987).

Yet complex ecological communities are generally not the outcome of some sampling process; they arise through development over time involving the addition, growth, decline, and elimination of populations. My approach to the complexity-stability question, which built on work by Tregonning and Roberts, showed that, whereas stable communities may be extremely rare as a fraction of the model communities being sampled, they can be readily constructed over time by the addition of populations from a pool of populations and the elimination of populations from systems not at a steady state (Taylor 1989). (This pool of potential entrants can be visualized as neighboring communities or patches, refuges in which species persist undetected at low abundances, the seed bank, and so on.) Moreover, the result of this ongoing construction and turnover depends on the order in which populations were added (Drake 1991).

This result indicated that ecologists who are interested in explaining the persistence of complex communities need to examine not only the stability and structure of the current configuration observed but also its construction over time from diverse components—its contingent history of becoming structured and its ongoing restructuring in a wider spatial context or "landscape."

b. The hidden complexity of simple models

Field ecologists are perennially sceptical about textbook mathematical models, but Vandermeer (1969) fitted his laboratory data on four competing protozoan populations to relatively simple equations and used them to make correct predictions about which populations could coexist. I noticed, however, an anomaly in Vandermeer's results that he and subsequent discussants had not addressed. The model he fitted to his observations indicated that three of the six pairs of interactions between the pairs of protozoan populations were positive for one population and negative for the other. One would expect this of predator-prey relations, not of competitive interactions. Were these interactions actually predator-prey? Indeed, were those pairs with negative-negative interactions actually competitors?

My questions arose in a context in the 1980s in which ecologists had begun to pay attention to the effects mediated through the dynamics of populations not immediately in focus (Strauss 1991). Note that Vandermeer's equations had not specified all the components of the community. Each day he had removed a sample from his experimental tubes and added an equal volume of culture medium with bacteria. The bacterial populations were alive and able to grow until consumed by the protozoa—they had dynamics of their own not referred to in the equations. Because there was no explicit reference to the relationships with the hidden part of the community, the interaction values in the model had to incorporate these and any direct interactions. The combination could be called "apparent" interactions. Although ecologists would think that the protozoan populations should be competitors because they share a food resource, Vandermeer’s study showed that some counter-intuitive apparent predator-prey interaction values were needed to fit the observations.

I developed a mathematical way to investigate apparent interactions more generally and concluded that they often deviate significantly from direct observations of interactions and from ecologists’ intuition about plausible interactions among populations (Taylor 2003). It seemed that when simple models fit observations well this may be because hidden variables happen to remain within narrow bounds, not because the model approximates the actual interactions. Principles proposed for simple sub-communities, such as the non-coexistence or "competitive exclusion" of species with similar requirements, can be confounded by the dynamics of populations with which those sub-communities interact in naturally variable and complex ecological situations. In short, boundaries that discount the dynamics of non-modeled variables become problematic.

The notion of <u>inseparable dynamics</u>, which link the system that is the focus of research with the backgrounded processes, has potentially profound implications for thinking about knowledge making. It makes clear that controlled experiments in which systems are isolated from their context are not appropriate for understanding organisms embedded in a dynamic ecological context and responding to consumers and resources that are unevenly distributed across place and time. Inseparable dynamics, together with the picture above of ongoing construction and turnover, suggest that 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 (Taylor 1992a).

It should be relevant beyond ecology to problematize boundaries in these ways. In the early 1980s the anthropologist Eric Wolf proposed a conceptual inversion: Whenever theory has built on the dynamic unity and coherency of structures or units—in Wolf's case, societies or cultures—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 (Wolf 1982, 387). As will emerge, socio-environmental studies has proved to be a more fertile field than ecology proper in which to elaborate on Wolf's conceptual inversion and paint a picture of intersecting processes (see, e.g., Little 1987, Taylor and García-Barrios 1995, Peet and Watts 1996.)

2. STRATEGIES OF MODELING

The perspectives developed in the previous section are especially challenging for mathematical modelers because the assumption of a fixed, delimited set of components is almost required for building and analyzing a mathematical model. Moreover, these theoretical explorations seemed to be working against aspirations for general principles about ecological patterns supported by mathematical models. Those aspirations were also being questioned, but from a different angle, by ecologists in the early 1980s who compared the models of community ecology and found that many fit the data no better than alternatives that assumed random interactions among populations. Conveying deep scepticism about the possibilities of general ecological theory, Simberloff (1982) argued that many factors operate in nature, and in any particular case at least some of them will be significant. A model cannot capture 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. They 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 modeling experience described in the previous section led me, however, to resist the emphasis on testing specific hypotheses about particular situations and to be less sceptical about theory built through modeling. The use of mathematical models for conceptual exploration had allowed me to reformulate issues and generate new questions, and this seemed worthwhile. In response to the philosophical positions ecologists like Simberloff were staking out, I noted that, although all models necessarily simplify reality, they are not designed and applied according to the same standard for correspondence with observations. I classified models according to the level of correspondence with observations established for a model's distinguishing feature and independently for its assumptions (Lloyd 1987, Taylor 2000). My taxonomy secured a place for the generation of theory through "exploratory" modeling (see also Levins 1966), at the same time as noting that the insights so derived are about a mathematical system or conceptual schema. Their relevance to biology is yet to be established; there remains an uneasy tension between mathematical and conceptual tractability and the demand that the exploratory model should eventually yield something that can be more strictly evaluated against observations. In this light, the results of exploratory modeling are best thought of as heuristics—propositions that can stimulate and orient research, but which may turn out to have been applied in circumstances for which they do not serve well or otherwise misguided the theorist.

Equally importantly, this account of modeling allowed me to identify several open sites, in which considerations other than explicit analysis of a model's correspondence with evidence must come into play. Modeling and theory-building necessarily operate within webs of social and technical decisions ecologists make about which questions to put to nature, categories to use, observations to construct, analyses to perform, degree of confirmation to require, ways to revise models, and so on. The recognition that knowledge-making must always extend beyond the dialogue between models and evidence—that this dialogue is embedded in a larger dynamic context of influences shaping the ecologists' decisions—opened my inquiry to include investigation of the "sociality" of ecology and environmental studies.

3. SOCIAL-PERSONAL-SCIENTIFIC CORRELATIONS

Conventional accounts of scientists and philosophers of science assume that the scientist's dialogue between models and evidence can be separated from the dynamics of their dialogue with other scientists to establish what counts as knowledge. They would contend, for example, that decisions made at the open sites I identified in the previous section can lead some scientists to tackle anomalies that others had dismissed as negligible, and thus science progresses. In any community of scientists disputes may be resolved when one scientist's biases are countered by those of another. Social influences, such as research funding, may primarily inhibit or accelerate improvement in scientific knowledge. In these senses science's sociality does not prevent its "referentiality"—the correspondence of models or theories with evidence—from determining what counts as knowledge—if not immediately, at least in the long run.

There is a deeper sense of sociality, however, that is harder to reconcile with conventional views. All scientists engage in various arenas of social activity—they build careers and institutions, use and transform language, facilitate policy formulation, and so on. Given this context, scientists 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 to enable others to act upon their conclusions. This might happen in idiosyncratic ways, but I was interested in the possibility that the simultaneous pursuit of referentiality and sociality could sometimes lead to systematic effects on the content of scientific knowledge. If this connection could be demonstrated and analyzed then ecologists who theorize about ecological complexity—or researchers more generally—might be encouraged to use awareness of such effects to modify their own inquiry and practice.

I began to explore the effects of science's sociality on its content by examining the history of systems ecology, a field that emphasizes nutrient and energy flows in entire systems. H. T. Odum (1924-2002), a pioneer in systems ecology, conceptualized ecological complexity in terms of energy circuits (like electrical circuits) subject to feedback that ensured homeostasis of the overall system. The principles ecologists might discover through collecting data on the energy flows in mature, productive ecological systems, such as tropical rainforests, could guide the design of "systems of man and nature." In this vein, Odum began to analyze energy flows in social systems; at the same time his theoretical propositions pointed to an important role in systems for high-quality, low-energy circuits (Odum 1971).

Odum's work during the 1950s and 60s reduced the complexity of social and ecological relations to a single currency, energy, whose flows could be adjusted or redesigned. As I interpreted Odum, the high-quality, low-energy circuits were allegorical; he had found "in nature" a special role for systems engineers, such as himself, working in the service of society (Taylor 1988). This overall interpretation built from noting that for Odum the social, personal, and scientific realms reinforced each other during the post-war years of "technocratic optimism." This reinforcement occurred from many angles. Government funding and organization of science under military imperatives during World War II had produced significant results, giving currency to the belief that intervention on a large scale could be practically realized. Moreover, scientific control of complex systems seemed necessary to prevent further social upheavals or holocaust. During the post-war decades optimism about the benefits of technocratic systems management overshadowed possible doubts about its implications for democratic political life. Science in the service of social progress was also a theme stressed to Odum by his father, the sociologist H. W. Odum, who promoted the cooperation of intellectuals and other social elements in the reintegration of the American South into the nation. In the vision of H. W. Odum and many others during the Great Depression, when the social "organism" is unhealthy the natural division of labour needs to be restored. In the realm of science, the young H. T. Odum explored electrical circuitry and the harnessing of energy (which in terms of wider social attention were the mid-century equivalent to computers at the century's end). After war-time service in tropical meteorology, Odum was recruited by the Yale ecologist G. Evelyn Hutchinson, whose wide-ranging work included synthesis of data on the stocks and flows of specific chemicals through the biosphere, exploration of mathematical approaches to ecological theory, and participation in the influential Macy conferences on cybernetics and feedback systems. These strands are all evident in the work Odum undertook as a young professor—in the diagrams he drew of energy stocks and flows, the analogous electrical circuits he built and manipulated, and the whole-system research projects he had funded generously by the major institutions of post-war science.

Historians of science, seeking to illuminate why certain categories are plausible and certain lines of inquiry are pursued, have often identified underlying, perhaps implicit, patterns of thought and metaphors shared among different sciences and social thought more generally (Stepan 1986). The scientist in me was interested in the mechanism producing such correlations—how could scientists do their actual research in ways that historians could interpret as resonating with the scientists' concerns about social order? By considering Odum's practice, which included not only his concepts and production of theory but also his methods and organization of research, I saw Odum as someone 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. This view of Odum's practice expanded the historian's idea of a shared metaphor—the various <u>social-personal-scientific correlations</u> enabled Odum not simply to <u>think</u> that ecological complexity is like electrical circuits, but to <u>act as if</u> it were.

4. HETEROGENEOUS CONSTRUCTION

If I had shown that the socialty of science may have systematic effects on the content of scientific knowledge, I still needed ways to bring such interpretations to bear productively on subsequent research. In this regard, the case of Odum was a limited model. Personal, scientific, and social considerations reinforced each other so consistently in Odum's life that it was difficult to see how he could have done anything differently. At best, only a very general lesson could be drawn from my interpretation: Scientists opposed to technocratic rationality should not treat ecological complexity as if it were made up of well-bounded systems analyzable in terms of a single currency. However, scientists wanting to heed such a lesson would still need specific ways to arrange or alter their own personal, scientific and social facilitations. For insights in that regard, a more fine-grained analysis than the interpretation of Odum would be valuable. With this in mind I chose to consider shorter-term projects of socio-environmental assessment likely to be governed by more complex and contested pragmatics.

The first case was modeling work I had undertaken in Australia, in a project analyzing the future of a salt-affected agricultural region; the second involved U.S. researchers in the mid 1970s building computer models of nomadic pastoralists in drought-stricken sub-Saharan Africa. In both cases I traced diverse interconnections between the various so-called "technical" decisions of the scientists and the social considerations that influence how scientists perform the resulting tasks. To address the question of how something comes to be established as knowledge, I assessed what would be entailed in practice to modify that knowledge (Latour 1987, Taylor 1995). The modeler in the U.S. project on sub-Saharan Africa, as I interpreted his work, had to deal with diverse considerations such the available computer compiler, published data, the short length of time both in the field and for the project as a whole, the work relations within the research team, the relationship of the United States efforts to other international involvement in the region, the terms of reference set by the funding agency and its contradictory expectations of the project, and so on. The practical considerations that were "resources" for the modeler's knowledge-making were also commitments to certain actions; these actions implicated many other social agents and spanned local and wider social realms (Taylor 1992b). In the diverse, particular ways, I contended, the modeler had been imagining and engaging in social change at the same time as making knowledge.

My image of scientists working in a social context had evolved from social-personal-scientific correlations into one where scientists harnessed heterogenous resources as they simultaneously represented socio-environmental situations and engaged in them. In short, scientific knowledge making requires <u>heterogeneous construction</u>. One virtue of heterogeneous constructionist interpretations is that they reveal multiple points at which scientists could engage differently in scientific practice and try to modify its outcomes. Whether any specific modifications—in the case above: working with a different compiler; spending more time in the field; and so on—are do-able depends on the position and resources of the specific scientists as they enter into negotiations with other relevant social agents. Scientists' ability in practice to make knowledge is <u>distributed</u> beyond their persons, not concentrated mentally inside them; it depends on intersecting processes (Taylor 2001a).

5. ECOLOGISTS MAPPING THEIR OWN SOCIALITY

If ideas about multiple specific modifications were to be fed productively back into science, scientists would have to become interested in analyzing their diverse resources and paying attention to their distributed agency—to become <u>practically reflexive</u>. It would be inconsistent for interpreters of science to take responsibility for analyzing the full complexity of any scientist's resources, let alone for delivering additional resources needed for the scientists to modify this complexity. One project I undertook to foster practical reflexivity was to convene some pilot <u>mapping workshops</u>, in which the scientist-participants were encouraged to be more explicit and strategic about modifying their social context of research and their research together. Interactions among the researchers centered on "maps"—pictorial representations they drew of the things—theoretical themes, methodological tactics, organisms, events, institutional facilities, disputes, and so on—that motivated, facilitated, or constrained their inquiry and action (Taylor 1999a).

Although I will not claim the pilot workshops were followed by dramatic changes in the work of the participants (who turned out mostly to be advanced graduate students), the experience provided me material for further theoretical reflection. I noted that participants articulated connections that had previously been unexamined, unspoken, or discounted. The researchers showed that, when encouraged or prodded by interaction with others, they knew more about their social situation than they usually acknowledged. In their usual practice and discourse, however, researchers discount their awareness of their complex, distributed "situatedness" in favour of a <u>concentrated</u> view of their own agency and a partitioning of the realms of science and interpretation. This implies, however—according to the perspective of heterogeneous construction—that researchers' simple formulations about their social situatedness were serving as resources for them in their knowledge making.

A reframing of practical reflexivity is suggested by this tension between the simple things that researchers say (or tell themselves) about what shapes their work and the complex situatedness that they could at times acknowledge. The ideal need not be that scientists at all times keep in mind a systematic account of the resources that enable them to do their research, but that they seek out situations and conditions that enable them to keep the backgrounded processes in view and periodically bring them back into play as they mobilize different resources or organize them in new directions. The tension between concentrated and distributed agency would be kept active and productive; the boundaries problematic, not taken for granted.

6. A FRAMEWORK TO KEEP TENSIONS ACTIVE AND PRODUCTIVE

If agency is distributed, reflecting on one's situatedness is no guarantee that a researcher will be able to mobilize different resources to significant effect. Stanley Fish (1989), an influential U.S. interpreter of the situatedness of legal and literary texts, took this insight a step further and asserted that reflection on one's situatedness is irrelevant to changing it. In contrast, I have treated it as an empirical matter—one to be established through experiment and experience—which kinds of reflection and workshop processes, which modes of interaction and support, contribute most to scientists modifying the situations in which they make knowledge. In this spirit, I sought out opportunities to develop my experience and skills in workshop facilitation (Taylor 2002).

More generally, I began to conceptualize a realm of critical, reflective practice in which researchers—not only scientists, but also interpreters of science—would address self-consciously the complexities of the situations they study and their own social situatedness as they affected social change. (Such "social change" might range from the level of global environmental politics or to the more modest level of teaching students and influencing colleagues.) In this realm, an important variant or analog of the simple-complex tension introduced above is evident. Simple themes, such as "Biodiversity is important to the balance of nature" or "Population growth leads to environmental degradation" are easier to communicate to a general audience than the complex intersecting processes in particular environmental situations. (More difficult still is to convey the heterogeneous construction of the practice of particular researchers or a combination of intersecting processes and heterogeneous construction.) The simple themes seem to be more potent resources for mobilizing others to think in your terms and to act accordingly. However, simpler, more memorable, and adaptable accounts are only apparently simple (analogous to the apparent interactions in ecology described earlier). Their impact and importance must depend on the ways they are linked to other resources by scientists and others who are negotiating how to contribute to changing knowledge, society, and ecology.

To address this tension I developed the following framework, which synthesizes the passages I have presented in the preceeding sections (Taylor 1999b). Consider three angles—like facets of a crystal— from which to view the practice of researchers as knowledge-makers:

A. their study of complex situations (sects. 1 & 2);

B. their interactions with other social agents to establish what counts as knowledge (sects. 3 & 4); or

C. their pursuit of social change through attention to the complexities of both the situations studied and the researchers' own social situatedness (sects. 5 & 6).

For each angle, I distinguish three broad ways to address complexity. The first is to rely on simple formulations of well-bounded systems, having coherent internal dynamics and simply mediated relations with their external context. These formulations contrast with work based on dynamics that develop over time among particular, unequal units or agents whose actions implicate or span a range of social realms. Although simple formulations are easier to communicate than reconstructions of particular complex situations, their simplicity can be confounded by the dynamics they hide. In between these two formulations I find it is useful to introduce scenarios and heuristics that are readily communicated, which open up issues and greater complexity and, at the same time, point to further work needed to be undertaken to deal with particular cases. Before discussing the full framework of three formulations for each of the three angles, let me contrast the three kinds of formulation for angle A—researchers studying complex situations—by rehearsing a lesson I often take students through (Taylor 2001b).

Many environmentalists point with concern to the rising global population, for example, "hold(ing) it to be self-evident (and) undeniable that prospects for the future would be more favourable if there were fewer people on earth" (Okoye and Smith 1994, 11). The greater the population, the greater the erosion of arable lands, consumption of non-renewable resources, and production of greenhouse gases and pollutants in general. This is an example of the first kind of formulation—a simple, well-bounded system.

Now let us consider a simple scenario: There are two countries. Each has the same amount and quality of arable land, the same population size, the same level of technical capacity, and the same population growth rate of 3% per year. Country X, however, has a relatively equal land distribution, while country Y has a typical 1970s Central American land distribution: 2% of the people own 60% of the land; 70% live on only 2%. Both countries double their populations very rapidly, but five generations—120 years—before anyone would be malnourished in country X, all of the poorest 70% in country Y would be—unless they acted to change their situation.

This is not just an issue of the timing of the crisis in the two countries. Y's poor would probably first experience what others call population pressure in the form of food shortages. They might link these shortages to inequity in land distribution (Durham 1979, Vandermeer 1977) and attempt to take over the underutilized land of the wealthy. The wealthy, anticipating this possibility, might fund paramilitary operations that target leaders of campaigns for land reform. Or build factories that employ some of the land-starved poor. And so on. The nature of foreign trade and availability of military aid would influence the actual choices in specific instances.

No real country is like country X. The crises to which actual people have to respond come well before and in different forms from the crises predicted on the basis of aggregate population growth rates and ultimate biological and physical limits to growth. Although many environmentalists stress that affluent countries and people have disproportionate effect on the environment, the scenario says something deeper. The point is about dynamics—groups with different wealth and power exist, change, and become involved in crises because of their dynamic interrelations.

The lesson so far can be summarized in a heuristic: the analysis of causes and the implications of the analysis change qualitatively if uniform units are replaced by unequal units subject to further differentiation as a result of their linked economic, social and political dynamics. This heuristic points to the need to examine a range of issues that complicate the demographic ones at the heart of the first formulation, but further work would be needed to analyze the complexity of any particular case. In this vein, let me sketch the picture produced by two colleagues after extensive field and archival work to understand the severe soil erosion in a mountainous agricultural region in central Mexico (García-Barrios and García-Barrios 1990).

The erosion in the region studied turned out not to be the first occurrence of such a problem. After the Spanish conquest, when the indigenous population collapsed from disease, the communities abandoned their terraced lands, which then eroded. The remaining populations moved to the valleys and adopted labour-saving practices from the Spanish, such as cultivating wheat and using ploughs. As the population recovered during the eighteenth and nineteenth centuries, collective institutions evolved that reestablished terraces. Erosion was reduced, soil dynamics were stabilized, and perhaps some soil accumulation was stimulated. But this type of landscape transformation needed continuous and proper maintenance. If a terrace were allowed to erode the soil would wash down and damage lower terraces; there was the potential for severe slope instability. The necessary maintenance was made possible by the collective institutions mentioned, which first revolved around the Church and then, after independence from Spain, around rich Indians called caciques. These institutions mobilized peasant labour for key activities—not only maintaining terraces, but also sowing corn in work teams and maintaining a diversity of maize varieties and cultivation techniques. The caciques benefitted from what was produced, but were expected to look after the peasants in hard times. Given that the peasants felt security in proportion to the wealth and prestige of their cacique, and given that prestige attached directly to each person’s role in the collective labor, the labour tended to be very efficient. In addition, peasants were kept indebted to caciques, and could not readily break their unequal relationship. The caciques insulated this relationship from change by resisting potential labour-saving technologies and ties to outside markets in maize.

The Mexican revolution ruptured the closed system of reciprocal obligations and benefits by taking away the power of the caciques and opening the communities to the changing outside world. Many peasants migrated to industrial areas, sending cash back or bringing it with them when they returned to the community for periods of time. Rural population declined; transactions became monetarized; and prestige no longer derived from one's place in the collective labour. With the monetarization and loss of labour, the collective institutions collapsed and terraces began to erode. National food-pricing policies favored urban consumers, which meant that corn was grown only for subsistence needs in this area. Little incentive remained for intensive agricultural production. New labour-saving activities, such as goat herding, which contributes in its own way to erosion, were taken up without new local institutions to regulate them.

The work of the García-Barrioses illustrates well a formulation of socio-environmental change in terms of intersecting processes (or inseparable dynamics): Population, agro-ecology, and socio-economic institutions are interlinked and local changes and continuities interlink with wider developments within Mexico. No single strand on its own, such as population growth (or, during the twentieth century, population decline), could be sufficient to explain the currently eroded hillsides.

Table 1. Three angles from which to view researchers' practice (A, B, C) and three kinds of formulation of each angle (1, 2, 3), with examples of each combination drawn from the essay

	A. Researchers' study of complex situations	B. Researchers' interactions with other social agents to establish what counts as knowledge	C. Researchers' pursuit of social change through attention to the complexities of both the situations studied and the researchers’ own social situatedness
1. Simple formulations of well-bounded systems (coherent internal dynamics and simply mediated relations with their external context)	e.g., Stability (or instability) of ecological communities derives from their complexity	e.g., Science's sociality has at most a transient effect on what counts as knowledge	e.g., Researchers preserve terms familiar to their audience and thus choose not to disuss their social situatedness
2. Scenarios and heuristics readily communicated, which open up issues and point to further work to deal with particular cases	e.g., Exploratory modeling of construction and turnover of ecological communities (sect. 1a)	e.g., Social-personal-scientific correlations in the work of systems ecologist, H.T. Odum (sect. 3)	e.g., Mapping workshops (scientist-participants expose and explore their social context of research and their research at the same time; sect. 5)
3. Work based on dynamics that develop over time among particular, unequal units or agents whose actions implicate or span a range of social realms	e.g., Case of soil erosion in central Mexico (see text above)	e.g., Heterogeneous construction of socio-environmental assessment (knowledge making requires diverse considerations in practice; sect. 4)	*participatory restructuring of knowledge making and social change*

The population-environment lesson puts me in a position to move to the full framework I developed (Table 1). The lesson spells out for angle A the use of an in-between formulation that disturbs understandings based on simple, well-bounded system and points to the need for work to be done in particular cases on dynamics among unequal agents (or units) whose actions implicate or span a range of social realms. Notice that the same three formulations are implicit in the three angles on the practice of researchers. The conventional formulation of science as a dialogue with the situations studied (angle A) is disturbed by interpretations of the researchers' interactions with other social agents to establish what counts as knowledge (angle B). Interpretations, such as those in sections 3 and 4, which emphasize the diverse practical considerations researchers address, suggest heuristically that researchers are always already imagining and engaging in social change at the same time as making knowledge. This opens up the possibility of people pushing the knowledge-making/social-changing connection self-consciously, that is, of critical, reflective practice (angle C).

Within the realm of critical, reflective practice the three kinds of formulation also apply. Reflexive researchers who want to impart their knowledge to others could decide, for example, that it is more effective to use terms familiar to other researchers in their audience. Conventionally, these terms center on marshalling concepts and evidence about the situation studied (angle A) and leave most aspects of the researcher's social situatedness (angle B) taken for granted. This formulation is disturbed by mapping workshops, which show that researchers can speak more about their social situatedness and relate it to possible modifications of their research. But the participants in the mapping workshops were self-selected and mostly at a similar early stage of their careers. More work would be needed to bring together a diverse set of researchers and other participants and (given that anyone's ability to make knowledge is distributed beyond their persons) to sustain the participants' interaction until new complexity-addressing collaborations emerged. These interactions would need to acknowledge and mobilize the tension between using simple, digestible models and addressing particular complexities of both the situations about which they want to make knowledge and their social situatedness.

I continue to struggle with awareness that many readers would like me to present an exemplary case of such a process. To do so, however, would be to concede to the angle-A idea that the power of this essay should rest on my ability to produce a faithful account of the complexity of the situation I study. (This "situation" is now one that includes interpretation of science and reflective practice as well as the ecological complexities with which I began.) Such an expository move would leave it to readers to mobilize the collaborators, sources of funding, and diverse, particular resources needed to contribute to work that matches the exemplary case. Too much weight would be placed on the concentrated, not distributed, agency of author and readers alike. Instead, the final cell in the framework—the intersecting processes/inseparable dynamics formulation of critical, reflective practice—should remain unfilled. This move leaves opened and active questions about the role of individuals and their knowledge, heuristics, and other awareness of complex situations and situatedness. Clearly, more work is needed on what I and other agents can do—but not alone nor through our accounts of the world alone—to contribute self-consciously to the ongoing restructuring of the dynamics among particular, unequal knowledge-making agents whose actions implicate or span a range of social realms.

EPILOGUE: FLEXIBLE ENGAGEMENT

The openness of such an ending cannot be expected to make any reader comfortable—or myself for that matter—so let me add a story. I am wary of ways that narrative tends to reinforce our experience of ourselves as concentrated agents, but I am learning that certain stories—told to myself and to others—can help keep distributed agency in view. These are stories that crystallize the challenges of mobilizing resources and organizing them in new directions, that orient the knowledge-makers less towards the product than towards contributing to knowledge-making/social-changing collaborations. In the process, tensions are kept active and opening up questions seems a virtue.

The Institute of Cultural Affairs (ICA) is an international organization whose approach has developed through several decades of experience "facilitating a culture of participation" in community and institutional development. Their work anticipated and now exemplifies the post-Cold War emphasis on a vigorous civil society, that is, of institutions between the individual and, on one hand, the state and, on the other hand, the large corporation (Burbidge 1997). ICA plannning workshops involve a neutral facilitator leading participants through four phases of envisioning and re-visioning the challenges they face (Stanfield 2002). The goal of ICA workshops is to elicit participation in a way that brings insights to the surface and ensures the full range of participants are invested in collaborating to bring the resulting plans or actions to fruition.

Such investment was evident, for example, after a community-wide planning process in the West Nipissing region of Ontario, 300 kilometers north of Toronto. In 1992, when the regional Economic Development Corporation (EDC) enlisted ICA to facilitate the process, industry closings had increased the traditionally high unemployment to crisis levels. As well as desiring specific plans, the EDC sought significant involvement of community residents. Twenty meetings with over 400 participants moved through the first three phases—vision, obstacles, and strategic directions. The results were synthesized by a steering committee into common statements of the vision, challenges, and directions. A day-long workshop attended by 150 community residents was then held to identify specific projects and action plans, and to engage various groups in carrying out projects relevant to them.

A follow-up evaluation five years later found that they could not simply check off plans that had been realized. The initial projects had spawned many others; indeed, the EDC had been able to shift from the role of initiating projects to that of supporting them. When the accomplishments were assembled, over 150 specific developments could be cited; together they demonstrated a stronger and more diversified economic base and a diminished dependence on provincial and national government social welfare programs. Equally importantly, the community now saw itself as responsible for these initiatives and developments, eclipsing the initial catalytic role of the EDC-ICA planning process. Still, the EDC appreciated the importance of that process and initiated a new round of facilitated community-planning in 1999 (West Nipissing Economic Development Corporation 1993, 1999).

The West Nipissing plan stands in contrast to the dominant model of relying on specialists to analyze a situation and formulate policy options. The plan built from straightforward knowledge that the varied community members had been able to express through the facilitated participatory process. The process was repeated, which allowed participants to factor in changes and contingencies, such as those that must have flowed from the start of the North American Free Trade Association and the decline in the exchange rate with the USA. And, most importantly, the process has led community members to become invested in carrying out their plans and to participate beyond the ICA-facilitated planning process in shaping their own future.

Some difficult questions for me were opened up by this contrast, given that my own ecological and socio-environmental research has drawn primarily on my skills in quantitative methods. What role remained for researchers to insert into participatory planning the "translocal," that is, researchers' analyses of changes that arise beyond the local region or at a larger scale than the local? Indeed, the "local" for professional knowledge-makers cannot be as place-based or fixed as it would be for most community members. What would it mean, then, to take seriously the creativity and capacity-building that seems to follow from well-facilitated participation but not to conclude that researchers should "go local" and focus all their efforts on one place? (Taylor 2002)

Reflecting on this question during a workshop "freewriting" exercise (Elbow 1981) I came up with the term "flexible engagement." This seemed to capture the challenge for researchers in any knowledge-making situation of connecting quickly with others who are almost ready to foster—formally or otherwise—participatory processes like those of ICA and to enhance, through the experience that such processes provide their participants, the capacity of others to do likewise. The term plays off the "flexible specialization" that arose during the 1980s, wherein transnational corporations directed production and investment quickly to the most profitable areas, discounting previous commitments to full-time employees, their livelihoods, and their localities. Will flexible engagement constitute resistance or accommodation to flexible specialization? This remains an open question.

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