

SOCIAL-ECOLOGICAL SYSTEMS, VALUES,
AND THE SCIENCE OF “PEOPLE MANAGEMENT”

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ABSTRACT

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This dissertation interrogates a shift in environmental science, policy, and management toward conceptualizing the environment as a social-ecological system. Social-ecological systems science reflects an interdisciplinary effort to understand how individuals and communities achieve their environmental goals through the institutions that they maintain. Though the paradigmatic institutions concern economic behavior (e.g. property rights institutions), the field embraces the social sciences broadly, with contributions from sociology, anthropology, geography, political science, and so on. That said, social science is fairly narrowly conceived; leaders in the field stress that they are studying social mechanisms in order to predict and manage social behavior. In a popular textbook on the subject, Fikret Berkes and Carl Folke stress that “resource management is people management” and call for a social science of this management.

Social-ecological systems scientists have generally neglected the ethics of people management—for the most part they subscribe to a fairly typical fact/value dichotomy according to which scientists describe social-ecological systems while managers and policymakers prescribe actions in light of these descriptions. Following several philosophical traditions (in particular pragmatist philosophy of science), I call attention to the ways that social-ecological systems science is value-laden. I take environmental pragmatism to provide a roadmap for conducting social-ecological systems science ethically. Environmental pragmatists stress that

science is always embedded in practical problem-solving activities that presuppose particular goals for, and side constraints to, inquiry. Many traditions in the philosophy of environmental science embrace social science for the specific role of facilitating this deliberation, but these traditions do not seem to anticipate the explanatory ambitions of social sciences. This leaves unaddressed several pertinent questions about how social explanations work (i.e. how functional distinction structure inquiry), which have very practical implications for which social science disciplines should be included in a collaboration and how social and ecological knowledge should be integrated. For example, most social situations are characterized by property rights institutions, cultural traditions, political alliances, and other social institutions within the purview of particular social science disciplines, but researchers are not reflexive about whether to explain environmental change according to one set of practices or another.

The dissertation traverses the following terrain: the first chapter more carefully motivates the questions above regarding the need for ethics and the promise, but present inadequacy, of environmental pragmatism to meet this need. Chapter two attends to Dewey's theory of inquiry, in particular the dialogical dimension of inquiry that authorizes warranted assertions. Through reflection on Daniel Bromley's volitional pragmatism and a debate between Richard Rorty and hermeneutic social scientists, chapter three attends to the way that social science structures inquiry in order to intervene in the normative practices of a community. Chapter four analyzes social-ecological explanations in order to locate normative and evaluative assumptions that should be accountable to democratic deliberation. Finally, chapter five redescribes interdisciplinary integration as an ethical project where decisions about the centering and decentering of different sciences is as much ethical as epistemological.

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CHAPTER 1

THE SCIENCE OF PEOPLE MANAGEMENT

Abstract: This chapter argues that the development of social-ecological systems science, and in particular the practice of explaining environmental change in terms of social mechanisms, presents a number of philosophical and ethical challenges. Founding figures in social-ecological systems science aspire to a science of “people management” that understands the mechanisms governing social institutions and norms. These mechanisms are investigated by disciplines as diverse as institutional economics, political science, geography, and anthropology. By attending to examples of social explanations in this literature, I argue that explaining environmental change in terms of particular social mechanisms (or according to particular social scientific disciplines) necessarily involves making normative and evaluative assumptions that complicates the fact/value dichotomy presumed by researchers. I introduce environmental pragmatism as a way of diagnosing such normative and evaluative assumptions, submitting these assumptions to democratic deliberation, and thereby working toward the legitimate and just integration of facts and values. Still, environmental pragmatism is currently ill-equipped to grapple with the full significance of a science of people management. I close by considering three emphases in environmental pragmatism that stand in the way of analyzing social-ecological systems science: (1) emphasis on the physical world but not the social, (2) emphasis on the goals of environmental management but not on the means for achieving these goals, and (3) emphasis on the intentional character of human action but not on its habitual character.

Social Explanations of Environmental Problems

In the Malinau District of East Kalimantan, Indonesia, communities grapple with the decentralization of forestry management. Traditional and industrial interests vie for control of valuable timber in the district’s rainforests; historically, the local communities have received few benefits from the extraction of their natural resources. Inadequacies in the region’s governance has led to an undervaluation of ecosystem services such as water purification and sustained fish stocks. Further, the governance system has failed to ensure that the benefits of logging are fairly distributed among the diverse stakeholder groups in the district. An interdisciplinary team of researchers has diagnosed these failings as the result of weak political institutions. Proper valuation of natural resources, and equitable distribution of their value, are difficult to achieve

when “customary land tenure and associated property rights are frequently contested by different groups and villages” (Lebel et al. 2006, 19). Without recourse to a stable institutional framework, the compensation that local stakeholders receive is small, and “promises made about payments or services to be provided are seldom kept” (Lebel et al. 2006, 19). The environmental problem of over-logging in the Malinau District results from inadequate social institutions, and the solution to such an environmental problem is a social solution. In particular, it is a social solution informed by institutional economics, which can explain how different property rights systems shape the management of common-pool resources such as the region’s forests.

In the Gulf of Maine, an important fishing ground in the United States’ New England fisheries, overfishing has led to decline of key commercial fish species for the region. Here common-pool resource management has struggled to interrupt the collapse of the gulf’s groundfish species such as the iconic cod, flounder, halibut, and sole. Management strategies unattuned to the complex social and ecological dynamics of the gulf have mistakenly managed the fishery as though these species are homogenously distributed throughout the area. Ecologically, the complex population structures of these fish are remarkably heterogeneous, and “localized ecological adaptations of fish, for example, local spawning groups, nursery areas, and so on, are ignored or assumed to average out over the management area” (Lebel et al. 2006, 19). Socially, fishing practices also flaunt the assumption of homogeneity, as the “fishermen, like fish, are diverse and operate on many scales” (19). An interdisciplinary team of researchers has begun to investigate these diverse operations, tracking the development from the local operation of small-scale fishing to technologically specialized operations that can chase down patchily distributed stocks. Again the environmental problem of the fishery collapse has a social origin, one which can only be appreciated by studying mismatches between the locations and scales of

management, fish populations, and fishing operations. Here the institutional economist on the team would give way to the geographer, who has the requisite training to offer explanations in these terms.

For the Chisasibi First Nation of Cree, Quebec, Canada, the hunting stewards of the Chisasibi Cree worked to develop new hunting practices in response to the return and subsequent overhunting of caribou to the territory. The caribou had been absent from the region for seventy years, and hunters were particularly wasteful in their first hunt after the return. The hunting stewards succeeded in reinstituting traditional management by “reinforcing the oral history lessons” of the community. These history lessons were vital to the culture of the Chisasibi Cree, and the lessons undergirded stewardship practices for sensitively managing caribou, beaver, and fish. Traditionally, “rules and enforcement are decided collectively by the stewards, who provide a second governance layer above the community in the form of the Chisasibi Cree Trappers Association” (Lebel et al. 2006, 19). Here an interdisciplinary team of researchers documented the rebirth of customary caribou hunting practices in light of the efforts of the Trappers Association. Stories of past declines of the caribou, and the caribou’s subsequent “70-yr ‘retaliatory’ absence,” impressed upon hunters the need for stewardship (19). Here it is an anthropological account of the environmental problem that reveals the ways that social practices are the cause and consequence of environmental changes. The team’s anthropologist is in a unique position to understand how customs and traditions helped to reinstitute stewardship practices and sustain the growth of caribou populations.

These three case studies are drawn from Louis Lebel and colleagues’ “Governance and the Capacity to Manage Resilience in Social-Ecological Systems” (2006). They represent three of nine examples drawn from throughout the world of how analyses of social dynamics can

contribute to our understanding of ecological change. Here they offer an introduction to social-ecological systems science. Social-ecological systems science reflects a turn in applied environmental science and management toward an appreciation of how the environment is shaped by both ecological and social dynamics. The ambitions of social-ecological systems researchers are significant; by coupling our understanding of ecosystems with an understanding of social systems, environmental scientists, managers, and policymakers will be better equipped to predict, control, and adapt to environmental change. Lebel and colleagues use these examples to locate several social mechanisms that help to explain environmental changes and thus serve as candidates for social-ecological systems research.

When I first encountered these sorts of case studies in the social-ecological systems literature, I was deeply impressed, but as a philosopher I was also interested in the way that these social-ecological explanations work. I should be very clear that I believe they do work—that is, by describing social-ecological systems according to mechanisms like those surveyed by Lebel and colleagues, communities are better able to predict, control, and adapt to environmental change. In that sense, these scientists are providing true descriptions of these complex systems, and this is no small accomplishment. Yet in the case studies above, particular social mechanisms bear the explanatory burden for understanding complex systems. Researchers explain logging with respect to the property rights institutions (or lack of institutions) governing common-pool resource management. They explain the collapse of fisheries by studying the scale at which fish and fishermen interact, and they explain the collapse of caribou populations by studying traditional stewardship practices and the oral histories through which those practices are shared. Other case studies in the article explain environmental changes by focusing on incentive

structures, or on negotiations between political coalitions, or on the legal procedures that govern civil lawsuits.

The interesting thing about these social explanations is that, in many cases, several of them seem applicable. Social systems are usually characterized by property rights institutions, by multiple scales of coordination, and by traditions and customs; they are usually shaped by incentive structures, coalition building, and legal procedures. Partly, the emphasis on particular mechanisms reflects the disciplinary training of members of interdisciplinary teams, teams that not uncommonly rely on a single social scientist from a single discipline. The institutional economist is able to explain the complex system by investigating property rights institutions, the geographer by investigating the scales of use, and the anthropologist by investigating customs and traditions. Philosophical questions abound: Are the contingencies of researcher expertise enough to justify focusing on some of the countless dynamics that shape social and ecological systems? If not the composition of the collaboration, what warrants explanations that focus on particular dynamics within these very complex social systems?

This first chapter serves two goals. First, it continues the above effort to articulate philosophical and ethical questions that arise in the practical problem-solving activities of social-ecological systems researchers. This goal reflects a programmatic commitment to do engaged philosophy, and in particular engaged philosophy of science, which recognizes occasion for philosophical investigation in the concrete situations facing communities (Thompson 2002). By integrating ecological and social sciences, social-ecological systems researchers give rise to a constellation of philosophical questions. Question that initially appear innocuous—Which social scientists should we recruit for our interdisciplinary collaboration? How should we coordinate diverse and sometimes rival explanations from different social sciences?—turn out to pose

complex philosophical problems. They require engaged philosophers to remember (in the sense outlined by Latour 2004, 154-161) how it is that we warrant explanations at all, and social scientific explanations in particular.

As will become clear through the analysis of scientific inquiry, these philosophical problems are as much ethical as they are epistemological. Pursuing the first goal reveals that methodological decisions inescapably require value judgments. The second goal of this chapter is to sketch out the implications of these value judgments from the point of view of environmental pragmatism. In pursuing this second goal, I want to accomplish two very preliminary tasks. Initially I want to show that environmental pragmatists are correct to call for democratic deliberation to structure inquiry into environmental problems. This structure is reflected most pronouncedly in the language used to describe a problem, a language that environmental pragmatists argue should be constructed and legitimated through public dialogue. But my second task, still preliminary, is to locate a lacuna in environmental pragmatists' theorization of the science-value nexus. While prevailing theories of environmental deliberation do guide us in deciding how to describe the environmental features of problematic situations, we lack a guide for describing the social features of these situations. In this chapter I only want to suggest that the tools we have for describing the environmental features are not the tools we need for describing the social features. These tools are especially important as social-ecological systems researchers become more adept at managing complex problems in light of social explanations. They are also especially important because the ethics of social explanations requires that we countenance values that are not as obviously at stake when describing the environment. Social explanations involve normative and evaluative judgments about *who we are* and *who we are to become*.

The goals, then, are to trace a series of moves from a very practical question—Which social scientists should help explain the dynamics of social-ecological systems?—through a set of philosophical questions—What *is* a social scientific explanation? What does it assume and what does it let us know?—back to another very practical question—By what procedure can we legitimate the use of particular social scientific explanations as the basis for environmental management? These questions are answered over the course of the dissertation, and at this point I want to justify the overall course that I’m charting through these questions. I begin by more closely interrogating the self-understanding of social-ecological systems researchers, in particular how they understand social science as the science of “people management” (Berkes and Folke 1998, 2).

The Science of People Management

In their introduction to the 1998 volume, *Linking Social and Ecological Systems: Management Practices and Social Mechanisms for Building Resilience*, Fikret Berkes and Carl Folke justify their social-ecological approach. Historically, they explain, resource management sciences had treated the natural world as comprising discrete boxes of resources (e.g. timber-producing forests, or fish-producing bays) that yield a flow of goods and services which should be individually maximized. This treatment compromised the sustainability and resilience of different environments, and in order to achieve sustainability and resilience, resource managers needed to fundamentally rethink their understanding of the natural world. The environment would be reconceptualized as an ecosystem in which entities once thought to be discrete were drawn into systematic relationships with other entities. Berkes and Folke report that “the systems approach is replacing the view that resources can be treated as discrete entities in isolation from the rest of the ecosystem and the social system” (2).

Just as vital as this new systems approach, Berkes and Folke advocate for greater appreciation of the social dynamics that bear on social-ecological systems. They argue ...improving the performance of natural resource systems requires an emphasis on institutions and property rights. A people-oriented approach which focuses on the resource user rather than on the resource itself is not a new idea; many have pointed out that ‘resource management is people management’. However, tools and approaches for such people management are poorly developed, and the importance of a *social science* of resource management has not generally been recognized. (2)

The project of this dissertation is to grapple with the ethical import of “people management.” Unfortunately, social-ecological systems researchers have reflected little on the ethics of this science. At risk of overgeneralization, most of these researchers operate under a traditional but problematic understanding of the science-policy interface. According to this traditional understanding, scientists describe the world in value-neutral terms and policymakers bring back values when deciding how to respond. This traditional understanding is complicated by efforts to co-manage natural resources (Olsson, Folke, and Berkes 2004), or develop place-based or participatory management strategy (Gunderson and Light 2006, Walker et al. 2002), but even in these efforts the goal is to abbreviate the distance between description and decision, rather than to integrate the two. The result of this understanding that concerns us at the moment is that social-ecological systems researchers rarely step back to reflect on whether and when “people management” is an ethical science, since on the prevailing self-conception, science is neither ethical nor unethical.

In order to understand more precisely what this social science of people management would investigate, it’s worth unpacking Berkes and Folke’s definitions and examples. The social

side of social-ecological systems is characterized by institutions, and the paradigmatic institutions concern property rights. The broader category of institutions is formally defined at the outset of the volume: “Institutions are defined as ‘humanly devised constraints that structure human interaction. They are made up of formal constraints (rules, laws, constitutions), informal constraints (norms of behavior, conventions and self-imposed codes of conduct) and their enforcement characteristics’” (5). Institutions “organize repetitive activities that produce outcomes” for a set of individuals (5). The definition is meant to be inclusive, covering any individual activity that means something for another individuals’ activity. Property rights are understood as a particular type of institution; property rights institutions are a complex arrangement of practices that coordinate the use, management, and exchange of resources. Of particular interest to social-ecological systems researchers, some property rights govern the use of common pool resources, and these institutions are especially important for understanding these systems. Generally, according to Berkes and Folke, “local systems of rights and responsibilities develop for any resource deemed important for a community. Even under rapidly changing conditions, there are usually incipient property rights; rules arise and evolve according to local needs” (7).

A more thorough example here is illustrative. Elinor Ostrom offers a foundational study of institutions significant for social-ecological systems research in her 1990 *Governing the Commons: The Evolution of Institutions for Collective Action*. Here Ostrom calls into question key assumptions of resource managers, specifically, assumptions that the collective use of common pool resources will lead to the degradation of those resources and the ecosystems that sustain them. A classic case of this degradation is the well-known “tragedy of the commons” (Hardin 1968). Tragedies of the commons affect common pool resources, such as the East

Kalimantan forests or Maine fisheries, which are open to all resource users. Indeed, many environmental goods can be thought of as “commons,” which secures the research significance of commons and the institutions according to which they are managed. The paradigm commons is a meadow where shepherds pasture their sheep. Now, from the point of view of the shepherding *community*, it is imperative that the sheep do not overgraze the meadow and compromise the ability of the commons to regenerate fodder for the sheep. But each *individual* shepherd faces a dilemma; unsure whether her fellow shepherds will appreciate this imperative, she must decide whether to increase her herd and graze more intensively. The rational response to this uncertainty, at least according to classical economic frameworks, is to always increase the intensity of one’s grazing—after all, if others restrain themselves, your own intensification may be sustainable, and if others intensify, your own restraint will go unrewarded. In any scenario, intensification is the smart strategy. Yet this leads to the counterintuitive results that, when everyone acts in their individual self-interest, the result is in the best interest of none. Hence the tragedy of the commons.

Ostrom observes that worries like these (generally, where individual rationality is at odds with what all take to be the optimal outcome) have informed the dominant approaches to environmental management and policy. Two schools of thought have emerged to grapple with these dilemmas; either the environment should be controlled by the state (Ostrom’s Leviathan) which governs with the best interests of all in mind, or the environment should be privatized, divided into property under sole ownership by individual actors. Absent these approaches, common pool resources will succumb to some host of market failures that threaten their exploitation and collapse. Yet, as Ostrom chronicles in *Governing the Commons*, communities around the world have developed social institutions that mediate both of these extremes and yet

circumvent tragedy. Her first example (based on Netting 1976) conveniently resembles the famous sheepherding thought experiment. In the Swiss Alps, villagers must coordinate the grazing of alpine meadows to avoid overgrazing. Villages democratically pass statutes, commonly known as “cow rights”, which limit the number of cattle that each villager can send out to pasture in the commonly owned lands. Key among cow rights is the “wintering rule”, according to which no villagers may pasture more cattle than he can commit to feed over the winter; since the economy of the village revolves around cheese production, pasturing beef cattle with Autumn expiration dates is effectively curtailed by the wintering rule. Statutes are enforced by an Alps association that monitors grazing, and the general system of commons management reaches back to at least 1507.

The burden for Ostrom is to explain how these institutions evolved, and why neither Leviathan nor privatization came to dominate the Swiss Alps. Here the account is rich in details of the social context of these herdspeople. Following Netting, Ostrom locates several features of the economy that help to explain the evolution of these institutions: the value of production per unit of land is low, the dependability of yield is low, the profitability of intensification is low, high upfront capital-investment is required, and so on. These features are significant because, by keying on economic realities, they relate this context to the paradigm of sheepherding at the core of Garrett Hardin’s influential “The Tragedy of the Commons” (1968).

I want to draw attention, for a moment, to some features of the social context that do not key on economic features. Alongside the features of the economic reality, Ostrom also includes details about the region’s political procedures, the village’s traditions for inheriting land holdings, the topography of the meadows, and the spirit of the culture regarding monitoring and enforcement. At least on their surface, these variables are not the subject of institutional

economics but of fields like political science, anthropology, geography, and sociology; each of these fields studies the causal relationships between, for instance, a village's traditions for inheriting land holding, and other anthropological interests like the cultural norms surrounding gender and family structure. The metaphor of "keying" is helpful here, as it points to the ways that some notes (terms) sound right in one field or key, as we understand the relationship between that note and other notes in that key. Part of training within a field is to learn which notes agree with which. Now, I don't doubt that notes in, for instance, sociological or geographic keys can be tuned so that they chime with economic variables—after all, I agree with social-ecological systems researchers that all of these features hold together in a system. Tuning would involve transposing our understanding of an anthropological note like the village's traditions into a more economic note (perhaps these traditions are a matter of "social capital"). Just as they can be tuned to chime with economics, they might be tuned to chime with political science, geography, or sociology. The openness raises a critical challenge—if all of these features work together to produce the social-ecological system, how should we decide what strings to pull to produce social-ecological change?

Ostrom's example offers a clear sense of what institutions are and why it is important to understand institutions in order to understand the whole system. Care is taken in the social-ecological systems literature to maintain an objective stance toward institutions and property rights. The case studies throughout Berkes and Folke's *Linking Social and Ecological Systems* address two objectives central to social-ecological systems research:

- how the local social system has developed management practices based on ecological knowledge for dealing with the dynamics of the ecosystem(s) in which it is located; and

- social mechanisms behind these management practices. (3)

The proper purview of a social-ecological systems researcher qua social scientist is to understand the causes of various institutions so that an understanding of these causes can inform environmental management and policy. Characteristically, social-ecological systems researchers investigate social *mechanisms*. Of course, these scientists appreciate that the mechanics of social-ecological systems are complex and, in a word, systemic. A *mechanistic* understanding is not intended to contrast with a holistic understanding.¹ Rather, a mechanistic understanding is an understanding that explains and predicts the behavior of individuals and social systems in terms of their causes, complex as they might be.

Berkes and Folke are hardly alone in their emphasis on mechanistic explanation. Neil Adger and colleagues explain that social-ecological systems achieve resilience by means of “diverse mechanisms for coping with change and crisis,” and that social mechanisms such as “institutions for collective action, robust governance systems, and a diversity of livelihood choices” increase resilience (2005, 1037-1038). Brian Walker and colleagues (2002) note that “we know that both social and ecological systems have self-reinforcing mechanisms” and that one goal of social-ecological systems science is “to describe these phases and the underlying mechanisms that give rise to them, and variables that effect those mechanisms” (14). Social scientists that participate in interdisciplinary environmental research often report that their biophysical counterparts prefer hypothesis-driven, mechanistic understandings over interpretive, often qualitative understandings (Gardner 2013, MacMynowski 2007, Campbell 2004). Despite these social scientists’ objections, leaders of the field uphold mechanistic explanation as the *sin qua non* of social-ecological systems science. Yet the preference for mechanistic explanation

¹ Berkes and Folke do note that social-ecological systems scientists do not go as far as the deep ecologist in their praise of interconnectedness (2)

writ large, or the mechanisms within the purview of particular disciplines, is rarely subject to critical scrutiny.

Some summary here is in order. Social-ecological systems research reflects a turn in applied environmental science to appreciate the social dimensions of environmental change. Systematic understandings of the environment require insights from both biophysical sciences and social sciences. The role of social scientists in these collaborations is to comprehend the social institutions that influence the environment, such as property rights for managing common pool resources and social practices such as local customs, legal procedures, and coalition building. While any of these social practices might invite research, social-ecological systems science is dedicated to discovering social mechanisms underpinning these practices—for now we might say that they are interested in understanding the causes and effects of these practices in order to better predict, control, and adapt to these mechanisms. Such a science of people management seems to give rise to philosophical and ethical questions that neither social-ecological systems scientists nor philosophers of science have engaged.

Inquiry, Facts, and Values

Part of social-ecological systems scientists' ignorance of ethics stems from problematic assumptions about the science-policy interface. If scientists did indeed procure value-neutral descriptions of complex social-ecological realities, so that policymakers could resolve what these descriptions meant for management and policy, then scientists could safely neglect many of the ethical implications of a science of people management. Part of the task here is to interrogate this picture of the science-policy interface, show that it is woefully misguided, and (for the moment) sketch the ethical implications that scientists can no longer neglect. This demonstration begins with a story shared by environmental pragmatist Bryan Norton before countenancing some time-

honored philosophical questions such as “What is an explanation?” Again the goal is to move from very practical questions about which disciplines to include in a collaboration to still very practical questions about how to facilitate stakeholder engagement, and to justify the series of transformations between the questions.

In his *Sustainability: A Philosophy of Adaptive Ecosystem Management*, Bryan Norton recounts his experience working with the U.S. Environmental Protection Agency (2005). Norton’s role was to help a team composed largely of ecologists and economists propose policies informed by the proper valuation of ecosystem services (things like water purification and sustained fish stocks). As a philosopher, Norton was invited in part to build bridges between the very different scientific communities of ecology and economics. The particular bridge that needed to be built would span the ideally value-free descriptions offered by ecologists and the ideally value-neutral evaluations offered by economists. For example, ecologists would eschew terms or phrases that might evoke human values, such as “ecosystem health” or “integrity,” in favor of terms or phrases that were free of the evaluative connotations that might be associated with those terms. Ecologists would instead study how hypothesized policies and management strategies would bear on ecosystem services, and economists would study consumer preferences to estimate the dollar value of these services. Get the ecological mechanisms correct and the dollar values correct, and policymakers could straightforwardly mandate whichever policies would optimize the total value produced by the ecosystem.

Norton suspected that this procedure was not as value-neutral as it espoused. The problem with deferring to economists’ valuation of ecosystems is that economists are better at measuring some values than others. Economists are very good at appreciating the value that individuals receive from using resources, okay at appreciating the value that individuals receive

from preserving resources, and pretty bad at appreciating the value that communities receive in sustaining ecosystems. This is not because economists' hearts are in the wrong places—they very much want to properly value ecosystems—but because the theories and methods of economics were developed to understand a particular range of values (generally individual use value in the short term). The supposed value-neutrality of economics turns out to be fundamentally biased to favor this particular range of values. The trouble, from Norton's point of view, is that scientists and policymakers have no way to weigh values beyond this range. Evocative terms such as ecosystem health and integrity are getting at something, and these vague notions need to be articulated, not purged. Norton's bridge building efforts located an important gap in the way that scientists and policymakers understood ecosystems; they needed a crucial set of terms that *described* environmental changes in ways that were directly significant for management and policy. Far from purging values from the way that ecosystems were described, scientists and policymakers needed to reenchant the environment so that the significance of our management practices was transparent.

To a non-philosopher, Norton's emphasis on language might seem peculiar. After all, scientists don't think of themselves as in the business of telling us what we ought to do; their goal is to remain as objective as possible, and to simply "tell us how it is." When "it" determines what scientists tell, there seems little room to argue over semantics. For now I want to forego some of the esoteric philosophy required to dismantle this theory of knowledge and quickly motivate a tentative alternative to be fleshed out in Chapter Two. On the traditional theory of knowledge, the idea of "telling it how it is" makes good sense; there is a fact of the matter about what exists in the world, and scientists need to name this fact to the best of their abilities. Claims that get these facts right are true, and scientists' main goal is to discover these truths. On the

alternative theory of knowledge that Norton embraces, there are too many facts of the matter to make good sense of the idea of telling it how it is. In particular, the idea of “it” loses any clear sense, as does the clear separation of the “telling” from the facts to be told. Norton’s emphasis, following pragmatism, is that the language in which we do the telling makes a huge difference for what facts the scientist discovers and what facts we organize our lives around. The economist speaking in one key (say the dollar valuation of ecosystem services) will make sense of an environmental problem differently than the economist speaking another (institutions governing common-pool resource use). The population ecologist will concern herself with different facts than the systems ecologist, and even more obviously, both of these ecologists are concerned with altogether different facts than the economists.

Norton and his philosophical allies appreciate this radical plurality of facts and resolve that whatever’s “out there” isn’t going to settle what we have to say about it. Because there are so many facts around which we could organize our lives, part of the challenge of science is to locate the significant facts, the ones that actually do make a difference for how we get about in the world (Kitcher 2011). Pragmatism offers the pretty mundane observation that we key on facts relative to our goals and interests. On this view, modern science is not the heir to classical quests for essences and eternal truths; science is the heir to designing plows, navigating the ocean, and treating illnesses. Mixing up these two lineages, so that scientists treat illness by discovering the essences of pathogens, adds nothing to the logic of science, save confusions such as the science-policy (or fact-value) dichotomy. Sometimes scientists discover new technologies to help us reach our goals, other times they discover new scientific descriptions that serve as solid guides for our pursuits, and still other times it becomes difficult to pull technoscience apart.

Perhaps because technologies so often advertise their goals, Norton focuses on the tougher cases of scientific descriptions. How is it that descriptions of the way the world is implicate particular goals and interests? Why isn't it possible to keep the facts separate from our values? The general answer—again to be elaborated in the next chapter—is that descriptions bring to bear a language that gives structure to scientific inquiry. Languages offer names for some of the plurality of objects we encounter in the world, and language codifies norms that help us respond to these encounters. In the language of some ecologists, blooming and buzzing confusion is experienced as an ecosystem, an ecosystem that we might label resilient or fragile (see James 1890 on blooming and buzzing confusion). According to these labels and what they mean for our community, we might restore stream banks or create habitat for pollinators. Notice that the language does not create the blooming and buzzing confusion that it names, and also remember that when two scientists disagree about what to name a blooming, buzzing confusion, they submit to their peers their various reasons for one name or another. Scientific inquiry is concerned with facts, and it achieves objectivity by abiding by norms of the scientific community. The main thing that falls away from the traditional theory of knowledge is the idea that some (or likely one) language gets the facts right, all of the facts, without any tainted by the values that motivated our inquiry.

Pragmatists like Norton are especially attuned to the way that inquiry is ameliorative—that it produces beliefs that help individuals improve their situation. This is the goal-setting dimension of inquiry, and at issue for environmental managers is that they have no way to describe inchoate environmental goals that do not register in economic studies. Norton summarizes, “in public policy debates concerning environmental choices, we lack a crucial type of term that can (1) encapsulate a great deal of information and (2) present this information in

such a way that its importance for widely held social values is transparent” (50). Pragmatists recognize that the construction of this type of term is a matter for democratic deliberation, where democratic deliberation is understood as inquiry into the values that the community should hold. Part of this is to say that while scientists can be trusted to review one another’s use of terms like “ecosystem,” they are not the best judge of how to use terms like “desirable” or “just.”² Just as the application of terms like “ecosystem” must be submitted to the careful scrutiny of one’s colleagues, the use of terms such as “desirable” or “just” must be adjudicated by those expert in making such judgments. Democracy is characterized by granting expertise about these ethical norms to everyone. Just as the scientist must offer reasons for declaring blooming and buzzing as signs of ecosystems, citizens must exchange reasons for finding a situation desirable or undesirable, just or unjust. This giving and taking of reasons constitutes ethical inquiry.

Ethical inquiry is not willy-nilly declarations of what one would prefer or lament; it is structured by preexisting commitments to democratic norms such as egalitarianism that stakeholders must share if they are going to pursue ethical truths at all (Putnam 2002). These commitments provide the ground that stabilizes inquiry into the uncertain values that ought to inform environmental science, such as whether an ecosystem has integrity. When inquiry into community values relies on such a structure, the language ordained through deliberation has promise to produce agreement between the community’s way of life and the environment it must navigate. This ordainment eschews a division of labor between a descriptive language—presently ecology—and an evaluative language—presently, by and large, economics. Insisting on a value-free environmental science amounts to “the construction of alien appendage to the world in which we do our living” (Sellars 1963, 40). Democratic deliberation mobilizes a

² The difference here is not hard and fast, however; it’s not clear who the best judge of terms like “integrity” might be, and indeed these discussions need both scientists and stakeholders.

vocabulary consonant with our being “first and foremost democrats” (Norton 2005, 251) and enrolls environmental science as a set of tools capable of directly addressing problems faced by the community.

Norton offers the deliberative construction of metrics as a clear example of how facts and values can be integrated through the negotiation of goals. Basically, environmental managers need terms that measure environmental change while fairly directly motivating public and private action. The problem is that the metrics that we do have either (1) require substantive debate over the meaning of the descriptions, e.g., what environmental health looks like in empirical terms, or whether and when we should be committed to protected environmental health, or (2) in the case of environmental economics, capture only a subset of the values at stake and impede the intelligent management of legitimate but competing goods (2005, 137).

Obviously, these thick ethical terms will not arise out of thin air, and less obviously, environmental scientists are unlikely to discover descriptions that coincidentally resonate with social values. Hence these terms need to be constructed communally such that their significance for environmental management is negotiated and internalized by the community from the start. Because a diverse community may attach significance to different thick terms for a plurality of reasons, deliberation holds out hope that alignment of values can be achieved in the interplay of deliberation and adaptive management of the environment.

Norton’s *Sustainability* goes a long way toward transforming a question about scientific practice (how should economists and ecologists collaborate in describing the environment?) to a question about stakeholder engagement (how should democratic deliberation settle on metrics for interdisciplinary investigation?). It charts a path that I will more or less adopt. First, we must explicate the ways that existing theories and methods presuppose values. Values inform both the

goals that inquiry sets and the practices that are taken for granted as fixed and stabilizing.

Second, we must resolve which of our many values ought to inform inquiry in a given case. For environmental pragmatists, these resolutions follow from democratic deliberation. Turning values questions over to democratic deliberation is consistent with our prior commitments to egalitarianism and inclusive participation.

While Norton's *Sustainability* offers a detailed guide of one path, the opening exploration of social-ecological systems research suggests uncharted terrain for environmental pragmatism. As Berkes and Folke point out, social-ecological systems science is characterized by two distinct commitments. The first of these commitments is to understand the environment as a system, governed by threshold effects, feedbacks, and other non-linear mechanics. It is this commitment that Norton and other environmental pragmatists have elegantly theorized in their appreciation of the values that ecosystems secure that are undervalued by conventional evaluations. While social-ecological systems scientists are liable to use terms such as 'resilience' without appreciating the dual descriptive and evaluative role of these terms, I don't believe these liabilities are due to inadequacies in pragmatists' theorization of adaptive ecosystem management.

The second commitment that Berkes and Folke uphold is the commitment to a people-oriented approach. As analyzed above, this commitment takes as its ideal a science of "people management" pursued through mechanistic understandings of human behavior. This, I contend, is undertheorized in environmental pragmatism, and generally undertheorized among ethicists of science. Generally, ethicists of science agree that social science offers a valuable contribution to environmental inquiry, but for the most part, the role of social science is to ascertain the values of community members (Norton 2005). This is a crucial role—if fully inclusive democratic

deliberation was required for every environmental management decision, we would spend all of our time deliberating and very little of our time savoring the fruit of these deliberations. It is, however, a very different role than the social-ecological systems scientists have in mind—by and large, they are more concerned with accurately describing complex systems, and less concerned with the legitimacy of these descriptions (that’s the job of policymakers). Human values are important to many social-ecological systems scientists only insofar as they help to explain human behavior.³ Social-ecological systems scientists seek a science of people management because a more mechanistic understanding of human behavior would help predict, control, and adapt to environmental change.

Three Unwarranted Emphases in Environmental Pragmatism

The remainder of this dissertation offers a detailed exploration of how to diagnose the normative and evaluative assumptions of social-ecological systems science, and how to subject these assumptions to democratic deliberation. Before embarking on that exploration, it’s worth interrogating Norton’s dominant account of environmental pragmatism, which reveals three emphases at odds with the thrust of social-ecological systems science. First, environmental pragmatism has focused on describing the ecological dimensions of social-ecological problems; on this view, policies and management strategies produce different ecological consequences rather than different social-ecological consequences. Related, but distinct, is a second focus on the goals of inquiry at the expense of careful attention to the means of realizing these goals. Recall that we can draw functional distinctions in inquiry between the goals that inquiry sets as well as the practices spared from reconstruction, and that values play a role in resolving both

³ Often following Heberlein (2012), many social-ecological scientists will lament that values are a poor predictor of behavior, and thus contribute little to understanding social mechanisms. The point here is that we often study values in order to legitimate environmental management and policies, and in these studies, the connection between values and behaviors is beside the point.

ends and means. Third, some environmental pragmatists are too optimistic that, armed with good reasons, communities will engage in social practices in accord with these reasons. What studies of the social side of social-ecological systems suggest is that our practices are not quite so free, and that many of our habits are less deliberate, or amenable to deliberation, than we might hope.

Consider the first emphasis. Most of the examples in Norton's *Sustainability* involve describing the environment in ways that capture what it is that we find valuable about the environment. This involves finding a language that can describe landscapes, or ecological processes with long temporal horizons, in a way that can be scientifically studied and immediately significant for environmental managers and policymakers. While Norton no doubt admits humans as members of ecosystems, most of the work he recommends involves redescribing the non-human environment. Indicators might track the biodiversity of threatened ecosystems, or the recreational quality of hiking trails, but rarely does Norton discuss indicators for the trustworthiness of logging companies, or the social sustainability of idyllic New England fishing communities. This is a problem if social-ecological systems theorists are correct that certain ecosystems are only sustainable when coupled with certain social systems. In other words, we need to begin to consider environmental outcomes together with the social outcomes with which these environmental outcomes are coupled. Other environmental pragmatists have been much more appreciative of the ways that social and ecological systems hold together. Paul Thompson has explored the ways that agrarian social practices constitute and are constituted by agricultural ecologies. As long as it is the case that sustaining particular ecosystems requires sustaining complementary social practices—which is a founding commitment of social-ecological systems research—then redescribing ecological dimensions, alone, is insufficient. We

need a language to evaluate both the ecological features of a social-ecological system and the social features.

Second, environmental pragmatism tends to focus on the goals of social-ecological inquiry without as much attention on the means by which these goals are realized. By democratizing the way that we describe the environment, environmental pragmatism ensures that ecological inquiry investigates ways of managing the environment that are responsive to our goals. When ecologists study the effects of different policies on biodiversity, their conclusions help us decide which of these policies we might prefer given a prior commitment to preserve biodiversity. If we think of explanations in terms of the effects that we explain (i.e., the *explananda*) and the “causes” that do the explaining (i.e., the *explanantia*), we might say that environmental pragmatism has so far focused on the language of explananda. But, as mentioned earlier, most social-ecological systems are the effects of too many causes to enumerate. Property rights institutions, the scale of fishing management, or the medium of cultural transmission might all serve as the explanans for a given explanandum. Norton has overlooked conceptual challenges on the explanantia front in part because his decision context is usually characterized by a polity debating between the implementation of a set of policy options. In that situation, it makes sense to evaluate the consequences of these different policies options in a language that does justice to the explananda. But that is not the only situation in which we find ourselves—sometimes we agree on what sort of social-ecological system we want to sustain, and we inquire into the various means of realizing that system. Environmental pragmatism has generally had less to say about the sort of democratic procedure that could legitimate the social scientific investigation of some means instead of others. Since it is exactly these means—social mechanisms—that social-ecological systems researchers seek to better understand, philosophers

of science need to scrutinize the values implicit in these understandings and the ways that such values might be legitimated through democratic deliberation.

Third, environmental pragmatism's emphasis on democratic deliberation is both a virtue and a vice. By focusing on democratic decision-making, pragmatists like Norton have shown how deliberation can integrate facts and values in the most ethical method we have so far developed. The trouble is that this emphasis can give the mistaken impression that all human conduct is the result of deliberate decision-making. Pragmatists have long emphasized that most human conduct is actually habitual, and that it is only when our habits fail us that we engage in conscious reflection. Classical pragmatists like James and Dewey studied psychology in addition to philosophy, and they admitted that our intentions played little part in many of the regularities of human behavior. Thompson's studies of agrarianism likewise attend to cultural practices that are more habit than reflective thought. When Norton relegates social science to facilitating communication between stakeholders (2005, 302), or somewhat more broadly to help evaluate competing policy proposals (298-301), he has abandoned this earlier pragmatist program.⁴ Yet it is this pragmatist program echoed in the social science of social-ecological systems research; Berkes' and Folke's emphasis on "repetitive activities that produce outcomes" can be thought of as an emphasis on habit. Their's is a social science that seeks to explain human conduct. Even if Norton is right that conscious decision-making is required to legitimate some courses of action over others, it would be an obvious mistake to think that all of our actions are the result of such dedicated attention. One of the promises of the social sciences is to reveal truths about us that we ourselves had failed to notice, and that once brought to our attention we can criticize or cherish.

⁴ In his discussion of the merits of social science, Norton does offer one cryptic remark that he doesn't develop: "It is possible to use social science methods to develop more comprehensive evaluative tools, tools that will allow us to move toward an articulation of a particular community's sense of identity, including who the community, collectively, really wants to be." This is the closest he comes to thinking of social science as answering questions about who we are and who we are to become, but he doesn't pursue that idea further.

Theorizing the science of people management requires tracing the same sorts of steps that environmental pragmatists have traced for the ecological side of social-ecological systems. Loosely, this requires recognizing the values implicit to social scientific explanations of human behavior, and then facilitating democratic deliberation that could countenance and legitimate these values. We need to come to think of social scientific inquiry as problem-solving, characterized by both assumptions (who we are) and goals (who we are to become). More concretely, this rethinking requires that we distinguish between institutional economics, human geography, and cultural anthropology (to name only those sciences featured in the opening) according to the values that structure their inquiry. At present, social-ecological systems scientists seem to think of these sciences as rivals, each offering a model of human behavior that is more or less predictive (Schluter et al. 2007; Fabinyi, Evans, and Foale 2014). Whichever science more truly predicts social dynamics is thought to better grasp social reality, and this grasp warrants our reliance on that particular science. The normative assumptions that social sciences make about social reality should not be thought of as themselves hypotheses that might be falsified, but as practices that a community might sustain, and that through sustaining, uphold the social reality presupposed by inquiry.⁵ Deciding to sustain particular practices (for example property rights institutions or cultural traditions) depends on whether we value those practices, and, at the same time, brings about a context where explanations reliant on those practices will be true. Hence, if social sciences present themselves as rivals, it is not as rival models of a pre-existing social reality, but as rival proposals for a way of life.

⁵ The difference between assumptions in social science and something like auxiliary hypotheses in the biophysical sciences is that the truth of these social scientific assumptions is something that *we make true* depending on the social practices we *choose* to sustain. When an auxiliary hypothesis in the biophysical sciences proves to be false, we are usually forced to abandon the assumption. When an assumption about community norms proves to be false, that community can choose to make it true, and thus to recontextualize our inquiry.

Insofar as social-ecological systems sciences do not understand social explanations in this way, they will fail to pose the right questions in stakeholder deliberations. Stakeholders need to be able to deliberate about questions like “Are the norms that researchers presuppose in their explanations norms that the community values?” and “Are explanations in terms of property rights institutions the right basis for describing and managing the environment, or would explanations in terms of the scale of governance be more valuable?” At the end of the day, such questions boil down to anticipating various social practices that might bring about a desirable social-ecological future, then deliberating over which social practices are consistent with preexisting commitments (e.g. egalitarianism and inclusivity). These deliberations separate truths about social realities, of which there are many, from significant truths about social realities, truths that answer the questions legitimated through deliberation, of which there are fewer (Kitcher 2011). Whereas deliberations over ecological metrics are somewhat straightforwardly questions about what environments we most value, deliberation over social practices are more complicated questions about who we are and who we are to become.

Conclusion, and the Path to Come

The goal of this first chapter was to show how certain philosophical questions arise in social-ecological systems science and suggest why these questions are inadequately addressed by environmental pragmatism. Social-ecological science calls for social scientists to provide a mechanistic account of the social institutions that bear on social-ecological systems. These scientists have overlooked the ethical implications of their inquiry, in part because they mistakenly understand the science-policy interface as a gap between value-free science and value-laden policy. This gap, symptomatic of a deeper fact/value dichotomy, is philosophically untenable—inquiry necessarily invokes values, in part because inquiry relies on language, and

language use, as complex social practices, is pervaded by values. Environmental pragmatists such as Bryan Norton have prescribed democratic deliberation to dissolve this fact/value dichotomy and integrate facts and values in our language. This is more or less the right path, save for three emphases: environmental pragmatists have tended to (1) focus on redescribing only the environmental features of social-ecological systems, (2) focus on redescribing only the goals, but not the means, of social-ecological change, and (3) ignore the extent to which our conduct is habitual. Environmental pragmatism must recognize the values implicit in social science, and facilitate democratic deliberation about the legitimacy of these values, if a science of people management is going to be just.

The following summaries roughly approximate the path of the remaining chapters:

Chapter Two provides a detailed account of inquiry from a pragmatist perspective. Drawing particularly from John Dewey, I analyze scientific practice as primarily an activity of problem-solving rather than as an activity of representing reality. Pragmatists' account of inquiry as problem-solving traces the continuity between pre-linguistic interactions between an organism and its environment to the rich, deliberate interaction characteristic of human inquiry. Although pragmatists insist that this continuity lacks pronounced gaps or dichotomies, they do acknowledge that the use of language in problem-solving deserves careful attention. I contend that pragmatist philosophers of science have so far failed to provide a clear picture of how inquiry is normatively structured through language, and hence it remains difficult to appreciate how scientific descriptions participate in our (as organisms) continual reconstruction of our environment. This lack of clarity is somewhat surprising given Dewey's theory of inquiry that locates "warranted assertions" as the goal and product of scientific inquiry. On the pragmatist picture, scientists construct warranted assertions rather than discover truths. What this amounts

to is a recognition, alongside many non-pragmatists, that scientists necessarily use language and that this language, when used responsibly in accord with ordinary usage, will issue in judgments significant for day-to-day practice.

Chapter Three considers pragmatist philosophy of social science to guard against a particular type of confusion: pragmatists cannot maintain an ontological distinction between the natural sciences and social sciences. I show this ontological distinction to damage the otherwise promising account offered by Daniel Bromley's volitional pragmatism. I argue that Bromley takes the wrong side in a debate between pragmatists Richard Rorty and Keith Topper, and that a careful review of this debate shows Rorty to be appropriately cautious about overreaching the warrant of inquiry. The trouble with accounts such as Bromley's and Topper's is that they grant too much to the will, and effectively endorse a picture of human behavior that would wrest the social from social-ecological systems. Thus they echo the third problematic tendency of Norton's environmental pragmatism, which emphasizes the intentional character of human action but neglects the many ways in which human action is habituated. Rather than think of human beings as always the sorts of beings that act intentionally, I argue that the social sciences provide descriptions in terms of norms and institutions in order to intervene at the level of human practice. Because our way of life is already organized according to a plurality of values, we need descriptions of practices, not mere behaviors, in order to recognize what is at stake in reconstructing these practices.

Chapter Four considers, in much greater depth, how social-ecological systems scientists have sought to integrate diverse social sciences. Some scientists recommend particular social theories, such as political ecology, that exhaustively cover what we know about human agency. Other scientists sort social theories according to how they fit the social context under

investigation, and recommend choosing the theory that is most predictive, and thus understood to best represent the social reality. I follow a third proposal, which distinguishes among social theories on the basis of normative and evaluative assumptions they make about who we are and who we want to become. I then consider a specific meta-study of coastal and marine social-ecological systems to illustrate the ways that particular social scientific disciplines understand situations as problems and understand mechanisms as ameliorative. In order to evaluate whether these disciplines rightly problematize these situations, we must bridge social scientific vocabularies with moral vocabularies. In the case of the social sciences, it will be vital to bridge scientific vocabularies with deontological and virtue-theoretic traditions in ethics, which represents a departure from the typically instrumentalist leaning of environmental pragmatism.

Chapter Five considers the challenge of integrating various social sciences and biophysical sciences in social-ecological systems science. I take two senses of integration to be relevant to this task: the integration of facts and values, and the integration of disciplinary understandings through interdisciplinary collaboration. After reviewing the ways that facts and values are integrated in all disciplines (not just the social sciences as explored in Chapters Two and Three), I diagnose philosophical confusions central to the interdisciplinarity literature that maintain integration to be an epistemological or ontological project. Instead, I advance an account of interdisciplinary integration that foregrounds the ethical nature of prioritizing among, and weighing tradeoffs between, candidate disciplinary descriptions of complex systems. I recruit Philip Kitcher's ideal of well-ordered science as a starting point for thinking about well-ordered interdisciplinary science. Well-ordered interdisciplinary science works outward from the truths deemed most significant to stakeholders, abiding by a procedure that integrates the insights of diverse disciplines. The integrated understanding of a social-ecological system is the

understanding that helps us to navigate tradeoffs between the plural values at stake in a community's interaction with its environment.

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CHAPTER 2

WARRANTING ASSERTIONS

Abstract: This chapter details a pragmatist theory of inquiry with the specific goal of demonstrating how science is value-laden. It opens with some prefatory remarks about naturalistic metaphysics that respond to the charge from pragmatists and non-pragmatists alike that a naturalistic metaphysics amounts to a contradiction. I argue that pragmatists can offer an account of experience grounded in organism-environment interaction provided that they do not treat this account as a scientific theory that provides the one true picture of the world. I then draw on Dewey in offering a pragmatist account of experience grounded in organism-environment interaction characterized by three levels of organization: a basic level comprising a system of tensions directed toward maintaining a dynamic equilibrium, a second level of psycho-physical organization or “feeling,” and a third level of “mindedness.” For Dewey, mind emerges when social organisms communicate and forge a language, through which they indicate their plans and expectations, and by which they mark off objects in the world. Locating the normative role of language in representing the world as a field of objects is crucial to appreciating the ineliminability of pragmatics, and pragmatic values, in scientific inquiry. By his later years, Dewey transitioned from a theory of inquiry characterized by doubt-inquiry-belief to one characterized by indeterminate situations-inquiry-warranted assertion, and it is this later theory of inquiry that can contribute to contemporary discussions of values in science. I close by focusing on the specific normative structure of inquiry into causes, or mechanistic explanation, to open up social-ecological systems science to necessarily pragmatic discussions of means and ends.

Naturalistic Metaphysics as a Genre

In his review of John Dewey’s *Experience and Nature*, George Santayana remarked that Dewey’s attempt to naturalize metaphysics seemed contradictory (1925). Naturalism can mean many things—indeed much of Santayana’s review disputes whether he or Dewey staked a rightful claim to the term—but naturalism in all of its meanings makes a living within experience. The entities and processes to which naturalistic philosophies appeal must be located in the activities of worldly beings. Entities and processes that claim to exist beyond these activities, that cannot be experienced, are eschewed from naturalism; they belong to the domain

of the supernatural. It is in that domain that traditional metaphysicians built their philosophical systems, drawing resources from realms of pure forms, sensible ideas, and world spirits. The trouble is that philosophers had long posed questions that set metaphysics against naturalism, while at the same time demanding a metaphysics for naturalism. Naturalists had to admit that their knowledge of the world was gained through experience, and yet any explanation of how this “experience” worked must either appeal to worldly knowledge (and thus beg the question) or appeal to metaphysics. It is in Dewey’s attempt to avoid this dilemma that Santayana suspected a contradiction. Near the end of his life, Dewey himself admitted regret for the title *Experience and Nature*, confessing that the term “experience” was beyond recuperation as a naturalistic notion, and suggesting that *Culture and Nature* might have better avoided misunderstandings.

Subsequent critics have issued similar complaints against Dewey’s naturalistic metaphysics. Richard Rorty reads *Experience and Nature* as trying to bridge the gap between our understanding of how organisms navigate their environments and our understanding of how human beings engage in rational discourse about these environments (1982). This gap separated the empiricist philosophies traced to Locke and the idealist philosophies traced to Kant, Hegel, and T.H. Green, while it also separated the emerging scientific fields of psychology and sociology (1982, 80-81). It is a gap that cradles many of the most trenchant philosophical problems, problems originating in dualisms between body and mind, object and subject, world and word. In his metaphysical writings, Dewey appealed to Darwin’s theory of evolution as a blueprint for bridging these dualisms. Evolutionary theory allows philosophers to recognize the continuity between our sensory engagement with the environment and our ability to deliberate about (and thereby act deliberately within) that environment. Rorty points out that this is exactly

the sort of bridge that the earliest empiricists sought in order to connect ideas with the causal antecedents of these ideas, which they dubbed sense impressions.

The trouble, Rorty maintains, is that this psychological account tells us absolutely nothing about how we go about justifying our ideas, which for Rorty is a thoroughly social affair. We should look to Dewey, according to Rorty, in those moments where Dewey dissolves philosophical problems by chronicling the historical conditions that gave way to now-outdated philosophical commitments. Look to the way that metaphysical systems undergirded social and political prejudices, for metaphysics is less the product of inquiry as it is a directing bias for inquiry (Hook, 1939). Indeed, the chief impediment to the sort of open-minded inquiry that Dewey sought was, according to Rorty, how distinctions such as mind and body have come to seem so natural to interlocutors. For instance, talk of “sense impressions” has grown popular enough that we can unobjectionably appeal to them in our discussions of our knowledge practices, yet no philosopher nor scientist has produced an account of sense impressions that does all of the work required of these entities in empiricist theories of knowledge. Clearing away such philosophical obstacles requires less, not more, naturalism; it requires philosophical writing that demonstrates the contingency of our epistemological and metaphysical commitments.

More recently, Isaac Levi has advanced a “wayward naturalism” with the intended goal of “saving Dewey from himself” (2012). Here, the project of tracing continuities between thought and action falls to the wayside. In the absence of “bridge laws” that connect terms like belief, desire, and value to bodily and linguistic behavior, Levi recommends a retreat from the Deweyan picture of inquiry (113). As I detail in Section 2, inquiry for Dewey is a matter of warranting assertions that guide us as situated organisms toward the reconstruction of our habits and our environment. Yet a naturalist who relies on inquiry to give an account of inquiry is,

according to Levi, dismayed by the lack of empirical evidence connecting thought and action. At best, naturalists can model doxastic categories, such as belief, as dispositions to linguistic action—for example, we might reply to a query about one’s name by uttering one’s name. Naturalism keeps its promise to appeal only to relationships found within experience, but breaks its promise of dispensing with dualisms (here word/world) that hark back to supernaturalism. On Levi’s account, inquiry ends by transforming an organism’s “commitments to points of view,” but does not extend all the way to the action of organisms embedded in an environment (116). Given pragmatists’ insistence that inquiry is only intelligible when understood as exactly that sort of organism-environment interaction, Levi’s position more resembles the idealisms that Dewey resisted than the pragmatism that he endorsed. If the pragmatists’ account of inquiry terminates in dispositions for linguistic action, then pragmatism has abandoned its characteristic commitment to the embeddedness of language and meaning in social practice. Language, as a system of meaning, only makes sense as a way of coordinating action within a community. Drop the way that thought guides our material practices, and we are left with linguistic idealism.

I offer these critiques of Dewey as a preface to a chapter that borrows heavily from Dewey’s theory of inquiry. The two critiques mark a transition from the naturalistic pragmatism of Dewey, through the linguistic pragmatism of Rorty, to the threat of linguistic idealism present in Levi. Responding to the threat of linguistic idealism requires exactly the sort of bridge terms, or continuity between the psychological and social, that Dewey attempted to motivate. While I agree with Rorty that the social cannot be reduced to the psychological, I believe that *Experience and Nature* can be read without reductionist ambitions. On that reading, chapters like “Nature, Life, and Body-Mind” should be read in the same spirit as intellectual history, and it is when writing in the spirit of intellectual history that Rorty finds Dewey at his best. There is a simple

reason for this reading: if Dewey believes that organisms' actions, including their linguistic actions, ought to be understood as tools for navigating their environment, then no description of the organism can be understood as disclosing its essential nature (a hollow concept for Dewey). Pragmatists like Dewey cannot be simple realists about their reasons for constructivism. Rorty wants to read the scientific passages of Dewey's corpus as part of a genre of philosophical writing, initiated by Locke, that appeal to scientific findings to try to get to the bottom of understanding. *That* genre, as Rorty takes pains to detail, is guilty of the charge of conflating conceptual and psychological questions.

But there is another way to read Dewey's naturalism—as natural history akin to intellectual history. Rorty and fellow linguistic pragmatists are more comfortable with dissolving philosophical dualisms through narratives of the social and historical contexts within which they arose. Their preferred narratives deflate realisms, foundationalisms, and essentialisms while deriving their force from exactly the philosophical positions that they critique (Janack 2012). Offering an alternative history of the ideas (ideas such as Cartesianism) works because we take those alternative histories to track the true origins of those ideas. We have to read these intellectual histories in an “ironic” spirit—where irony is the posture Rorty recommends for recognizing the contingency of our beliefs while still being moved by them in the same way that we are compelled by realist, foundationalist, and essentialist commitments (Rorty 1989). Reading them literally would succumb to the same inconsistencies that the histories are meant to highlight; we can no more be simple realists about history than we can about biology.

It is in that ironic spirit that pragmatists can offer biological and psychological accounts of the nature of experience. This requires that we read *Experience and Nature*, as well as the naturalistic metaphysics that show up in Dewey's later work such as *Logic: The Theory of*

Inquiry, as imagining an alternative natural history of humans as organisms. This alternative natural history is not a *theory* such that it could turn out to be true or false (that is what Levi requires of it); rather, it is a different story about the human organism without which we may remain captivated by the story offered by empiricism. Dewey's metaphysical writings spend more time contrasting the pragmatist picture of organism-environmental interaction with the empiricist picture of sensory impressions than they spend marshalling evidence to attest to the superiority of the pragmatist theory. Instead, as natural history, they ask us to *think like this*, and then to reflect on whether thinking thusly would be beneficial in the long-run. They concede Sydney Hook's point that all metaphysics is a directing bias for inquiry, but they maintain that, as a directing bias, their consequences are preferable to those of other metaphysical systems.

What I find desirable about Dewey's naturalistic metaphysics is that, given its account of the logic of inquiry, it cannot lose sight of the role that values play in inquiry. Fundamentally, pragmatism's theory of inquiry is a logic for ameliorating problematic situations, and situations are problematic insofar as they jeopardize what we value. Here I offer an account of organism-environment interaction to ground a theory of inquiry that is value-laden through and through. I argue, however, that contemporary pragmatists have overemphasized the biological matrix of inquiry while underemphasizing the cultural matrix. The contrast between these two emphases can be brought out by explicating Dewey's notion of "warranting assertions" in juxtaposition to the Piercean notion of "fixing belief." After providing a general account of how values play a role in warranting assertions, I consider the specific case of warranting assertions about causation, which Dewey (1938, 454) declares "the *ultimate* objective of any existential inquiry". The goal here is to clarify many of the philosophical confusions that plague our commonsense understanding of causation; by clearing away these confusions, the role that values play in

inquiry into causes is undeniable. As shown in Chapter 1, social-ecological systems scientists aim for a mechanistic account of social practices while ignoring the ethical dimension of this account; Chapter 2 will show that this objective inescapably demands value judgments, and therefore that ethical dimensions cannot be ignored.

Organism-Environment Interaction

Dewey's theory of inquiry, which anchors most if not all of his philosophy, is grounded in his account of organism-environment interaction. This grounding can be productively likened to a naturalization of Hegelian metaphysics. Dewey's contribution to that tradition was to articulate the continuity between a Hegelian picture of the development of consciousness with a Darwinian picture of organism-environment interaction. In "The Influence of Darwin on Philosophy" (1910), Dewey traces how pre-Darwinian biologies suggest that existence echoed essence and that the task of the philosopher was to decipher the essences, and thus the absolute truth, that stands behind appearance. While most scientists had embraced the new picture of biology, philosophers were yet to recognize the flawed biological underpinnings of their quest. Such philosophers still assumed the account of organisms implicit to modern thought, where a passive observer forms ideas by synthesizing sense impressions into ideas. Dewey found in Darwin an antidote to the dualisms given rise by this traditional account. A more Darwinian account of organisms didn't so much solve these dualisms, but dissolve them, as the new account avoided the problems from the start.

Many of Dewey's writings, from treatises on education (1916) to epistemology (1938), open with his account of organism-environment interaction. In *Experience and Nature* (1925), Dewey distinguishes three levels of organism-environment interaction; these levels range from simple to complex, where complex levels amount to a dynamic reorganization of the capabilities

already present in simpler levels. The simplest level of organism-environment interaction depicts organisms as a system of tensions through which an organism sustains its continued existence (Dewey 1925, 253). This system of tensions serves to establish a dynamic equilibrium between the life-functions of an organism and an environment as it bears on these life-functions (1938, 40). Dewey cautions that we should avoid wresting the organism from the environment within which it is embedded and only within which its activities are meaningful. An organism “does not live *in* an environment; it lives by means of an environment” (1938, 32). Living is the activity of drawing energy from the environment in order to preserve the unstable equilibrium characteristic of a particular organism. This picture of organism-environment interaction is not a biological theory; it does not provide an explanation of why an organism pursues a dynamic equilibrium, but rather it seeks to clarify, conceptually, what we mean when we talk about living beings. Dewey notes that no special telos distinguishes the inanimate iron molecule from a living organism (1925, 254). The living organism responds to interruptions in “a temporal pattern of activity” through internal and external changes that tend to perpetuate that pattern. The iron molecule simply does not—there is no “why” in either case, and if the iron molecule happened to have the sort of organization that did perpetuate a particular pattern of activity, “it would have the marks of a living body, and would be called an organism” (254).

Dewey distinguishes this very simple level of organization from two more complex levels: the psycho-physical and the mental. Though Dewey generally sticks to the term “psycho-physical,” likely in order to emphasize the embodied quality of psychology, he sometimes refers to the psycho-physical level as “feeling” (1925). The psycho-physical helps to distinguish between the more constrained life-functions of plants from the life-functions of animals that extend outward through the environment. Organisms with certain biological capacities—in

traditional parlance, sense organs—render themselves sensitive to qualities now characteristic of their environment.⁶ Each “new organ provides a new way of interacting in which things in the world that were previously indifferent enter into life-function” (32). Notice that Dewey does not say “in which things in the world *to which the organism was* previously indifferent;” interaction goes both ways. In response to sights and sounds and smells, the organism undergoes changes to continually reestablish a dynamic equilibrium between its organization and the environment.

It’s important philosophically that Dewey does not treat perception (regardless of sense organ) as yet representing the world. As he would say, experience at this level is *felt*, but it is not “had”; what we mean by “having an experience” requires capabilities characteristic of the more complex level of mind. Psycho-physical organization is simply the means by which organisms anticipate, adapt, and respond to an extended environment. We might say (though subsequent remarks about “causes” will discourage such phrasing) that perception is a way of having one’s activities caused by the environment where, in the absence of perception, the objects of this environment were unrelated.⁷ Pragmatism’s naturalistic remarks are existential, not epistemic, and Dewey does not commit the error that Rorty suspects and that empiricists accused him of committing of conflating psychological and conceptual issues. But Dewey is adamant that any objective account of inquiry must ground conceptual practices in organism-environment interaction (1938, 40). This means that inquiry, usually understood “by terms such as doubt,

⁶ Because Dewey restricts “environment” to the world as it enters into the life-functions of an organism, it would be misleading to refer to these qualities as pre-existing within the environment—they *become* characteristic of their environment once they enter into the life-functions of the organism. Dewey is not teetering toward idealism here—he acknowledges that there is of course a world external to the organism and its environment—but he is cautious about treating qualities as “existing” independently of organism-environment interaction. This is a *conceptual* remark about qualities—that they obtain relative to the life-functions of an organism—not an ontological remark about nature.

⁷ The use of “cause” here accords with Searle’s (2015) account of perception. I don’t deny that there is an ordinary way to discuss causality where we can speak straightforwardly about causes. The phrasing that I discourage in the final section of this chapter is the projection of causality from this phenomenological context to a scientific context. When we talk about causality in a scientific context, we are doing something different than describing the world as observed by inquirers.

belief, idea, conception,” ultimately refers back to a reconstruction of organism-environment interaction. It is through this interaction that an organism establishes a dynamic equilibrium with its environment.

One clarification is in order. For pragmatists such as Pierce inclined to give a semiotic account of experience, the psycho-physical level of organization brings with it the emergence of signs. On this semiotic account, something becomes a sign of something else when the former comes to elicit the same response as the latter; smoke is a sign of fire when the organism responds to smoke in the same way as it would respond to the immediate perception of fire. Among classical pragmatists like Pierce, James, and Dewey, even psycho-physically complex organisms exist in a semiotically rich environment. Such complex organisms live in a world shot-through with signs, responding to the scent of prey as prey itself, or the calls of a potential mate as the mate itself. In contrast to later, linguistically oriented philosophers, classical pragmatists referred to these associations as the meaning of a sign, where the meaning of a sign amounted to the difference it made in the actions of an organism. The trick in all this is to demarcate sign from symbol, since ordinary language does not neatly distinguish the two. Pragmatists contrast the two terms by thinking of signs as a conduit for responsiveness and symbols as a vehicle for reasoning; a sign brings about a difference in the actions of an organism, but a symbol can serve as a reason for the actions of an organism. At the level of psycho-physical organization, Dewey denies that organisms use or encounter symbols. Though psycho-physically organized organisms live a meaningful life in the sense that signs allow them to anticipate and dynamically respond to their environments, they are yet to acquire meaning in the sense (prevalent among philosophers) that one experience “stands for” or “represents” another.

Symbols emerge for Dewey in the third level organization, mindedness. Here Dewey must excavate the rubble of earlier epistemological theories that suggest that mental, or conceptual, relationships are relationships between a word or idea and the ready-made objects in the world to which it refers. Such theories invest in the psycho-physical level of organization two metaphysical realms; first, there is the physical level, comprised of ready-made objects, and second, there is the psychic level, often portrayed as a sort of theater inside of our heads where we represent these objects to ourselves. This investment gives rise to a host of philosophical questions, but its cardinal sin for a naturalist metaphysician is that it seems to require a rupture in the evolutionary development of human organisms. Non-human organisms make sense by responding to changes in their environment through changes in their own constitution; non-human animals establish psycho-physical relationships through which they anticipate and adapt to environmental conditions through changes in their behaviors. Yet the prevailing non-naturalist metaphysics has a tough time understanding the evolutionary origins of the mind without positing the surreptitious appearance of a whole new faculty.

Dewey traces a continuity between the psycho-physical capabilities of organisms to the mental by attending to the social practices of social organisms. The key here is the development of linguistic, or more broadly, communicative practices; he writes:

As life is a character of events in a peculiar condition of organization, and ‘feeling’ is a quality of life-forms marked by complexly and mobile discriminating responses, so ‘mind’ is an added property assumed by a feeling creature, when it reaches that organized interaction with other living creatures which is language, communication. Then the qualities of feeling become significant of objective differences in external things and of episodes past and to come. (258)

Language or communication is key because it marks the deliberate deployment of signs to coordinate “organized interaction”; Dewey stresses this deliberate quality by noting that “differences in qualities (feelings) of acts when employed as indicators of acts performed and to be performed and as signs of their consequences, *mean* something (258). Notice the relationship between “qualities” or “feelings” in this passage and the passage previously cited. In this passage, the quality of the act serves as an indicator of how the organism responds to the world and the consequences of that response; in the previous passage, the quality of the act becomes significant of *objective* differences in external things and of episodes past and to come. Indeed, Dewey is consistent throughout his work that the objectivity of the world is constructed according to our abilities to interact with the world and how the world behaves in these interactions (here he shares much with phenomenologists). Objects simply *are* the totality of *our* possible interactions with them—they do not stand ready-made, independent of us, awaiting representation.

For signs to serve as indicators of acts performed and to be performed, or to be a medium for communication at all, both parties to the communication must attach similar significance to differences in the qualities of sounds or gestures. In other words, social organisms must use sounds or gestures “normally,” or in keeping with the norms that surround their use within their community. If a social organism attempts to signal its plans, or signal the likely consequences of its or another’s interactions with the world, through sounds and gesture that lack significance to its “interlocutor,” that sign will fail to produce the intended differences in the interlocutors actions. Dewey pays close attention to how human beings are socialized into a community that uses sounds and gestures to communicate—render common—an understanding of an objective situation. When sounds and gestures, or signs in the semiotic sense, are shared and significant of

objective differences, then these signs have reached the level of symbols, and the community shares a language.

A “just-so story,” one which sketches a plausible evolutionary account of the emergence of language or communication, is helpful here. Organisms who happen to live together become sensitive (in the psycho-physical sense) to the movements of one another. When a member of the social group inadvertently yips at the sight of danger, others (at least the more fortunate) come to attach the same significance to this yip as they’d learned to attach to those sights themselves. In a very rudimentary way, the sound of the yip becomes a *sign* for danger and gives rise to the same anticipation and adaptation that a direct encounter with danger would. Note that at such a rudimentary level, “yip” hardly stands for anything in the world. Pragmatist just-so stories speculate on the refinement of these psycho-physical capabilities; “yipping” in the face of danger gives way to “yopping” for terrestrial threats and “yapping” for aerial threats (see Kitcher 2011 for a paradigmatic example of a pragmatist just-so story, and Schel et al 2013 for an empirical study corroborating these sorts of stories). The community responds to “yopping” much differently than they would respond to “yapping,” and when we talk about the meaning of “yap” or “yop” we are describing differences in organism-environmental interaction. For all this yapping to amount to a language, organisms must learn to yap under the same conditions as one another, and to actually *use* their yapping to indicate their plans and expectations. Yapping serves as a symbol when it conveys these plans and expectations, and thus represents an object understood as a regularity in our interaction with the world.

The eventual richness of our symbolic practices is impossible to convey through these sorts of just-so stories—most of our symbolic practices today are enmeshed in syntactically and semantically complex linguistic practices—but the basic machinery of such stories can help to

dissolve our pre-Darwinian intuitions about a nature with essences. The symbol “snake” picks out a particular feature of a situation by connecting—immanently, through material practices such as sounds and gestures—that feature with the social practices that have developed in response to snakes. Still, no single symbol captures the complexity of these practices. Settling on a robust description such as “this is a snake that probably will not bite our dog and could help with the infestation of crickets in the garden” involves such a suite of symbols that a complete analysis of their meaning would take us through every alternative symbol countenanced in its construction. This is a far cry from tracing the meaning of “yops” and “yaps.”

On the earlier epistemological account, finding the right word was a matter of settling on the symbol that accurately represented the way the world is. This account then struggled to produce theories of how we could come to know the essences of objects that justified our referring to them the way that we do. Dewey finds in Darwin a solvent for this essentialism—there are no lingering *philosophical* puzzles regarding about how organisms develop social practices that differentiate objects in the world (though empirical questions abound). Being socialized into a community simply involves learning the words that communicate the exigencies of a situation. Dewey writes that, “To represent things as they are is to represent them in ways that tend to maintain a common understanding; to misrepresent them is to injure—whether willfully or no—the conditions of common understanding” (1911, 103). Uttering a sound that has no common meaning—for instance, “yop”—is meaningless. It is only within a community that sounds become symbols and that symbols have meaning for how we interact with one another and the world. To work, these interactions must abide by the community’s norms. Here the naturalist metaphysician must deny to Nature the normative role of determining the meaning of discourse. For Dewey, “either there is no social way in which it is fitting to conceive and state

to others and hence to oneself objects, and then the matter is wholly outside the sphere of truth and falsity, or the objects have a social status and office, which are authoritative for all statements about them” (1911, 105). Truthfully describing a situation has to do with abiding by the norms of the community. Though there are no norms governing the use or response of the sound “yop,” there exists a whole host of norms governing the use and response to each word in our robust description, from “infestation” and “garden” to “probably,” “not,” and “is” (the norms governing these latter words are the primary subject of Dewey’s *Logic*). It is according to these norms that an inquirer justifies their description of a situation, or warrants an assertion.

Some summary is in order. Throughout his career, Dewey developed an account of the nature of inquiry that serves to defuse many of the founding intuitions of traditional empiricism. His naturalistic metaphysics articulates the continuity between human cognitive practices and the life-sustaining practices of other organisms. Dewey traces this continuity across three levels of increasingly complex organization. Being an organism means being engaged in transactions with an environment that function to establish dynamic equilibria. Animals in particular are marked by sensory organs by which they establish more complex patterns of action in response to psycho-physical transactions. Experience for psycho-physically organized creatures is felt. When organisms come to live in social groups, they become psycho-physically responsive to one another’s activities. Sounds and gestures enter into situations as symbols that are meaningful for the conduct of the social community. The meaning of any particular symbol is a matter of communal norms that govern the appropriate use of and response to that symbol. Sophisticated languages coordinate a community’s conduct, and settling on the “right” description is a matter of abiding by communal norms. To warrant an assertion is to demonstrate that one is using the right symbols as governed by shared social practices and the individual situation.

Fixing Belief Versus Warranting Assertability

By Dewey's later works, he had come to appreciate the thoroughly social character of objective scientific inquiry. This appreciation is evident in his transition from a Peircean theory of inquiry comprised of doubt-inquiry-belief to his own theory of inquiry comprised of indeterminate situation-inquiry-warranted assertion. Whereas Peirce's theory of inquiry is available to the individual inquirer, the main concepts of Dewey's theory—indeterminate situation, inquiry, and warranted assertion—require a social context in which the individual inquirer is situated. In this section I discuss recent philosophical debates between pragmatists and non-pragmatists on the role of values in science. My main aspiration is to highlight the social context of Deweyan inquiry, which provides inquiry its normative structure; without attending to this normative structure, pragmatists are hard-pressed to contribute to a pluralist engagement with values in science. With this normative structure in plain sight, pragmatism can make significant contributions to the values in science debate, particularly with respect to the normativity of problem formulation.

Much of the values in science debate concerns the sense in which science is laden with values, and pragmatists can be too quick to assert an ontological basis for the value-ladenness of inquiry. At the close of his essay, "John Dewey's Logic of Science," Matthew J. Brown concludes that:

It is because all inquiry has to do with transforming a problematic, indeterminate situation into a unified one, which requires an active modification of one's circumstances, that all judgment is (in part) a judgment of practice because the 'primary object of our attempts to understand the world is not to describe it but to *manage* it' (Welchman 2002, 39). A problematic situation is, as we have said, a disruption of some practice that matters; resolving it requires a judgment about how to reconstitute the practice (2009, 301).

From this characterization of inquiry, it follows that “there is no phase of inquiry in which values play no role” (2009, 301). Values inflect the language used in problematizing the situation, which bestows on inquiry “a specific normative structure governed by values” (301). Reaching a judgment about the facts of a situation puts into action a way of describing that situation, and the purchase of that description is its significance for how we interact with one another and the environment. Inquiry simply *is* a matter of reconstructing our practices when those practices fail us. Descriptions of situations are not merely mental representations of an objective world; they signify our ways of interacting with the world, a significance that is first and foremost pragmatic. On this pragmatist account, there is no coherent sense in which the scientist can first describe a situation in value-free terms so that the policy-maker can respond to the situation in a value-neutral manner.

What is lacking in Brown’s pragmatist account of inquiry is a specification of the normative structure of inquiry, one which shows how (and not simply that) every phase of inquiry is governed by values. Such a specification is exactly what Dewey articulated in his *Logic*, which is principally concerned with the structure (for Dewey, the form) of inquiry. Inquiry is structured according to functional distinctions made within experience; for example, certain experiences count “as evidence” in light of the role that those experiences are taken to play in the reconstruction of organism-environmental interaction. Much of *Logic* is dedicated to a functional account of how propositions of particular forms provide structure to how we should interact with the environment. Propositions that state particulars coordinate these interactions differently than propositions regarding generalizations or universalizations; propositions that state disjunctions coordinate interactions differently than those stating conditionals. The project of *Logic* is to avoid several philosophical puzzles by demonstrating that these logical roles are

grounded in the ontology of organism-environment interaction. Logical forms are not the shadows of Platonic forms; they are enacted through the practices of a community, and they are themselves the product of prior inquiry.

Pragmatists may be guilty of moving too quickly from a Deweyan account of the organism to a declaration of the value-ladenness of all inquiry. Unless elaborated as a logical, rather than ontological, theory, pragmatism's account of organism-environment interaction is not obviously significant for today's philosophy of science. By articulating the functional role of propositional forms, Dewey places his pragmatist account of inquiry into conversation with the logical empiricists who were prominent in his day and that still shape the contemporary discussion of science and values. Offering a *logic* that details a specific normative structure of inquiry promises to avoid conflating conceptual with psychological issues. In order for Dewey to attend to conceptual issues, however, he had to transition from a pragmatism of fixing belief to a pragmatism of warranting assertions. The practice of warranting assertions is the practice of giving and sharing reasons within a community, and it is these practices that provide normative structure. They provide structure because "the genius of language, reflecting a vast network of social traditions and purposes, enters quite as much as the thing told about in deciding whether what is told is a truthful representation" (1911, 103). The conceptual, as Dewey takes pains to demonstrate, is fundamentally a matter of abiding by community norms; he joked that "to observe a thing rightly is perforce to observe social prescription; that 'observe' is used in both senses may be a poor philosophic pun but is nevertheless a description of a basic fact" (1911, 105). We need both senses, sensing the world at the psycho-physical level and "making sense" at the mental level characterized by shared social norms, in order to observe a thing rightly.

That said, placing Dewey's theory of inquiry into conversation with contemporary discussions of values in science requires a broader view of inquiry than these discussions usually countenance. Pragmatists mobilize a model of inquiry that is functionally divided into multiple phases (see Brown 2012 regarding disagreement over the precise number of phases). Following Thompson (2015), we might functionally divide inquiry into five phases (though I use somewhat different labels for the phases, I draw the same distinctions). First, inquiry is initiated by a disturbance in the dynamic equilibrium established between an organism and its environment. This disturbance establishes the formerly determinate situation as indeterminate, which sets into motion inquiry. Inquiry must first find orientation within the indeterminate situation, and this second phase of inquiry establishes the formerly indeterminate situation as a problematic situation. In light of the way that the situation is taken to be problematic, the inquirer proposes conduct that might respond to the problem and re-establish unity. This third phase is approximately the articulation of hypotheses, which are then evaluated in the fourth phase. Once proposals are evaluated, the inquirer settles on the most promising proposal and, in this fifth phase, engages in conduct that follows from that proposal. The success of this conduct is in no way guaranteed by the positive evaluation adjudicated in the fourth phase. This pattern of inquiry can be enumerated as (1) disturbance, characteristic of an indeterminate situation, (2) orientation, characteristic of a problematic situation, (3) proposal of conduct, (4) evaluation of proposals, and (5) action, which if successful re-establishes a determinate and unified situation.

The entire arc of inquiry is initiated in response to a disturbance in the activities of organisms, and activities, properly understood in conceptual contrast with mere movements, are purposive. The purposes that motivate activities are the values of an organism (again, a conceptual point, not a causal claim about motivation), and so inquiry is necessarily value-

laden.⁸ But pragmatists have so far struggled to clearly specify the role of values in the normative structure of inquiry, which impedes conversation between pragmatists and non-pragmatists. Harold Kincaid argues that many traditions in the philosophy of science grant that values play a role in inquiry, and that this sanctions further questions: “What kinds of values are involved—moral, political, epistemic? How are they involved—inevitably or contingently?—In what part of science—questions asked, confirmation, explanation—are they involved? And what does this tell us about science?” (2007, 219). The task for pragmatists is to make a critical contribution to this conversation, but that requires quite a bit of reconstruction to fit pragmatists and non-pragmatists into a dialectical relationship.

The difficulty of bringing pragmatist accounts of inquiry into conversation with non-pragmatist accounts is, in part, due to the more narrow focus of many non-pragmatist accounts. The specter haunting contemporary accounts of inquiry is the threat of relativisms and anti-realisms that locate their point of departure in Kuhn (himself neither a relativist nor anti-realist) and that locate the central philosophical challenge in the determination (or underdetermination) of theory by evidence (Brown 2013a). For certain concatenations of empiricism, underdetermination is *the* crisis that threatens the rationality of scientific inquiry and societies that adjudicate competing claims through scientific inquiry (Janack 2012). Meanwhile, non-pragmatists have long granted that values play a role in questions asked, and (for the most part) non-pragmatists appear willing to concede the value-ladenness of any phase of inquiry that doesn’t directly deal with hypothesis formation and testing. More or less, many non-pragmatists

⁸ That this very much sounds like a causal claim is a result of misplaced philosophizing about the nature of values (Sellars 1963). Because “value” sounds like a noun, and because part of the meaning of “value” is that it governs action, philosophers have been compelled to offer an ontology of values according to which they are the cause of actions. This reflects a confusion regarding the grammar of values—“value,” like many of our mental constructs, is grammatically more naturally an adverb than a noun. Talk of values most appropriately describes *how* an action was carried out, whether it tends to produce a certain sort of world, whether the actor showed satisfaction but not surprise in that production, and so on. When we talk of values we are not talking about things in the world as much as we are talking about processes in the world.

adopt the view of inquiry held by social-ecological researchers: policymakers can bring values to bear in supporting particular research, and in deciding what to do in response to the conclusions of research, but values cannot directly interfere with the conduct of research itself.

On the pragmatist view, values have already entered the picture during the phases of inquiry of concern to non-pragmatists, but in a much more robust sense than that granted in conceding the value-ladenness of questions asked. By conceding that values play a role in motivating research, philosophers of science acknowledge that the first phase of inquiry—disruption, which marks an indeterminate situation—is guided by values. Then, by focusing on the third and fourth phases—proposal of conduct and evaluation of proposals—philosophers of science narrow their focus to those aspects of inquiry that they take to be essentially epistemic. The trouble for conversation between pragmatists and non-pragmatists is that the second phase of inquiry—orientation, wherein the situation is problematized—receives little attention. Yet this is exactly the phase of inquiry that provides normative structure to scientific practice, and it is to these norms that pragmatists attend when investigating the values implicit in inquiry.

Orientation is critical because indeterminate situations do not come ready-described. Indeterminate situations are experienced like the blooming and buzzing confusion discussed in Chapter One; they are marked merely by the disruption of our practices that maintain a dynamic equilibrium, or unity, with our environment. With William James we might stress that existence is a stream of consciousness, and that our task is one of analysis not synthesis (1890). In Chapter One, I noted that an indeterminate situation might set into motion inquiry into the ecological characteristics of that situation, and in particular whether the ecosystem should be described as resilient or fragile. Scientists inquire into the details of an individual ecosystem and exchange reasons for describing the ecosystem one way or another. Crucially, these descriptions participate

in our engagement with the world—describing the ecosystem one way will support a suite of practices different than those supported by an alternative description. This follows the pragmatists' account of language as a constellation of symbols and relations among symbols that coordinate collective action.

An important point here is that the environment alone does not determine what description is correct. Initially, inquiry is relatively unstructured, and the first task of the inquirer is to orient herself to the situation. Again, we can borrow from the first chapter and think of this orientation as deciding on a vocabulary from which any descriptions of the situation will be drawn. Concretely, this might amount to deciding whether to approach the situation with the vocabulary of a systems ecologist or the vocabulary of a population ecologist; in a truly unstructured situation, inquirers might debate the merits of a broadly ecological vocabulary or a broadly economic vocabulary. Note though that, in this phase, the merits of a vocabulary is a wholly separate matter from the merits of a description. When scientists exchange reasons for one description or another, they are commonly debating the truth of these candidate descriptions. When exchanging reasons for one vocabulary or another, truth is yet to enter the picture, and if it did, it would provide little guidance. Both the ecologist and the economist are able to offer true descriptions of the environment, so truth alone is not determining which description is right. Rather, when deliberating over the appropriate orientation, or vocabulary, for a situation, we must reflect on the pragmatics of the situation. What are our values, in all their plurality, and what practices might harmonize our pursuit of these values? There are truths that will help us in this pursuit, but they are truths about the environment *and ourselves*. The whole situation, marked by organism-environment interaction, enters into deliberation about the right orientation to an indeterminate situation. Because an indeterminate situation might warrant any number of

responses, we must narrow down the responses we might consider, and we do this by prioritizing one vocabulary over others.

For Dewey, and for Pierce, the vocabulary within which inquiry is conducted serves to orient us to the truths that we deem significant for reconstructing our practices. Pierce expresses this orienting feature by noting that inquiry as we know it is not possible without granting that many of our beliefs are beyond doubt. On the Piercean model, inquiry is a practice of fixing belief in the face of doubt. Belief here notes a disposition to act, as Pierce sets the stage for a pragmatism that roots thought in our practical engagement with the world. We cannot doubt everything all at once, in part because fixing belief involves abiding by the norms that adjudicate whether a belief is true or false. For example, one cannot fix belief about the resilience of an ecosystem without accepting beyond doubt that resilience is marked by resistance to change and ability to adapt (Walker et al. 2004). These relationships between concepts mark the vocabulary of the inquirer (in this case, the vocabulary of the systems ecologist). As a language, a vocabulary structures the inferences that we can draw from any particular judgment; these inferences are both general (if A then B, A, so B) and specific (ecosystems marked by resistance to change and ability to adapt are resilient, resilient ecosystems can support particular human practices, and so on). This web of belief connects our conceptual practices to one another and to our material practices, and it is in this “ungrounded way of acting” where thought connects us to the world (Wittgenstein 1969). On the Piercean model of fixing belief, inquiry is settling on a disposition to act in the face of a situation that calls into doubt our established patterns of conduct.

Dewey initially accepts the Piercean model of fixing beliefs, but eventually transitions to a model of warranting assertions. This transition is noted without much elaboration in Brown’s

reading of Dewey (2012, 281), as Dewey shifts from doubt-inquiry-belief to indeterminate situation-inquiry-warranted assertion. On Brown's view, this maturation in Dewey's thought mostly aims at avoiding the subjectivist or psychologistic reading of the Piercean phrasing, but I think the transition is more philosophically significant. Between *How We Think* (1910) and *Logic: The Theory of Inquiry* (1938), Dewey adopts a much more sociological perspective than in his earlier work, which was anchored in psychology. His middle works, such as *Experience and Nature*, locate intelligent conduct in the social practices of a community.⁹ When founded in psychology (e.g. "The Reflex Arc Concept in Psychology") Dewey takes the objects of experience to be constructed in the relationship between the organism and its environment. By *Experience and Nature*, the objective quality of inquiry is a thoroughly social accomplishment. Dewey notes that it is a mistake to take "the structure of discourse for the structure of things, instead of for the forms which things assume under the pressure and opportunity of social cooperation and exchange" (1925, 170-171). This is a rather cryptic way of stating what was mentioned earlier, that the environment alone does not determine what ought to be said of it. Under the pressure and opportunity of social cooperation and exchange, communities have mobilized symbols that distinguish objects in the world. To be an object in this sense is to mark an opportunity to practically direct the present situation toward valued situations in the future.

Pragmatically, vocabularies provide structure to how we reconstruct our practices in light of our particular values and the exigencies of situations that threaten these values. Deciding on a vocabulary to orient inquirers to an indeterminate situation is, in effect, privileging certain projects and the values with which those projects are aligned. It is mobilizing a set of social practices that work to restore the problematic situation to a desirable unity. Perhaps less

⁹ "Intelligent" conduct for Dewey is simply action regulated in accordance with what we've learned of the world. It is more or less used as the adjective form of "mind," as "mindful" conduct carries different connotations.

obviously, sparing some beliefs from doubt effectively spares some of our practices from reconstruction. This too involves valuation, as the decision to secure some of our practices from criticism and reconstruction is a decision about which of our practices we value and which we are willing to adapt. This type of valuation is especially salient when we inquire into the causes of a particular situation, which is discussed in the next section. But valuation informs any form of inquiry because it is only through orientation to a problem as a problem that some actions count as ameliorative. The ultimate test for how we describe a social-ecological system is whether that description helps us to coordinate action and direct that system toward a valued state.

Dewey's shift toward a more sociological perspective on meaning represents an appreciation for the role that social values play in inquiry. On the doubt-inquiry-belief model, the inquirer appears insulated from her community's purposes and projects. Pierce does not attend to the role that community plays in establishing the meaning of symbols, and his imagery suggests that an inquirer can mobilize a system of symbols to personally structure inquiry. Dewey agrees that symbols are indispensable when carrying out inquiry, but that these symbols only attain meaning within a community. This is because meaning is a product of how a community responds to a particular sound or gesture as it figures into an evolving system of symbols. Remember, "either there is no social way in which it is fitting to conceive and state to others and hence to oneself objects, and then the matter is wholly outside the sphere of truth and falsity, or the objects have a social status and office, which are authoritative for all statement about them" (1911, 105). But having social status and office is not arbitrary—objects obtain these roles because they navigate the pragmatics of individual situations and eventually become standard for navigating similar situations. Languages evolve as new uses of symbols prove pragmatically

useful, and vocabularies (as used here) function as subdivisions of languages that prove pragmatically useful for particular sorts of problems.

These remarks on the sociality of language development are meant to discourage any stark division between values, such as between “moral, political, [and] epistemic” values as queried by Kincaid. Many debates on the role of values in science have concerned whether epistemic values alone should adjudicate scientific decisions, or whether non-epistemic values can also play an important role (Douglas 2009, Steel and Whyte 2012). The trouble for pragmatists is that epistemic values do not even enter into the equation until a vocabulary has been selected, as it is only within pragmatically structured inquiry that truth and falsehood pertain. So there is no sense in which epistemic values alone could adjudicate scientific decisions, and there’s no sense in which inquiry is coherent without reference to non-epistemic values that structure it. Of course, once a structure is adopted for a given inquiry, truth and falsehood are no longer directly accountable to non-epistemic values, even if non-epistemic values played an indirect role in establishing the truth conditions of that inquiry.¹⁰ In other words, once we warrant an orientation to the problem as a problem, the reasons that we provide for that orientation no longer serve as reasons for or against our experimental conclusions. As scientists are wont to say, “Wishing it so does not make it so.” The larger point here, though, is that “wishing it so” plays a critical role in structuring inquiry in the first place. Non-epistemic values inform how we orient ourselves to a problem, what sort of unity we pursue, and what counts as ameliorative. Wishing that an ecosystem can sustain particular human practices does not make a sustainable ecosystem, but it does inform how we ought to go about describing that ecosystem in order to manage it toward sustainability.

¹⁰ See Douglas 2009 and Elliott 2013 on direct and indirect role of values, and Brown 2013b on the ambiguity of this distinction.

On a pragmatist account of inquiry, these non-epistemic values do not compromise the objective character of inquiry. Indeed, they are constitutive of it. For Dewey, the objective character of inquiry is owed to the form that objects take in the social practices of a community of organisms: “To represent things as they are is to represent them in ways that tend to maintain a common understanding” (1910, 103). Here the significance of “warranting assertability” is most clearly felt. On the doubt-inquiry-belief model, there was no procedure through which beliefs were held accountable to the common understanding of a community. Because beliefs were taken as individual dispositions to act, their symbolic expression through language could be viewed as merely incidental. To transition from “beliefs” to “assertions” is to foreground the linguistic form that beliefs take in a social community. This linguistic form demands that dispositions to act take their place in “the genius of language,” or in the system of social practices that coordinate collective action. To transition from “fixing” to “warranting” is to appreciate the social character of justification. Whether one rightfully asserts that things are thus-and-so is always accountable to whether community members would themselves make such an assertion or trust you to do so. Warranting an assertion, though, begins in the earliest phases of inquiry. We must warrant our orientation to an indeterminate situation, and warrant the vocabulary deployed in problematizing the situation. Generally, this involves deliberation about the values at stake in managing a problem and the different conduct we might propose for ameliorating that problem. The facts that are pertinent to how we should orient ourselves are as much a feature of a situation as are the facts that are pertinent when proposing and evaluating conduct. After all, *we* are parts of these situations too, as situations include both the organisms and the environments of organism-environment interaction. To assert that a situation is

problematic, or that a reconstruction of that situation is desirable, is to issue a claim that may come under public scrutiny and require warrant.

Inquiry into Causes

It is the normative structure of inquiry into causes that most concerns the project of this dissertation. In Chapter One, I noted that social-ecological systems scientists characteristically pursue “social mechanisms behind...management practices” (Berkes and Folke 1998, 3). Berkes and Folke are explicit that the proper purview of social science within the social-ecological systems approach is to locate the causes of human behavior and subsequently for social-ecological change. Like these social-ecological systems scientists, Dewey held a special place for causal explanations in his theory of inquiry:

The determination of a sequential order of changes is the goal of every scientific investigation that is occupied with *singular* phenomena. The institution of just such temporal-spatial continuities is the *ultimate* objective of any existential inquiry. When the objective is realized, there is *judgments*, as distinct from propositions as means of attaining judgment (1938, 454).

For Dewey, ordinary talk of causal mechanisms, and of explanation that cites causal mechanisms, is appropriately understood as inquiry into “sequential order of changes” or “temporal-spatial continuities.” These are not simply esoteric rephrasings of ordinary language, however, as Dewey is motivating a vocabulary that avoids some of the confusions implicit to common usage. The stakes are high, too, because for both pragmatists and social-ecological systems researchers, inquiry into these “mechanisms” is the ultimate objective of inquiry. Meeting this objective results in “judgment,” which is the determination of a course of action—perhaps issuing a warranted assertion—that concludes the fourth phase of inquiry and justifies a response to the indeterminate situation. Such judgments are critical to Dewey’s account of inquiry. Whereas “propositions as means of attaining judgment” are presupposed, and neither

true nor false, judgments *are* a matter of truth or falsehood. Successful inquiry generates true judgments that hold promise for ameliorating problematic situations.

Why is Dewey so reluctant to embrace the ordinary language of “causation” or “mechanism”? In short, Dewey takes talk of causes and forces to be an artifact of that pre-Darwinian mindset that took for granted the distinctness of objects in the world.¹¹ This artifact is owed to the aforementioned conflation of “the structure of discourse for the structure of things,” and, in its more metaphysical version, the commitment that different kinds of things share a common essence. Once things are taken to be essentially distinct from one another, the idea that some special force must relate them and explain change was a natural development. Talk of “causes” or “forces” have puzzled philosophers since the Greeks, as these occult substances do not seem available to empirical investigation. Yet this has not stopped most of us from talking of causes and of positing causal relationships as a special sort of ontological relationship.

The trouble here is that this account of causality mistakes a logical structure for an ontological insight. Dewey’s philosophical remarks here are meant to discourage the first ontological move—that objects are fundamentally distinct from one another—that demands a special relational force in the first place. It is only in light of our capacities as organisms that objects are distinguished from the flux of experience—our world assumes a particular form because we as organisms have learned to relate to it in particular ways. As social organisms, these relations have been codified through language, but the stable relationships between words and the world need not suggest that the world stands ready-made for our labels. On the

¹¹ As I go on to discuss at the opening to Chapter Three, this pre-Darwinian mindset has deep roots—following Sellars, we might say that the notion of cause is fundamental to “the framework in terms of which man came to be aware of himself as man-in-the-world. It is the framework in terms of which, to use an existentialist turn of phrase, man first encountered himself—which is, of course, when he came to be man” (1963, 6). Causes on this view are conditions for which we are responsible, which form the basis of understanding ourselves as agents. This self-awareness is then projected onto the world, where other objects are conceived as agents. The key here is that this projecting is doing something quite different from describing the world scientifically, as I elaborate throughout this section.

pragmatist account, the universe can accommodate all sorts of ontological structuring depending on how we are trying to bring a future, desirable situation into fruition. Apart from pragmatic questions of how we are trying to manage it, is not structured. If the universe is not already structured, then there is no coherent sense in which some objects are uniquely “real” and that some relationships between objects, such as causal relationships, are uniquely real.¹² Rather, existence is better imagined in keeping with the John Muir quote popular among ecologists: “When we try to pick out anything by itself, we find it hitched to everything else in the Universe” or simply, that everything is connected to everything else (1911, 211).¹³ And if everything is connected to everything else, then connectedness cannot be sufficient for distinguishing something as a cause. Dewey laments that any ontological interpretation of causation will fail to make sense of the ordinary meaning of saying one thing caused another, which cannot simply mean that one thing is connected to another, for in that case the specification of the cause will swim in a sea of similarly interconnected causes. He resolves that “the only possible conclusion upon the basis of an existential or ontological interpretation of causation is that everything in the universe is cause and effect of everything else—a conclusion that renders the category completely worthless for scientific purposes” (1938, 453).

But causation is not entirely worthless—indeed, Dewey suggests that it is the ultimate aim of any existential inquiry. Recuperating the concept requires abandoning the ontological interpretation in favor of a logical interpretation. Rather than thinking that an “event comes to us

¹² The trouble here is the notion of “uniquely,” not with “real,” though some versions of realism appear committed to the idea that there is only one true way that the world is structured (see Rorty 1999, “A world without substances or essences,” on this commitment). Pragmatism offers a realism that is opposed to the picture of carving nature at its joints. Dewey tended to grant that, of course, products of inquiry were real, but to resist the inference that these products were uniquely real or that “real” entailed a nature with joints.

¹³ The Muir passage is frequently misquoted, and it’s worth emphasizing that, in the correct quote, Muir makes note of the observer who is actively picking out the “anything.” The popular mantra implicitly suggests that there’s an “everything,” or that all that there is can be thought of as a collection of things, and this picture of an already structured existence is misleading.

labelled ‘cause’ or ‘effect’,” Dewey reminds, “An event has to be deliberately *taken* to be cause or effect” (1938, 453). Though causality is not an ontological category for Dewey, it is a vital concept to inquiry, because it provides a logical structure to ameliorating a problematic situation. *The functional role of causal explanations in inquiry is to locate means-end relationships.* When searching for causes, inquirers are analyzing antecedent events into objects that might serve as means for bringing about desirable, subsequent events. This requires at least two important distinctions within temporal-spatial continuities: some feature of the antecedent situation is distinguished as a means, and some feature of the subsequent situation is distinguished as the end or effect. While we can be said to discover these temporal-spatial continuities, we cannot be said to discover causes and effects, for an event’s status as a cause or an effect requires a pragmatic point of view. The search for causes imposes a purposive structure to inquiry. Dewey writes, “The conception of *effect* is essentially teleological; the effect is the end of be reached; the differential means to be employed constitute its *cause* when they are selected and brought into interaction with one another” (1938, 455). Later Dewey declares that any intelligible sense of causation “is practical and teleological through and through” (456).

While Chapter Four explores in detail the actual practices of social-ecological systems researchers engaged in causal explanations, a quick example here is helpful. Recall the discussion in Chapter One of the social practices of Swiss cowherders. There I suggested that the sustainability of that social-ecological system can be explained by reference to economic institutions, but that it might also be explained in various alternative keys, such as political science, anthropology, or geography. On a pragmatist theory of inquiry, we might say that everything antecedent to the rise of sustainable practices are, on an ontological or existential sense, connected to, and in this overly broad sense causal with respect to, subsequent events.

This everything could be analyzed into objects according any number of vocabularies, but the products of these analyses—for example economic features such as low dependability and high upfront capital-investment—are not suddenly more causally responsible for the continuity of events. On the ontological interpretation, this sense of causal responsibility is “worthless for scientific purposes” since everything is connected to everything else. Those antecedent events could be analyzed into the region’s political procedures, or the village’s traditions for inheriting land holdings, or merely the regular rising or setting of the sun, since all of these dimensions exist in continuity with the emergence of sustainable practices in the region. We are inclined to think that some of these continuities represent more rich and productive mechanisms for managing the social-ecological system, but these inclinations are rooted in pragmatic considerations that we mistake as ontological.

Conclusion, or Finding the Social in the Ecological

This chapter set off to contextualize pragmatists’ account of inquiry within pragmatists’ account of organism-environment interaction. The goal here was not to “reconcile” the two images, or reduce the social to the biological, but to join them together for a richer account of our social and material practices (Sellars 1963). Dewey is not guilty of conflating psychological with conceptual issues as charged by friend and foe; his ontological writings must be understood as offering an alternative account of what it means to be an inquirer if we have any hope of understanding inquiry. That alternative account centers on the notion of organism-environment interaction, whereby an organism acts to maintain a dynamic equilibrium with its environment. Humans and some non-human organisms have developed complex social practices to coordinate these actions. Language is among these social practices, and it is through language that the present situation symbolizes future possibilities that we may actively pursue. Social practices

including language are meaningful only within the ecological activities of an organism and its community (Rosenthal and Buchholz 1996).

Communities engage in inquiry in order to deliberately maintain a dynamic equilibrium with their environment. Pragmatist philosophers of science have emphasized that this ontological basis of inquiry establishes the value-ladenness of inquiry; after all, situations are only problematic, or unstable, with respect to situations deemed less problematic or more stable. So far, however, pragmatists have struggled to articulate the normative structure of inquiry and the role that values play in this structure. I suggested that part of this struggle is due to narrower scope of non-pragmatist theories of inquiry, which tend to focus on hypothesis formation and testing, and which take these phases to exhaust the epistemic character of science. Pragmatist theories recognize disruption (which establishes an indeterminate situation) and orientation (which establishes a problematic situation) as crucial phases in inquiry. In particular, orientation requires consideration of values when choosing a vocabulary that structures inquiry, and it is only within such a structure that epistemic values pertain.

Social-ecological systems science is particularly concerned with mechanistic, or causal, explanations. Rather than shelter social-ecological systems scientists from evaluative or ethical questions, this quest for a causal understanding brings values to the foreground. Although causality is commonly considered an ontological category, pragmatism details the inconsistencies of treating causality as an ontological relationship between pre-existing things in the world. Because “everything is connected to everything else,” inquiry into causes is in fact inquiry that abides by a particular logical structure of causal explanations. This logical structure is fundamentally teleological, as it requires features of antecedent situations to be taken as pragmatic means for bringing about consequent situations. This “taking” requires valuation of

both means and ends, and such valuation involves judging a situation as desirable according to the norms of one's community. Warranting an assertion about causes is, in part, warranting an assertion about the pragmatic value of both means and ends, and this opens up inquiry to the wider community of stakeholders.

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CHAPTER 3

VALUES AND THE STRUCTURE OF SOCIAL SCIENTIFIC EXPLANATIONS

Abstract: This chapter argues that we have good reasons to describe the social dimensions of social-ecological systems in terms of norms and institutions, but that these reasons are thoroughly pragmatic rather than ontological. The chapter opens with a discussion of Sellars' distinction between the manifest and scientific images of the world. Sellars' clarifies the sense that the manifest image, according to which human beings are persons who act for reasons, and the scientific image, according to which the world is comprised of deterministic relations between objects, are not rival images but rather two different ways of relating ourselves to the world. This clarification informs my critique of Daniel W. Bromley's volitional pragmatism, which insists on an ontological treatment of human action in social-ecological systems. The critique is furthered through a consideration of a debate between Richard Rorty and Keith Topper over the appropriate methodology for social science. I follow Rorty in arguing that our reasons for preferring hermeneutic methods are pragmatic rather than ontological, but I diverge from Rorty's dichotomous framing of the methodological choice. The diversity of disciplinary approaches in the social sciences reveals a range of value-laden methods for describing and explaining social practices, and our choice among these methods depends on our normative and valuational commitments.

Pragmatism and the Manifest Image of Humanity

In his 1963 Presidential Address to the American Philosophical Association, Wilfrid Sellars suggested a new way to read the history of philosophy. Sellars noticed that his peers in the association had become preoccupied with the increasingly reductive account of nature discovered in the physical sciences. As philosophers, they took their task to be the reconciliation of this picture, wherein everything reduces to inert matters, with the widespread sense that human beings do things for reasons, and when they have reasons, act on their own free will. The trouble for philosophy and for the humanities in general was that science had become so powerful that few philosophers were willing to stand behind a picture other than that given by the physical sciences (for a vivid account of this transition, see Reisch 2005). That widespread sense, which Sellars calls the "manifest image" in contrast to the "scientific image," seemed to

be an irresponsible holdout in the face of mounting evidence that reality is comprised of atoms and the void. Such a view of reality had little place for mystical forces like the human will.

Sellars regards the tension between the manifest and scientific images of humanity as a perennial problem for philosophy. This struggle can be located in how it is that we understand human beings as *persons*, beings who think, hold beliefs, build character, deliberate with one another, and act for reasons. That understanding is constitutive of the manifest image, which, Sellars chronicles, is “the framework in terms of which man came to be aware of himself as man-in-the-world. It is the framework in terms of which, to use an existentialist turn of phrase, man first encountered himself—which is, of course, when he came to be man” (6). Only by happening upon patterns of behavior that anticipate the consequences of our behavior could we have come to see ourselves as we see ourselves in the world. Being who we are, we now understand these complex behaviors as the eventual basis of self-descriptions. Movements become significant of their consequences; they become actions. The contrast between the manifest image and the scientific image is rooted here—on the scientific image, objects move, but they do not act.

Originally, Sellars reminds, humankind understood all objects as persons. Trees and forests were not mere physical entities, but fellow actors to whose movements we could attribute reasons or purposes. On such an understanding, Sellars maintains, “to say of the wind that it blew down one’s house would imply that the wind *either* decided to do so with an end in view, and might, perhaps, have been persuaded not to do it, *or* that it acted thoughtlessly (either from habit or impulse), or, perhaps, inadvertently” (12-13). It is this original image that philosophers traditionally revised through the “de-personalization” of objects other than human beings. But Sellars insists that we do not owe these revisions to discoveries about what wind really is. The manifest and scientific images are not rival images, in the sense that one can stake a claim to

being true of the really real; they are instead radically different ways of relating human beings to the world. Philosophers who struggle to defend the personhood of human beings against the further encroachment of the scientific image make a mistake when they insist that human beings are *really* persons or *not really* atoms and the void. For while this is correct, as far as things go in the manifest image, their interlocutors would be just as correct to insist that human beings are, when we get to the bottom of things, atoms and the void. What is at stake is not *what is*, but how *what is* is to be related to everything else.

The relating of everything to everything else is what Sellars takes to be the purview of philosophical reflection, in part because philosophers spend much of their time reflecting on the practices of language use, and language—as previous chapters have shown—are in the business of codifying these relations. Philosophers get into trouble, however, when they misplace concepts that are at home in either the manifest or the scientific images. These mistakes are easy enough to make but result in the pinning of the two images against one another, such that we do feel compelled to adjudicate which is truly representative of reality. Pertinent to our endeavor, Sellars echoes the point made by Dewey that the notion of “causes” is not at home in the scientific image:

...it is important not to confused between an action being *predictable* and its being *caused*. These terms are often treated as synonyms, but only confusion can arise from doing so. Thus, in the ‘original’ image, one person causes another person to do something he otherwise would not have done. But most of the things people do are not things they are *caused* to do, even if what they do is highly predictable... Thus the category of causation (as contrasted with the more inclusive category of predictability) betrays its origin in the ‘original’ image (13).

Sellars point is that the notion of “cause” only makes sense in contrast with the deliberate actions of persons. Detached from a framework where some behavior is intentional and other behavior unintentional, or indeed the intention of another person, and the category of causation ceases to hold meaning. In short, causes require because.

Philosophical confusions abound, though, when we confuse predictability (the hallmark of the scientific image) for causation (one way in which things might be predictable, but hardly the only):

...the category [causation] continued to be used, although pruned of its implications with respect to plans, purposes, and policies. The most obvious analogue at the inanimate level of causation in the original sense is one billiard ball causing another to change its course, but it is important to note that no one who distinguishes between causation and predictability would ask, ‘what *caused* the billiard ball on a smooth table to continue in a straight line?’ The distinctive trait of the scientific revolution was the conviction that all events are predictable from relevant information about the context in which they occur, not that they are all, in any ordinary sense, caused (14).

It is the confusion between predictability and causation that pervades social-ecological systems scientists’ understanding of the social sciences (Berkes and Folke 1998, Walker et al. 2002).

These researchers justify their pursuit of mechanistic explanations of human behavior according to their goals to predict, control, and adapt to environmental change. If we distinguish between causation and predictability, this goal does not justify their mechanistic orientation. What does justify the search for causes is the need to assign responsibility for environmental changes and to recommend means for bringing about different ends, and yet all of these categories (responsibility, means, ends) are alien to the scientific image.

Reconciling this confusion is no small task. The prevailing alternatives similarly conflate prediction and causation and, agreeing that causation is a misleading way to describe human beings as persons, insist that empirical investigation of human activity is bankrupt. As Charles Taylor remarks (1985),

There is a constant temptation to take natural science theory as a model for social theory: that is, to see theory as offering an account of underlying processes and mechanisms of society, and as providing the basis of a more effective planning of social life. But for all the superficial analogies, social theory can never really occupy this role. It is part of a significantly different activity (92).

In this chapter I am worried about a specific type of confusion—the idea that the social sciences and natural sciences reflect an ontological divide between the study of things (persons) who will their actions and the study of things that do not. Cede too much to this idea and the possibility of science—for Sellars the “conviction that all events are predictable from relevant information about the context in which they occur” and for social-ecological researchers the goal to predict, control, and adapt to environmental change by appeal to human practice—is seriously compromised. For social-ecological systems scientists and pragmatists alike, this compromise is damning; classical pragmatists were as dedicated to the social sciences as they were to the humanities—James in psychology (1890), Dewey in educational theory (1916), and Mead in sociology (1934). All of these figures believed that social science must eschew the sorts of dualisms that protect the manifest image from the scientific image, and that a mature social science aimed for prediction warranted by empirical description of the context of human activity. Their empiricism, however, was more radical than that imagined by Taylor and the philosophies of social science that follow in his wake (James 1912).

My reason for worrying about this specific type of confusion is that I see it as a fundamental flaw in an otherwise promising pragmatist philosophy of social science in Daniel W. Bromley's volitional pragmatism (2006). Bromley's philosophy of social science is especially pertinent to social-ecological systems science because Bromley, a practicing institutional economist, articulates a philosophy meant to grapple with epistemological issues confronting the study of economic institutions such as land use practices. Situating Bromley's argument within the context of a debate between Richard Rorty (1982) and Keith Topper (2000) on the ontological purchase of social theory, I argue that Bromley mistakenly locates the human will as an ontological feature of human nature. Such ontological commitments are misplaced because, to quote Sellars one last time, "the essential dualism in the manifest image is not between mind and body as substances, but between two radically different ways in which the human individual is related to the world" (11). To find the will "in" human action is to experience human action within the social contexts within which behavior is meaningful. This is a logical feature of explanation, not an ontological discovery.

Economic Institutions and Volitional Pragmatism

In the first chapter, we encountered two different approaches to economics. On the one hand, social ecological systems researchers deployed economics to make sense of the regulations and informal conventions by which Swiss alps cowherders managed their pastures. Here the emphasis was on the evolution and enforcement of social norms for collective action. On the other hand, Bryan Norton chronicled the interdisciplinary policy assessments of economists and ecologists. There, economists deployed cost-benefit analysis to recommend a course of action that maximized social utility. The former framework is institutional economics, and the latter framework we will call "Pareto economics" following Bromley. Although it is Pareto economics

that has been the center of debates in environmental ethics and environmental philosophy more broadly (e.g. Sagoff 1988, Daly, Cobb, and Cobb 1994), it is institutional economics that most commonly appears in social-ecological systems inquiries (e.g. Berkes, Folke, and Colding 2000, Olsson, Folke, and Berkes 2004, Ostrom, 2009).

In his *Sufficient Reason: Volitional Pragmatism and the Meaning of Economic Institutions*, Bromley defends institutional economics as the more defensible approach to policymaking generally and environmental policymaking and management in particular. While the specifics of his critique are beyond the scope of this dissertation—the problems of cost-benefit analysis and rational choice theory do not directly pertain to the problems of explanation in institutional economics—a brief review helps locate his conception of economic institutions. Rational choice theory assumes that people make choices by weighing the costs and benefits of each of their alternatives and choosing the alternative that maximizes net benefits. At the level of policymaking, this calculus is aggregated across the population of stakeholders affected by the policy. For individual action, maximizing net benefits is the definition of rationality; for collective action, maximizing net benefits for all affected is the definition of rationality. Bromley reminds that decisions can be rational only against a background of established costs and benefits accruing for particular choices. The social practices that render some actions possible and others impossible, or even unimaginable, are understood to be outside the scope of rational decision making. Laws that ban certain practices, or technologies that make new practices available for comparison, are “exogenous,” and so the existing ethical, social, legal, and technological limits are taken as given when comparing costs and benefits.

As a pragmatist, Bromley follows Dewey in taking causal explanation to be “the *ultimate* objective of any existential inquiry” (1938, 454). In light of this conviction, Bromley’s volitional

pragmatism finds Pareto economics woefully inadequate to inform social policymaking. His critique has two fronts: first, Pareto economics is circular with respect to how it explains action endogenous to a given “field of action” (33), and second, Pareto economics cannot, on its own terms, explain the development of institutions that define these fields of action. The first point is relatively simple. To say that a person chooses a course of action because it maximizes net benefits hardly counts as an explanation, in part because any choice that they make must be taken as rational. Bromley points out that, “given that much of contemporary economics employs the circular logic of revealed preference to ‘explain’ choices consistent with preferences, it should be obvious that we are unable to say anything meaningful about the substantive reality of so-called rational choice” (9). Rather, Bromley recommends, we must (at minimum) look to the institutions that define some outcomes as costs and others as benefits, as well as to whom these costs and benefits accrue and who is responsible to compensate whom. It is this architecture that provides the context within which people act, and, according to Bromley, it is this architecture that explains distinctively economic action.

Some Pareto economists recognize the significance of traditionally exogenous factors and seek to make them endogenous to their frameworks. This would amount to explaining the construction and maintenance of different institutions by appeal to cost-benefit calculus and rational choice. Bromley regards this endeavor misplaced, however, because the assignment of costs and benefits relies on a background of social practices through which costs and benefits are constituted as such. To explain these constituting practices as themselves the result of cost-benefit analysis is once again circular, as it makes use of the very norms and institutions that it intends to explain. Ultimately, Bromley concludes, the effort to render endogenous the institutions that structure economic activity is “doomed on logical grounds” (70). “Once

something is made endogenous,” he contends, “it is no longer capable of being explained by the structure within which it is embedded. By virtue of its embeddedness, it is now indistinguishable from the system of which it is a part—the ‘two’ things are, in fact, one thing” (70). On Bromley’s view, there is simply no way to explain human action purely in terms of instrumental rationality.

If the institutions that explain economic activity cannot themselves be explicated in economic terms, where should we look for their origins? Here is where volitional pragmatism takes on its volitional character. In contrast to Pareto economists who seek to reduce human action to the cold calculus of “a machine process,” Bromley urges:

What is needed here is not mechanism. What is urgently needed is, instead, a theory of institutions and institutional change built on the concept of prospective volition—the human will in action, looking to the future, and deciding how that future ought to unfold. The prospect of attaining particular outcomes in the future constitutes the reasons for humans to undertake specific events today—whether acting as individuals or acting collectively in those democratic entities (legislatures, parliaments, administrative agencies, courts) created precisely for the purpose of considering and implementing institutional change. When we get a grip on those reasons, we will get a grip on why institutions change (22).

According to volitional pragmatism, institutions change because we have sufficient reasons for changing them. It is human agents armed with good reasons for altering the architecture that defines fields of action that are *the* causes for institutional change. It is crucial to Bromley that “the human will in action” is the cause of changes in the formal and informal norms that regulate collective conduct. Volitional pragmatism draws on a certain interpretation of pragmatism’s

theory of action to free the human will from the mechanistic logic of more deterministic social theories. On this interpretation, policymakers can freely imagine creative solutions to social problems, and it is these creative imaginings that, once democratically negotiated, serve as justification *and* explanation for institutional change (Bromley 2006, 72-73). It is here that explanation “bottoms out”—Bromley takes the human will in action to be the necessary ontological level according to which we explain institutional change.

In order to understand the ontological character of volitional pragmatism, it's worth attending to one of Bromley's examples. Bromley contrasts the standard economic approach to tropical deforestation with that recommended by volitional pragmatism (169-178). On the standard approach, deforestation is modeled as the outcome of readily quantifiable factors in a region's economy, including rates of population growth, household income, infrastructure that provides access to markets, timber prices, foreign debt, and so on. The Pareto economist then attempts to correlate changes in these factors with observed rates of deforestation. The factors that are found to be antecedent to increases in deforestation are taken to be causes of the deforestation, and policymakers try to develop policies that will alter these factors and prevent further deforestation. Policymakers, for example, may find a strong correlation between population growth and deforestation, deem population growth the cause of deforestation, and set forth to construct policies that discourage population growth and thereby discourage deforestation. Alternatively, they may find a strong correlation between road building and deforestation, and set forth to discourage road building and the access it provides to rainforest timber.

Bromley's complaint is that the epistemology underlying such policymaking is inadequate. He charges that such analyses “focus too quickly on what is imagined to be the

antecedent conditions to deforestation” even though “events and outcomes such as deforestation can only be understood—explained—by giving explicit recognition to the purposes that might be served by those particular outcomes” (170). This leads Bromley to the somewhat hasty conclusion that deforestation happens “because there are purposes to be served by deforestation,” and further, that causes such as population growth or road building cannot count as causes because neither activity intends deforestation. By narrowing the search to causes emanating from human intentions, Bromley finds it “logically necessary” to explain deforestation by appeal to one of two possible causes; to either (1) earn revenues from harvesting trees, or (2) convert forest land to other uses. These intentions are, at the end of the day, the basis for any number of intermediate institutions that structure, but do not cause, deforestation. The Pareto economist who attributes deforestation to timber prices is appealing to institutions governing the exchange of timber, yet the volitional pragmatist insists that these institutions are themselves subject to explanation.

Bromley concludes his analysis of tropical deforestation with the following general remarks about explanation in the social sciences:

The key here, however, is to keep in mind that explanations pertaining to human action are incoherent without explicit reference to intent. All other alleged explanations are merely mechanical and, as such, provide no insights about policy reform. That is, the prescriptions that flow from mechanical causes will inevitably address symptoms of the problem—observed phenomena—rather than the reasons for those phenomena (176-177).¹⁴

¹⁴ There’s a lot going on here, and I focus only on a few philosophical confusions. It’s worth noting, though, that Bromley’s distinction between “observed phenomena” and “the reasons for those phenomena” is a straightforward appeal to the appearance/reality distinction. That distinction appears throughout Bromley’s work on volitional pragmatism and, as far as I can tell, echoes a Kantian conviction about the noumenal subject.

Bromley's hearkening to inevitability marks an important contrast between the volitional pragmatist's concept of causality and the Deweyan treatment of the concept of causality from the previous chapter. For Bromley, there *must* be some ontological level at which explanation bottoms out, and he argues that social explanations necessarily bottom out in the reason giving and taking of democratic deliberation, which makes explicit reference to intent. For Dewey, however, explanation doesn't bottom out in this way. Causality is a concept that functions to structure inquiry into problematic situations, but we do not discover *the* cause to phenomena—we take certain features of situations to be responsible for others, but we could take other features (but not just any other features), depending on where we choose to intervene. For most pragmatists, we certainly should not approach inquiry with preconceived notions about what sorts of things can count as causes.

Volitional pragmatism is motivated by a contrast between Pareto economics and institutional economics. Pareto economics is methodologically invested in a theory of human action—rational choice theory—that withdraws from the social context of preferences and purchases. Institutional economics is instead interested in the formation and maintenance of the social context within which economic activities can be rational. As Bromley takes pains to show, it is only within established norms for conduct that consequences can count as costs or benefits. This irreducibility of economic norms is the basis for volitional pragmatists' ontological distinction between human beings and other things. Human beings can be properly said to “act,” which means our movements must be understood as goal-directed rather than merely the mechanical interplay of cause and effect. And the goals toward which actions aspire are not the abstract goals of utility maximization; the goals of economic actions, qua volitional pragmatism,

are available as socially-recognized reasons to the actor. The human will in action is the ontological capacity to justify our actions by giving and asking for such reasons.

Bromley's theory of inquiry rests on a false but widespread conviction that, at some ontologically fundamental level, humans are different sorts of things than rainforest timber or new roads. Following Sellars we might agree that humans are different from this other stuff, but we would not hold this conviction on the basis of an ontological distinction between mind and body. And it turns out that our reasons for maintaining a distinction between persons and non-persons does matter quite a bit. Since volitional pragmatists believe that persons are uniquely free from mechanical causes and instead subject to teleological causes, they see no way to assign responsibility for social-ecological changes beyond assigning them to human actors. As I will continue to argue, insofar as we have reasons to assign responsibility to fellow human beings, it must be on pragmatic rather than ontological grounds.

The Ontological Basis of Social Theory

Bromley's reliance on an ontological basis for his volitional pragmatism places him into a longstanding debate between social theorists and philosophers. I here consider that debate as it plays out between Richard Rorty and Keith Topper, which concerns the specific matter of whether the success of a social science provides a reason for taking onboard the ontological assumptions of that science. At stake is whether we think of successful social scientific theories as representing the "really real," instead of the more modest achievement of describing the world in ways that help us to get about productively and reliably. On the one hand, many social scientific traditions have presupposed that the epistemologies of the biophysical sciences are appropriate for studying human behavior, and these traditions have sought to discover law-like generalizations for social activity. Rational choice theory is a variant of such traditions, though

the mindset finds its paradigm in behaviorism. On the other hand, other social scientific traditions have rejected the epistemologies inherited from the biophysical sciences and championed new methods that stake a claim to better understand social reality. These methods commonly trace their heritage to Clifford Geertz and attend to the richly symbolic practices of human beings (1973). Rather than seek law-like regularities among third-person observations of social behavior, these traditions argue that social science must begin from a first-person perspective. According to this view, it is only from within the perspectives of language-speaking communities that we might understand social conduct.

Richard Rorty tries to reconcile this debate in his “Method, Social Science, and Social Hope” (1982). At the time of Rorty’s essay, the debate had crystalized as a dispute over the specific method of hermeneutic interpretation. Advocates for the hermeneutic approach, most notably Charles Taylor (1971), insisted that the epistemologies of the biophysical sciences were inappropriate for studying human beings. In contrast to the objects studied by biologists, chemists, and physicists, social science was interested in objects (human beings) who used language to reflect on, and thereby direct, their own actions. Whereas studies of brute matter need not take the standpoint of its subject matter, the social sciences, according to this hermeneutical tradition, cannot proceed without taking that standpoint. In short, human beings are the sorts of things who create meaning, and understanding these meanings is necessary for understanding human beings. The hermeneuts maintained that the understanding gained through a first-person engagement with their subjects was, at minimum, a different sort of understanding than that offered by rival epistemologies. Some went so far as to claim that third-person methodologies ought not claim to understand human behavior at all, and that the immaturity of

the social sciences was in large part due to these methodologies' mistaken assumptions about the human subject.

While Rorty found this debate productive, he did admonish the more brazen advocates for hermeneutics. Social scientists get into trouble when they try to draw ontological conclusions from the success of their practices. Rorty cautions that the criticisms issued by Taylor and fellow hermeneuts are "a useful protest against the fetishism of old-fashioned, 'behaviorist' social scientists who worry about whether they are being 'scientific.'" But this protest goes too far when it waxes philosophical and begins to draw a principled distinction between man and nature, announcing that the ontological difference dictates a methodological difference" (198-199). Waxing philosophical starts when social scientists assume that the predictive success of their theories provides a reason for taking onboard the theories' ontological commitments:

...philosophers who make a sharp distinction between man and nature are, like the positivists, bewitched by the notion that the irreducibility of one vocabulary to another implies something ontological. Yet the discovery that we can or cannot reduce a language containing terms like 'is about,' 'is true of,' 'refers to,' etc., or one which contains 'believes' or 'intends,' to a language which is extensional and 'empiricist' would show us nothing at all about how to predict, or deal with, language-users or intenders (201).

Rorty here echoes Sellars' opening point that we find ourselves with both a manifest image (a language that contains terms like "believes" or "intends") and a scientific image (a language which is "empiricist," or refers exclusively to the features of the situation that any observer could verify, regardless of their familiarity with prevailing social norms). With Sellars, Rorty concedes that neither of these languages is irreducible to the other, and yet both resist the urge to draw ontological conclusions from this irreducibility.

Most pragmatists resist this urge because they set aside the longstanding philosophical question, “*Why* does this language help us to get about productively and reliably?” For such pragmatists, this question only has the semblance of intelligibility—all that we can say is that such a language *does* help us to realize our goals. Advocates for hermeneutics do engage this question, and it is the sort of question that begs an ontological response. Because the language preferred by hermeneutics, which draws on the first-person perspective of human subjects, often helps understand subjects’ reasoning and patterns in their conduct, *that* language is the one that more accurately represents the really real. Meanwhile, traditions that face criticism from the hermeneutics, mainly the many brands of empiricism which Rorty refers to as “Galilean” science, also engage the question. According to these traditions’ epistemological convictions, purely extensional and empiricist vocabularies are appropriate to all scientific endeavors because the modern success of the biophysical sciences attests to the merit of such vocabularies. The argument traces the same path: because third-person vocabularies are more productive and reliable, such vocabularies have proven their ontological purchase. Most pragmatists, with the pertinent exception being volitional pragmatism, simply reject the ability to draw these sorts of inferences. We might say “because one vocabulary or another is more reliable, we ought to use it when making sense of a particular problem,” but we should not say “because one vocabulary is more reliable, it must have a better handle on true reality (social or otherwise), and hence all inquiry must be conducted in its terms.”

Volitional pragmatism wants to make this further claim—to discredit rational choice theory and the constellation of assumptions, theories, and methods that undergird Pareto economics. On this point Bromley finds an ally in the hermeneutics and in particular Keith Topper.

Topper responds directly to Rorty's "Method, Social Science, and Social Hope" in his own essay, "Disunity, Pragmatism, and the Social Sciences" (2000). According to Topper,

...hermeneutical commitments to an ontological and methodological distinction between the natural and social sciences, and to the idea that 'a people's understanding must be among the things which any adequate theory can explain,' not only are compatible with pragmatism but are the offspring of it. With this in mind, we might profitably restate the conflict between Rorty and hermeneutical social science: the problem is not that hermeneutical social science is unpragmatic but that Rorty's pragmatism is not pragmatic enough (532).

Topper's charge here is worth examining. It accuses Rorty of three fallacious arguments. First, Rorty argues that hermeneuts have not refuted the possibility that social scientists could explain human conduct using the same epistemologies as the biophysical sciences (Topper refers to this as the possibility of scientific unification). Second, Rorty argues that hermeneuts must rest their epistemological and ontological convictions on outdated metaphysical dualisms. Third, Rorty accuses the hermeneuts of mistaking the first-person perspective as the social scientific corollary of "Nature's Own Language," or the idea that there is some vocabulary that carves nature, or social reality in this case, at its joints.

Let us quickly countenance the first and third arguments as they do not explicitly relate to the core of the debate. Topper notes that Rorty's first argument should not lead us to adopt the Galilean epistemologies for social science. Just because we have no a priori demonstration that an extensional vocabulary will not work, we are under no obligation to employ such a vocabulary. This in my mind is a bit of a distortion of Rorty's claims. What Rorty is advancing is the more modest claim that, until the productivity and reliability of extensional vocabularies is

ruled out once and for all, we should continue to experiment with those (among other) vocabularies. Because ruling this out once and for all would require an ontological commitment that Rorty finds unjustifiable, advocates of hermeneutic and Galilean methods should not debate the unity or disunity of social and biophysical sciences (see Kitcher 1981). Meanwhile, the third argument should not be read as an argument for or against Galilean methods, but as a therapy to help us understand the rise of ontological philosophizing in the first place. A genealogy of modern commitments to a language that carves nature at its joints cannot adjudicate whether we are warranted in maintaining such commitments. What Rorty is trying to do by chronicling that history is help us understand ourselves and the contingencies of our metaphysical assumptions, which for at least some of us will help loosen the grip that these assumptions have on us. Perhaps we should maintain them, but our resolve should be warranted only according to whether these commitments improve the quality of our inquiry.

It is with respect to Rorty's second argument that Topper and Rorty fundamentally disagree. Topper remarks that extensional approaches such as behaviorism, cognitivism, operationalism, systems-theory, and rational choice theory have not found predictive or explanatory success to date. Meanwhile, Topper asserts that hermeneutic approaches have found such success (curiously, evidence for this is conspicuously absent from the essay). Accordingly, Topper believes that we should adopt hermeneutics' ontological and methodological commitments on pragmatic grounds. He argues that this belief "rests on a claim about ontological differences between physical nature and human beings" but that "it is manifestly not a metaphysical thesis in the sense that most pragmatists presume when they describe metaphysics pejoratively as a 'sterile' or 'vain' activity" (530). Rather, "far from being a product of disembodied exercises in first philosophy, the ontological and methodological distinctions adumbrated above emerge from

examination of the fortunes of different types of natural and social scientific inquiries” (530). These examinations lead Topper and fellow hermeneuts to conclude that human beings are a different sort of thing from physical objects, and as different sorts of things, they require different methods for inquiry.

Topper is straightforwardly drawing the inference that Rorty forbids: we cannot infer from the productivity and reliability of a vocabulary anything ontological in the robust sense that Topper requires.¹⁵ In order for the productivity and reliability of hermeneutic methods to count *against* Galilean methods, Topper first needs to locate the two methods as rival ontological positions, and second needs the success of one method to substantiate its ontological purchase. The first need, according to which the two methods are deemed incompatible, requires that we think of social reality as consisting of objects for which there is only one true way of describing. The second need requires that we ask *why* certain vocabularies work, and here it is useful to remember William James’ point that “it is useful because it is true,” or “it is true because it is useful” mean exactly the same thing for our epistemic practices (1907, 115). Vocabularies that we find productive and reliable are true as far as it goes, but no further. And in the absence of bottoming out in a robustly ontological sense, social scientists such as Topper and Bromley cannot justify their preferred ontological and epistemological commitments the way that they have.

The Pragmatic Basis for Social Theory

While robustly ontological and epistemological commitments cannot justify choices between vocabularies, I want to spend this final section explaining how normative and

¹⁵ Dewey was prone to say something like “the practical achievements of our inquiries can attest to the potentiality of nature to abide by the regularities to which our inquiries attest.” Rorty is on the whole skeptical of whether these potentialities add anything above and beyond the specific claims made about the objects themselves, and would prefer we stop talking about “nature” at all. In any event, Topper needs these practical achievements to have more ontological purchase than either Dewey or Rorty allow.

valuational commitments can justify the logical structure of social scientific inquiry. The basic claims are as follow: (1) most social theory is, by design, operating at a logical level more complex than that of behaviorism and its correlates, (2) the logical level distinguishes within experience those practices that are taken to be given (for the purposes of a particular inquiry), (3) such practices are intersubjectively maintained, and (4) by operating at such a level that social theory can offer explanations that intervene in the practices at the level.

At the outset, it is important that I complicate the account that Rorty provides regarding the pragmatics of social scientific inquiry. Rorty is correct to diagnose the dispute between hermeneutic and Galilean social science as a pragmatic disagreement masquerading as an ontological debate. Where hermeneuts and behaviorists essentially disagree, on Rorty's take, is over the goals that social science ought to help us better pursue. In part this disagreement is spurred by the uncomfortable role that social scientists are asked to play in contemporary societies. Social scientists are expected to play two parts: first, they are expected to provide value-free descriptions that serve as the basis for evidence-based policies or management strategies, and second, they are expected to issue these descriptions in terms that already hold significance to policymakers, managers, and their constituents (197). This is exactly the set of obligations that Norton (2005) finds impossible to reconcile given prevailing misunderstandings of the practice of science. Wrest terms with evaluative significance from the vocabulary of any science, and the descriptions that they offer for the world will not mean anything for deliberation and collective action.

Galilean and hermeneutic social scientists accept these terms of the debate but disagree over how to reconcile the two roles (198). Galilean social scientists assume that a value-free social theory will most widely predict human behavior and that policymakers will be able to

bring their values to bear in deciding on a course of action in light of these predictions.

Hermeneuts assume that the only predictive social theory is the one articulated from the perspective of community members and that this language is already invested with sufficient significance for legitimate policymaking. Rorty's point is simply that the roles are not so tightly interwoven, and that it is both possible to issue a value-free but policy-irrelevant social theory and possible to issue a social theory in policy-relevant terms that is poorly predictive.

Hermeneuts' reasons for preferring a first-person perspective, and the vocabulary used from this perspective, is not merely empirical. Rather, Rorty intimates, we adopt a first-person perspective on moral rather than epistemic grounds. He writes:

Friends of hermeneutics have protested that Behaviorese was inappropriate for 'understanding' people—meaning that it could not catch what they were 'really' doing. But this is a misleading way of saying it is not a good vocabulary for moral reflection. We just don't want to be the sort of policy-makers who use those terms for deciding what to do to our fellow-humans (198).

This diagnosis is oversimplified. While Rorty is correct that moral considerations must be brought to bear when choosing a vocabulary for social science, he suggests a false dichotomy between first-person and third-person perspectives.¹⁶ Instead the choice should be from a plurality of frameworks that span the vocabulary of first-person moral reflection and the purely extensional vocabularies that Topper finds lamentable in behaviorism, cognitivism, operationalism, systems-theory, and rational choice theory. We can truthfully describe the social dimensions of social-ecological systems in all of these frameworks, and none is uniquely true in

¹⁶ This is less of a provocative claim than it may seem. Social science is constitutively about human subjects, and describing human subjects has moral implications *because we in fact do import such significance on such descriptions*. In short, it would be disingenuous to say both "here is how human beings act in a particular context" and "these are simply the facts and mean nothing for how we assess one another's character and assign rights and responsibilities." Language-users simply do not have that sort of authority over what their verdicts mean.

the sense that it refers to a fundamental social ontology. Rorty sets up a narrow choice between purely extensional and empiricist vocabularies that rarely mean much for policy and management and the subject's own self-understanding that can be unreliable or idiosyncratic. The real choice, however, is between the diversity of frameworks that are (contingently) reliable *and* formulated in terms pertinent to deliberation and collective action.

These frameworks are represented in the wide-ranging approaches in the social sciences, from anthropology and human geography to policy science and institutional economics. The primary purview of all of these social sciences is human action, which resists description in purely empiricist vocabularies.¹⁷ When an anthropologist investigates the traditions of a community, say the importance of oral traditions in sharing knowledge of caribou behavior, they hardly describe these traditions as the mechanical interplay of stimulus and response. Likewise, the institutional economist who describes the maintenance of property rights systems does not, per Bromley, issue descriptions in the reductive calculus of cost-benefit analysis. Social sciences are studying human actions, not human movements, and this means that they must understand (most of) the behavior that they observe as purposive, not mechanical. This attests to the first claim, that most social theory is, by design, operating at a logical level more complex than that of behaviorism and its correlates.

To clarify briefly, and without getting too much into the weeds, it is important to note the notion of “operating at a more complex level” as a “grammatical” remark (Wittgenstein 1953).

The grammar of a concept is the sense in which it refers to objects in the world. We might think

¹⁷ While I am skeptical that any social science could peddle in purely empiricist vocabularies (e.g. I think even the rigorously behaviorist framework would struggle to predict human action through a mechanistic description alone), I do think folks like Rorty and Norton have provided sufficient reasons to reject such vocabularies as the basis for policy and management. The upshot is that either these vocabularies do have significance for our policy and management, and that they are therefore subject to pragmatic analysis, or they do not, and escape this analysis at the expense of their relevance.

of grammar as the set of rules that determine how to verify the truth or falsehood of statements that use the concept. Now, Galilean vocabularies abide by very simple grammars—the terms of these vocabularies refer to objects that can be verified by observing their presence at a given time. This simple grammar is part of the power of these vocabularies, as the set of observations required to investigate the world through these concepts is straightforward and capable of garnering widespread agreement. Meanwhile, the grammar of action concepts is more complicated. Actions refer to patterns in the behavior of agents. To verify that a person is “sharing” is not as simple as observing an exchange, but further observing that they do not receive remuneration (which might be better described as “selling”), that they do not lament the delayed return of the shared thing (“borrowing”), that they are not simply discarding unwanted goods (“giving away”), and so on. This is just what “sharing” means. That it would be difficult to disambiguate an action as either sharing or selling or so on attests to the complex grammar of action concepts.¹⁸ Hermeneutic methods are essentially a practice of disambiguation; the act of “interpreting” is practically a matter of verifying the truth of statements that require that we engage the whole field of action to validate observations about particular action statements. Hermeneuts are correct to treat the expressions of subjects as acts too, and to find satisfaction in the description that holds up in the face of continuous observations of physical and linguistic acts. It is a mistake, though, to conflate grammatical complexity with ontological complexity. What grammatical complexity achieves is a “radically different way of relating human beings to the world” (Sellars 1963), a way that allows us to anticipate and coordinate our own actions with the purposes and pursuits of our fellow actors.

¹⁸ The notion of “complexity” is liable to give the impression that action concepts refer to complex objects that are composed of the simple objects to which Galilean vocabularies refer. That is an impression that is based on a bad picture of language and one that we need to resist—actions are not reducible to movements, nor are observations of actions reducible to observations of movements. It might be better to simply say that action concepts abide by a *different* grammar, but given widespread (mis)understandings of observation, the idea of complexity is useful.

By operating at this more complex logical level, social scientific descriptions describe practices in the world. This is to say that the distinctively social terminology of a social scientific theory is grammatically teleological, that locating “the will in human action” is describing, to ourselves and to one another, our movements as actions that bring about foreseeable responses in our interactions with one another and the world. Terms such as “logging,” or “fishing,” or “hunting” are all concepts that require inquirers to observe patterns in the activities of communities. No single, one-off observation can verify whether a community is logging, and this grammar holds for all of our social terminology. Movements are called “logging” only after we observe the norms that seem to govern those movements. This is the trouble with purely extensional descriptions of human conduct—operationalizing action concepts as a list of observational conditions does not explicate the rules that distinguish the right application of the concept from the wrong. Still, observing the norms governing the use of action concepts is observation, it is still empirical, even as it is oriented to these observations as teleologically structured.

It is worth stressing that these patterns in human practices are not attributable solely to the intentions of human actors. They depend on the maintenance of a social context within which movements give rise to regular and predictable responses from others; in short, they depend on a social reality. Following Searle (1995), we should remember that our logical commitment to the existence of reality, social or otherwise, is not a commitment to a reality that has joints (see Rorty 1979). Reality in this logical sense is not the commitment to there being any unique set of objects that our statements mirror, but rather the more modest commitment that the stuff that our statements are about makes those statements true or false independent of the investigator’s beliefs. As Searle points out, the stability of the stuff predicated by social descriptions is

intersubjectively maintained—we *can* rely on regularities in the behavior of others. It is our participation in the norms that constitute social reality that gives meaning to our social descriptions, both in the sense that this participation is a condition for statements being either true or false, and in the sense that participation is a condition for descriptions to help us navigate that social reality.

So when we investigate through the use of social terminology, we are describing practices that depend on the actions of others in order for those descriptions to be true. When we cite a particular practice as a cause, in other words, when we take our descriptions to be the basis of explanations, we assume the social context within which that practice leads to the effects that it does. We are ordinarily right to assume this—after all, social practices rarely change overnight—but it is nonetheless important to note how reliant social inquiry is on background normative practices. The regularities upon which social descriptions depend are owed to the fact that individuals pursue their goals by navigating this intersubjectively maintained field of action. This means that values are at stake twice over. In the sense expressed in Dewey’s theory of inquiry and elaborated by Norton, valuation is necessary in analyzing a situation as a problem and in deciding what features of that situation serve as causes through which we can intervene. But in social scientific explanations, values are also constitutive of the level at which we describe the situation, the level of social practices for which we can provide better or worse reasons. As noted above, we should understand this layering as a design feature of social scientific inquiry. We do not strip bare our vocabulary for navigating social reality to the grammatically simple, behaviorist models of cause and effect, simply because doing so would produce interventions that ignore the rich social contexts that facilitate our way of life. We *want* descriptions that

participate in our existing social reality, because we want interventions that leave most of that reality intact.

In the next chapter I analyze social scientific inquiry in social-ecological systems science as a way to elucidate this chapter's arguments. What should be clear going forward is that social scientific inquiry is value-laden above and beyond the sense appreciated by environmental pragmatists. Social science that is significant for environmental policy and management mobilizes concepts that orient us to the purposive character of human action. The diversity of disciplinary approaches in the social sciences reveals a range of value-laden methods for describing and explaining social practices, and our choice among these methods depends on our normative and valuational commitments. When we cite human actions as causes of environmental change, we are making a pragmatic claim that changing particular social practices can serve as a means for bringing about different ends. The social practices that we may change are already value-laden, and part of providing pragmatic warrant for an explanation is countenancing the social values that are at stake in making that change. For example, intervening in the way that a community maintains its property rights institutions requires that we consider the value of the consequent environmental changes *as well as* the value of the new social relations that we must subsequently maintain. These are the values, absent from most discussions of science and democracy, concerning who we are and who we are to become.

Conclusion, or Mechanism in Social-Ecological Systems

Social scientists like Bromley and Topper are correct to describe our social reality in terms that matter to us and according to a grammar that orients us to our actions rather than our movements. Bromley and Topper rightly resist the mechanistic accounts of human action that they argue miss "the human will in action" (Bromley 2006, 22). But my contention is that they

are wrong insofar as they take this will to be an ontological discovery about human nature, a ground upon which any adequate social theory must be built. The sense that human will is “in” action ought to be understood logically, or grammatically, as a consequence of how we are oriented to the world when we describe it teleologically. With Sellars we should remember that the conflict between telos and mechanism, between mind and body, is not a substantive conflict. Rather, it is a pragmatic decision about how it is that we are oriented to the world.

When social-ecological systems scientists pursue a mechanistic account of social-ecological systems, they do not escape the need to make evaluative judgments. If by mechanistic understanding they mean something like the facts about causes and effects, they face pragmatic decisions about where to locate interventions. If they instead mean something like a third-person account, as contrasted with an interpretive or hermeneutic account, they still confront value-judgments. Unless we are after a description of our social practices that strips down to mere behavior—a description that would, in effect, alienate us from the meaningfulness of our existing way of life—a purely empiricist or extensional vocabulary will not do. Insofar as we wish to leave intact most of what matters to us, we must conduct our social scientific inquiry in terms that already register within our social lives. Operating in this register means that our assertions have implications for who we are and who we are to become, a set of values that environmental scientists are mistaken to think they might avoid.

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CHAPTER 4

THE MEANING OF THE SOCIAL WITHIN SOCIAL-ECOLOGICAL INQUIRY

Abstract: This chapter argues that the choice among social scientific theories in social-ecological systems science must appeal to non-epistemic, social values. It further argues that social-ecological systems scientists tend to describe systems in language that is inadequate for recognizing and legitimating such non-epistemic values. I trace this inadequacy to the early emphasis on structural or functional explanations of social practices, which echoes the ontology of ecology, but which neglects the ways that human agents reproduce these structures and perform these functions. Situating recent frameworks for integrating social dimensions into Chapter Three's categories of Galilean and hermeneutic methodologies, I recommend Muriel Cote and Andrea Nightingale's approach, which calls for social sciences that are explicit about the values they advance and the constraints they accept. I then explore a meta-study of coastal and marine social-ecological systems sciences to demonstrate three interrelated claims. First, the vocabulary in which explanations are warranted reflects the disciplinary background of the social scientists in a collaboration. Second, different vocabularies make different valuational and normative assumptions about the practices worth upholding and the practices open to change. And third, this value-ladenness of social explanations has downstream and upstream implications for how social-ecological systems scientists are accountable to stakeholders' objections. Through a concluding reflection on Dewey's "Three Independent Factors in Morals," I recommend deontological and virtue-theoretic vocabularies as resources for locating concepts that might inform upstream decision-making.

The Early Emphasis on Structure and Function

In the early years of social-ecological systems science, researchers were unsure how to integrate the social sciences, and their reliance on ecological frameworks incorporated the social sciences in a way that was partial and problematic. Fikret Berkes and Carl Folke's call for a science of people management was largely inchoate in *Linking Social and Ecological Systems*. The editors characterized social-ecological systems science as a "people-oriented approach," and, of course, echoed the mantra that "resource management is people management" (1998, 2). This mantra ensured that the social sciences played a particular role in inquiry. In the volume's early articulation of social-ecological systems, the human dimensions of the environment were

closely tethered to resource extraction and use. Resource management provided the link between social and ecological systems, and the primary purview of social-ecological systems scientists was to investigate institutions that regulate resource management. Chapters considered various social institutions that bear on resilience and sustainability, but all of the institutions considered were right at that boundary between human and ecological dimensions. In this early agenda for social-ecological systems science, it was our understanding of the ecological system that provided orientation to the overall system; the social system was conceptualized only in its relation to the ecological system. Institutions regulating transactions between human communities and ecosystems were located in the contours of ecological systems that were largely taken for granted. Given the expertise of Berkes and Folke, as well as the many contributors to the volume, this orientation was hardly surprising.¹⁹ But the orientation produced an asymmetry that is the focus of this first section—ecosystems were conceptualized from the ground up, and the components held together as a system, but social systems were built by adding components to the already conceptualized ecosystems, and the relations between these added social components were much less visible.

Ultimately this methodology led social scientists of people management to try to “reinvent the wheel” (Fabinyi, Evans, and Foale 2014, 34). The vocabulary used to describe social systems in social-ecological systems science bears little resemblance to existing theories from disciplines such as sociology or political science. At the conclusion of the volume, Berkes, Folke, and Colding (1998, 418) provide a list of social mechanisms for resilience and sustainability. The full list is worth reflection:

¹⁹ Chapters were authored by anthropologists, human ecologists, human geographers, and natural resource economists, though more critical traditions in the social sciences such as sociology and political science were conspicuously absent.

2. Social mechanisms behind management practices

- a) Generation, accumulation and transmission of ecological knowledge
 - Re-interpreting signals for learning
 - Revival of local knowledge
 - Knowledge carriers/folklore
 - Integration of knowledge
 - Intergenerational transmission of knowledge
 - Geographical transfer of knowledge
- b) Structure and dynamics of institutions
 - Role of stewards/wise people
 - Community assessments
 - Cross-scale institutions
 - Taboos and regulations
 - Social and cultural sanctions
 - Coping mechanisms; short-term responses to surprises
 - Ability to re-organize under changing circumstances
 - Incipient institutions
- c) Mechanisms for cultural internalizations
 - Rituals, ceremonies and other traditions
 - Coding or scripts as a cultural blueprint
- d) Worldview and cultural values
 - Sharing, generosity, reciprocity, redistribution, respect, patients, humility

Two things stand out here. First, there is an obvious presence of anthropological theory in the articulation of these mechanisms. Given Berkes' and Folke's burgeoning interest in traditional ecological knowledge, as well as the involvement of a number of anthropologists who provided case studies, this presence makes sense (Gadgil, Berkes, and Folke 1993). The second noteworthy feature is the structural emphasis of many of the mechanisms that do not harken from anthropology. The "Structure and dynamics of institutions" is the most populated category, and includes characteristics such as cross-scale institutions, responses to surprises, and ability to re-organize, all of which are paradigmatic characteristics of resilient *ecological* systems. In trying to reinvent the wheel, early attempts at social-ecological systems science produced social theories that looked an awful lot like ecology.

For those familiar with the social-ecological systems literature in the decade following *Linking Social and Ecological Systems*, this dual emphasis neatly characterizes the research agenda. On the first front, the literature on traditional ecological knowledge grew rapidly (Berkes, Colding, and Folke 2000, Folke 2004). In part because theories of knowledge production and distribution are clearly normative, this literature has been thoughtfully engaged and problematized from diverse perspectives (Simpson 2004, Shackeroff and Campbell 2007). The second front, though, is only recently receiving criticism. Muriel Cote and Andrea J. Nightingale point out that social-ecological systems science “has mainly evolved through the application of ecological concepts to society, problematically assuming that social and ecological system dynamics are essentially similar” (2012, 475). By attending to the structure and dynamics of social systems, social-ecological systems researchers set themselves apart from contemporary critical social sciences. This is not because sociologists, anthropologists, and political scientists had not considered social systems as structures that are dynamically maintained—indeed, it is exactly because many disciplines operated in a *post*-structuralist paradigm that they had little to say about this reinvented wheel (Fabinyi, Evans, and Foale 2014, Bell 2005). For about a decade, social-ecologist systems scientists explained social-ecological change by appealing to structural characteristics of social systems. Instead of talking about power or coalition building, these scientists described the political in terms of polycentric governance and decentralized decision-making (Folke et al. 2005, Ostrom 2010). Instead of reflecting on the social reproduction of identity and subjectivity, they described the social in terms of the distribution of risks and the adaptive capacity of vulnerable groups (Lebel et al. 2006). The cultural found its place in the new social-ecological systems discourse through descriptions of connectivity and social capital

(Brondizio, Ostrom, and Young 2009); the economic through property rights systems and poverty traps (Anderies, Janssen, and Ostrom 2004, Holling 2001).

These conditions made it difficult for more critical social scientists to collaborate with ecologists and other biophysical scientists. Frustrations traced the rift between Galilean and hermeneutic epistemologies discussed in Chapter Three; on the one hand, the majority of biophysical scientists favored purely extensional and empiricist vocabularies, while on the other hand, the majority of social scientists favored internal analyses in research subjects' own language (Lele and Norgaard 2005, Rorty 1982). In an excellent article by Todd A. Crane, these methodological disagreements are succinctly summarized:

...external analyses of a social-ecological system tend to construe it solely as a mechanistic web of interlinking actions and outcomes. Although this may be empirically accurate, it is likewise incomplete. In internal analyses of the same social-ecological system, one conducted in the minds and communities of people who live within them, the web of actions and outcomes is equally a web of interlinking socially constructed meanings and normative values that are intimately interconnected with the material behaviors, social institutions, and environmental outcomes. (2010, 19)

Crane is uniquely careful to avoid drawing ontological conclusions from the successes of these disparate methodologies, but other social scientists positioned their disagreement as fundamentally ontological. As sociologist Michael M. Bell explains, “several of the social sciences are quite wary of systems theory, especially sociology and anthropology, still spooked by the ghosts of structural-functionalism and its effort to chart the ‘social system’” (2005, 472). As Bell tells it, any social scientist who had suffered through an introductory social theory course had the errors of systems approaches drilled into them, and saw in such systems approaches the

denial of power and agency that they now knew to be fundamental to system maintenance and change.

The difficulties in integrating social and ecological sciences can be summarized as follows: early social-ecological systems scientists strove for a mechanistic account of social institutions that would allow researchers to straightforwardly extend their models of ecological structure and function to include human practice and culture. Most of the well-developed disciplines in the social sciences did not lend themselves to this sort of integration, so social-ecological systems scientists faced the daunting challenge of reinventing a social science in their preferred extensional and empiricist terms. Not unsurprisingly, this innovation produced structuralist and functionalist accounts of social systems that drew skepticism if not cynicism from traditional social scientists. Far from providing a *lingua franca* to integrate our understanding of both the social and ecological dynamics of complex systems, the social-ecological approach further privileged biophysical explanations and silenced critical contributions from social scientists (Bell 2005). At least for the first decade of social-ecological systems research, the social entered into research only through the novel theoretical notions such as “geographical transfer of knowledge” and “incipient institutions” (Berkes and Folke 1998, 418). This focus on structuralist and functionalist explanations led researchers to neglect the ways that human agents reproduce structures and perform functions, leading to a partial and problematic integration of social dimensions.²⁰

²⁰ Structuralist and functionalist frameworks are incomplete in precisely the sense that worried folks like Rorty and Norton; while they may successfully predict the variables of concern to a particular model, they have little bearing on our pragmatic deliberations over how to act in the world. Variables of concern to traditional social scientific disciplines, such as power and agency, inherit vocabularies in which we can deliberate about the ethical implications of different management and policy options. Structural or functional characteristics, such as “cross-scale institutions,” do not benefit from what Crane called “a web of socially constructed meanings and normative values” (2010, 19). Vocabularies impoverished in this way make it immensely difficult to deliberate about whether it would be ethical to foster cross-scale institutions toward building resilience, or whether we should prefer to foster coping mechanisms, or perhaps instead incipient institutions.

Frameworks for Integrating the Social

More recently, the epistemological and ontological barriers separating social-ecological systems science from the more critical social sciences have met new efforts to integrate across disciplines and create frameworks for cross-disciplinary collaboration. Here I want to consider three proposals that I take to be emblematic of the types of efforts that may characterize this new frontier, and which fall neatly into Chapter Three's categories of hermeneutic, Galilean, and pragmatist methodologies. The first proposal, from Michael Fabinyi, Louisa Evans, and Simon J. Foale (2014), diagnoses these barriers as a matter of inadequate theory. They propose a new social science of social-ecological systems that carries the torch of social anthropology and political ecology and that replaces the structural-functional emphasis with a power-agency emphasis. The second proposal, from Maja Schluter and colleagues (2017), offers a framework for choosing among rival social scientific theories on the basis of their empirical fit with the social context of inquiry. Far from taking issue with the extensional and empiricist epistemology of past efforts, they provide a survey of behavioral sciences in the hope that collaborators can choose the most predictive option. Finally, I consider a slightly earlier proposal from Cote and Nightingale (2012) that suggests a pragmatic approach that differentiates between rival social sciences on the basis of the valuational assumptions that each makes about human action and social relations. This approach best recognizes the role that values play in inquiry, and it gestures toward considerations that I later argue to be at the core of choosing between social theories. The dialectic between the three proposals should be familiar; the hermeneutic approach misunderstands its warrant as ontological, the Galilean approach misunderstands inquiry as purely empirical, and the pragmatist approach offers a middle path grounded in a more holistic account of inquiry.

Fabinyi, Evans, and Foale's (2014) proposal can be summarized briefly, as it traces the same critiques of social-ecological systems science discussed above. In light of "a growing recognition that the emphasis on human relations with the environment has led to a weak theorization of the 'social' in the SES model," the authors offer lessons from social anthropology and political ecology as an alternative theorization (28). These more critical social sciences depart from the structuralist and functionalist commitments of earlier social theories, and thus offer a rehearsal of the transformation necessary to more strongly theorize the social in social-ecological systems. Chief among their criticisms is that the reductionism of fields like ecological anthropology, which anticipates social-ecological systems science, so emphasizes the ecological consequences of social institutions that it neglects the role of these institutions in maintaining social hierarchies and power (29). Institutions often serve multiple purposes, and when researchers focus only on the role of institutions in managing natural resources, they ignore the ways that those same institutions can perpetuate unjust social relations. A stronger and more complete theorization of the social would help us to understand social mechanisms that do not immediately pertain to the management of natural resources.

Though Fabinyi, Evans, and Foale provide valuable insights into the sorts of truths that social anthropology and political ecology offer (as distinct from ecological anthropology), they occasionally wax philosophical about the reasons why these critical fields provide better theories, full stop. They argue

The wider, fundamental point that is made in much of this literature is that the ways in which an environment (or an SES) is managed is not something that can be reduced to a matter of objective analysis. It is a value-laden exercise that is contested by groups with different power, who employ a range of strategies that include debating and negotiating

the very ways in which environmental issues are commonly understood and represented.

(33)

This echoes the dispute between Taylor, Rorty, and Topper detailed in Chapter Three; according to Fabinyi, Evans, and Foale's proposal, the shortcoming of prevailing social-ecological systems theories is that it falsely assumes that human behavior can be characterized in objective terms, despite hermeneutical methods that (on their view) prove that human behavior is just not that sort of thing. Instead of a social science in objective terms, they recommend drawing concepts from social anthropology and political ecology, which address some of the biases of prevailing conceptualizations of the social. The trouble here is that the notion of "bias" is unclear in this proposal, and the authors' give the impression that prevailing conceptualizations are biased because they overlook the real role played by social diversity and power. Once this sort of bias is acknowledged and taken as a reason to pursue another theory, the methodological discussion is directed toward a pursuit of the social theory that includes everything that we know about social reality.²¹ This, as noted in Chapter Three, is a misguided pursuit, for we shouldn't strive for a social vocabulary that "mirrors" the really real, nor should we warrant our social theories by appealing to supposed ontological truths.

This philosophical pursuit compromises an otherwise promising proposal for cross-disciplinary collaboration that concludes the social scientists' argument. The authors realize that the quest for a "theory of everything" or "overarching 'grand narrative' that describes the nature of reality" is too ambitious of a goal (33-34). They instead propose a more humble commitment to reflexive social theory and cross-disciplinary collaboration. As I go on to argue in Chapter

²¹ We need "bias" in these discussions to be a remark about the social values that are upheld by describing the world in one social theory rather than others. Theories cannot be biased without reference to social values, and by using the term without such reference, we are liable to give the impression that there is some social theory that is unbiased, full stop. The alternative it seems to me is that all social theories are biased, which is going to undercut the inference we wish to draw in preferring one theory to another in light of its bias.

Five, I believe this is the correct response to the diversity of irreducible social and ecological theories. By privileging social anthropology and political ecology because they recognize particular social truths, however, Fabinyi, Evans, and Foale intimate that this cross-disciplinary inquiry finds its foundation in the way the world is. Despite their protestations that there are serious issues with the idea of “the nature of reality,” they offer no other basis for adjudicating between rival theories.

Meanwhile, Maja Schluter and colleagues (2017) advance a Galilean proposal where different social theories are compared according to their predictive accuracy in a given context. They too recognize that “knowledge is fragmented across disciplines and disciplinary languages” and aim “to provide a tool and common language for mapping, describing, organizing, comparing, and communicating theories of human decision-making” (22). This lingua franca would help identify the appropriate behavioral science for a particular social-ecological system, allowing modelers to locate the true causal relationships in the system (23). The proposal mobilizes a framework that attempts to render commensurable diverse behavioral sciences according to a schematic framework they take to be implicit to all. Specific behavioral theories, such as rational choice theory or descriptive norm theory, are categorized according to states of the agent (goals, values, knowledge, and assets), perceptions, behaviors, and feedback loops by which perceived and evaluated behaviors influence subsequent states of the agent. This categorization is offered as a contribution to an integrated social science for social-ecological systems, and nearly twenty years after *Linking Social and Ecological Systems*, scholars such as Schluter and colleagues still espouse that “managing natural resources is managing people” (29).

Ultimately, I agree that some sort of common framework is necessary to reasonably compare different disciplinary languages. The trouble here is that Schluter and colleagues

advocate for theory choice according to exclusively epistemic values; this commitment, as argued in Chapter Two, is based on an inadequate theory of inquiry. Their argument for theory choice champions formalization and completeness, in addition to a fairly opaque recommendation that the behavioral theory describe in detail those aspects “critical” or “crucial” to the management context (33). Formalization is valuable because it makes explicit assumptions regarding causality so that these assumptions can be subjected to empirical scrutiny; the authors go so far as to suggest that theories which are not explicit about causal relationships may be inappropriate to social-ecological systems science (33). Completeness is valuable because theories that cover a wider range of behaviors provide more information about the causes and consequences of environmental behaviors. Both are fairly standard epistemic values cited for favoring one theory over another, though they are certainly in the mold of values like “simplicity” that have been shown to privilege particular social values (Longino 1995).²²

The concern for “critical” or “crucial” aspects of behavior is promising, because we may be able to have an open deliberation about the goals for, and constraints to, social scientific inquiry. Unfortunately, the authors do not clearly unpack these ideas, and in their final remarks, offer sensitivity analysis as a way of locating critical causal mechanisms. Sensitivity analysis is a way of determining what independent variables in a model have the largest influence on dependent variables; if a dependent variable is highly sensitive to a particular independent variable, then small changes in the value of that independent variable can have dramatic effects on the dependent variable. If such sensitivity is what they mean by ‘critical’ and ‘crucial’, their

²² Longino offers the example of simplicity justifying the preference for rational choice theory and several methodological commitments in economics (e.g. treating heads of household as the main economic actors). In this case, the appeal to simplicity functions to maintain patriarchy in economic studies and the policies based on these studies. For Longino, examples from economics show how simplicity leads us “to suppose the social world is composed of just one or a few kinds of basic entity...eras[ing] the difference among persons that are fundamental to how they act” (1995, 393).

meanings are too narrow; the sensitivity of a social-ecological system to particular interventions is hardly a sufficient reason to choose a theory. After all, we might reasonably choose to intervene in a system according to a mechanism to which the system is less sensitive, but that is more consistent with our non-epistemic values. Perhaps introducing property rights is an immensely efficacious way of producing particular management practices; we could still reasonably prefer an alternative system of coordinating resource management because of legitimate worries over the sorts of interpersonal relationships characteristic of privatization.

There is another reason why choosing a social or behavioral theory should not be a purely epistemic decision. As Schluter and colleagues carefully elucidate, different behavioral theories develop in different observational and experimental contexts, and hence locate causal mechanisms that operate in those particular contexts. When the authors talk about choosing a theory, they recommend choosing the theory which makes assumptions about the context that more or less fit with the social context of a given inquiry. For instance, rational choice theory assumes that agents have all relevant information available to making decisions that maximize their individual utility, whereas bounded rationality theory attempts to grapple with decision-making for agents with limited information and varied cognitive capacities (26-27). As they note at the onset of the paper, the assumptions characteristic of rational choice theory are notoriously idealized, and rarely characterize a particular social system. This might lead us to prefer theories such as bounded rationality theory that do not make such idealized assumptions, especially when these theories are more descriptively accurate with respect to agents' decisions in a given context. But this is not the only conclusion that we could draw from such an understanding of social mechanisms. We might have good reasons to intervene according to the mechanisms understood in rational choice theory, and strive to create a context wherein these mechanisms

become descriptively accurate. In this highly streamlined case, we can imagine providing full information and building cognitive capacity so that the assumptions of rational choice theory come to fit the social context. Indeed, Schluter and colleagues point out that the range of behavioral theories that they consider all neglect the possibilities of learning and institutional change (32). We may have good reasons for limiting our analysis to the behavioral theories that they consider, but these reasons are definitely pragmatic, not purely epistemic. They are reasons regarding the sort of social reality we hope to collectively maintain, and thus the sort of social reality about which we desire truths.

It is Cote and Nightingale's (2012) account of the social sciences that best wrestles with pragmatic and especially ethical questions at the heart of choosing a social scientific theory. They offer a similar historiography of the emergence of the social in social-ecological systems, accusing the field of "problematically assuming that social and ecological system dynamics are essentially similar" (475). "More specifically," they write, "The reliance on ecological principles to analyse social dynamics has led to a kind of social analysis that hides the possibility to ask important questions about the role of power and culture in adaptive capacity, or to unpack normative questions such as 'resilience of what?' and 'for whom?' when applied to the social realm" (479). This way of problematizing past social theories teeters toward the hermeneutical convictions of Fabyini, Evans, and Foale. But Cote and Nightingale do not appear committed to the idea that particular social theories better track the really real, or even that there is any fundamental social ontology to discover. Instead, they advise that asking normative questions such as "resilience of what?" and "for whom?" renders visible researchers' own orientation to the objective features of social-ecological systems. Recognizing the role of power and culture is not a matter of adding *more* objective features, or replacing the wrong objective features with the

right ones, but instead entails a more radical shift in our theory of inquiry. Objective inquiry, and its paradigmatic social-ecological models, becomes situated in the pragmatics of our way of life.

Cote and Nightingale recommend a shift in the methods and subject matters of the social side of social-ecological systems science. They charge that there is a

need to go beyond an emphasis on ‘rules’ and institutional designs that reflect logics of economic maximization, and to broaden our consideration to subjective identities and affective relationships, through gender, class and ethnicity, for example, that shed light on the role of multiple, complex, and contested rationalities in ecological decision-making processes. (483-484)

Meeting this need, though, isn’t merely a matter of producing empirically adequate explanations of social-ecological resilience and change. By attending to the normative stakes of social inquiry, they argue, researchers move “firmly out of the science of description and prediction as it is understood today and into moral and ethical terrain” (484). We need truths about subjective identities and affective relationships, in large part, because these truths register within moral and ethical deliberations. What sorts of subjects we should cultivate, and what sorts of relationships are moral and ethical, is precisely the sort of information that democratic citizens need when deliberating over the goals for, and constraints to, environmental management and policy. While we can enroll existing theories from the social and behavioral sciences to better understand our own norms, Cote and Nightingale also locate a lacuna in our normative inquiries. They argue that our conversations about what ought to be would gain tremendously from investigations of our “cultural commitments and political relations,” but that we so far do not understand how these bear on social-ecological systems or even the framing of social-ecological systems as problems. This begins to address what it would mean to “use social science methods to develop

more comprehensive evaluative tools, tools that will allow us to move toward an articulation of a particular community's sense of identity, including who the community, collectively, really wants to be" (Norton 2005, 302-303). It encourages us to see our science as presupposing values about who we are and who we want to become, and to legitimate these values through democratic deliberation.

As social-ecological systems science matures, it is gradually moving beyond its original emphasis on structuralist and functionalist ontologies that echo ecology. Social scientists are now free to study social practices in ways that recognize agents' participation in reproducing structures and performing functions. *How* we study these social practices, though, should not be bound to pre-existing ontological commitments characteristic of hermeneutic methodologies as in Fabinyi, Evans, and Foale's (2014) proposal. Nor should we choose our social science based solely on epistemic values like empirical adequacy and completeness, as recommended by Schluter and colleagues, since non-epistemic, pragmatic considerations are ineliminable from theory choice. It is Cote and Nightengale's attention to the how social sciences presuppose normative and valuational commitments that opens up social-ecological inquiry to stakeholders' non-epistemic and pragmatic concerns. Inquiry requires that we orient ourselves to an initially indeterminate situation, and our choice of a social scientific theory and its characteristic vocabulary provides much of this orientation.²³ Warranting our orientation to an indeterminate

²³ Several social-ecological systems researchers have recognized that different social scientific disciplines owe their differences to the diverse disciplinary languages that they bring to bear in describing the system. In the absence of a more pragmatist theory of inquiry, however, this insight has not led to a normative conversation about what these diverse disciplinary languages mean for describing a situation as a problem and evaluating actions as ameliorative. Fields like sociology and political science are often taken to offer rival theories of individual and collective action, and the task of a cross-disciplinary collaboration is to resolve which of these two fields offers the true theory (Lele and Norgaard 2005). On the view here, however, both theories may be true in terms of what they set forth to describe and explain, and the difference between the theories is a pragmatic difference regarding what these theories let us *do*.

situation requires that we deliberate over the value of our subjective identities and affective relationships.

Values in Coastal Social-Ecological Systems Science

In this section I want to consider how different social-ecological systems scientists make different normative and valuational assumptions, assumptions difficult to recognize and subject to critical scrutiny. Concretely, this amounts to answering the following sorts of questions: How do the various social sciences presuppose different goals for, and constraints on, inquiry? How do the various social sciences make normative assumptions about the different features of a social reality that we collectively reproduce and maintain? Ultimately, I want to support the following three interrelated claims. First, the vocabulary in which explanations are warranted reflects the disciplinary background of the social scientists in a collaboration. Second, different vocabularies make different valuational and normative assumptions about the practices worth upholding and the practices open to change. And third, the value-ladenness of social explanations has downstream and upstream implications for how social-ecological systems scientists are accountable to stakeholders' objections.

The ideal way to investigate these sorts of questions and support these three claims would be to compare how different social theories describe the same social-ecological system, and subsequently to compare the sorts of policy proposals and management strategies that followed from these descriptions. Unfortunately—and perhaps in part because we do generally assume that some social theory does truly represent the social dimensions of a given system—multiple studies of the same social-ecological systems, conducted by different collaborations with different disciplinary compositions, are rarely commissioned. In lieu of this ideal method, I instead consider the diversity of social mechanisms identified in the more narrow field of coastal

and marine social-ecological systems. Though this study summarizes studies of social-ecological systems from across the world, its attention to only coastal and marine systems allows for easier comparison of the various mechanisms available for explaining social-ecological change. These mechanisms were systematized in Ferrol-Schulte and colleagues (2013) review article of the coastal and marine social-ecological systems literature, and offer insight into how multiple mechanisms are available to truthfully describe and successfully intervene in these complex environments. I argue that differences in how these systems are described are due in large part to the different orientations of researchers wielding different, often disciplinary, languages. In other words, it was the structures of different inquiries that account for different explanations of social-ecological change; as Dewey put it, “the genius of language, reflecting a vast network of social traditions and purposes, enters quite as much as the thing told about in deciding whether what is told is a truthful representation” (1911, 103).

Like the open-access forests discussed in Chapter Three, coastal and marine environments are a paradigm ecosystem investigated by social-ecological systems scientists. These habitats are frequently subject to external shocks such as storms or pollution from human activities, and these external shocks create important test conditions for the resilience of coastal and marine social-ecological systems (Adger et al. 2005). Resilient systems are able to maintain core functions in spite of perturbations like hurricanes or disease epidemics, and social-ecological systems scientists have studied the social and ecological mechanisms through which these systems are able to adapt to and learn from these perturbations. Beyond the experimental value of coastal and marine systems, human dependence on these environments renders them especially significant on ethical grounds. At the turn of the millennium, researchers estimated that 1.2 billion people lived within 100 kilometers of a shoreline, and that number is expected to

have increased due to migration, development, and globalization (Small and Nicholls 2003, Ferrol-Schulte et al. 2013). Overexploitation of global fisheries and eutrophication due to human activities produce acute pressures that exacerbate ongoing vulnerabilities due to climate change (Ferrol-Schulte et al. 2013). Given our dependence on coastal and marine environments, researchers study ways to build and maintain these systems' resilience in order to ensure that communities can sustain their way of life in the face of disasters (Gaillard et al. 2009, Pomeroy et al. 2006).

The coastal and marine social-ecological system literature is illuminating for one additional reason: it widely relies on a framework for evaluating social-ecological systems known as the Sustainable Livelihoods Approach. The Sustainable Livelihoods Approach emerged in the mid-1980s and early-1990s as development organizations adopted a more holistic approach to food aid and security (Food and Agriculture Organization 2000). The approach reflects a concerted effort to govern natural resource challenges in ways that are aligned with the goals and values of communities that use and depend on those resources. This accords with Norton and fellow environmental pragmatists' insistence that environments be studied, described, and managed in ways that speak to stakeholder values. For our purposes, it simplifies at least one way that researchers might differ in explaining social-ecological change; we can be reasonably assured that studies of coastal and marine social-ecological systems share a similar, though context-sensitive, explanandum, even when they recommend different explanantia for how the system sustains desirable livelihoods. These studies have taken to heart the importance of conducting environmental science in value-laden vocabularies that are aligned with community values and commitments. However, they have not to this point considered the values that are at stake in explaining environment change according to different social mechanisms.

In Daniella Ferrol-Schulte and colleagues (2013) review of the coastal and marine social-ecological systems literature, social mechanisms bear much of the explanatory burden for resilience and, conversely, vulnerability. They note that these systems differ from their terrestrial counterparts in part because the social institutions that govern human-environment interactions are much more tenuous. Paradigm institutions such as resource access and ownership are complicated by the inherent lack of firm boundaries, and this has a tendency to produce contestations between local communities, private industry, and governments. This emphasis on social dimensions is paralleled by the emphases on the Sustainable Livelihoods Approach, which is prominent in this literature. The approach “emphasizes the capabilities and potentialities that exist within resource user communities and grants a necessary focus on social relations, government processes and institutions in contributing to livelihoods” (254). The authors stress that the approach is dedicated to explaining all aspects of wealth and poverty, exploring why it is that some people are poor and some ecosystems are overexploited and degraded. The goal of this research is to provide a complete picture of the mechanisms that support or impede the development of institutions that sustainably manage the use of natural resources in coastal and marine environments.

Ferrol-Schulte and colleagues expect that coastal and marine social-ecological conditions would encourage the development of institutions regulating land use and tenure, as observed in Ostrom’s studies of Swiss cowherders discussed in Chapter One. However, they note that “social, economic, and political factors have been shown to undermine rather than support bottom-up management strategies even where enough biological data and motivation exist” (2013, 254). In passages like this, we can start to elucidate the structure of more recent social-ecological systems science. Scientists can no longer assume that social institutions, such as

shared-contract arrangements or patron-client systems, will organically evolve in response to ecological conditions. Such social institutions, as long maintained by more critical social scientists, are also shaped by existing social institutions, with power and agency exerting their own pressure on the sorts of institutions that are developed and sustained. Still, the development of bottom-up management strategies, especially strategies that are decentralized and adaptive, is widely assumed as a goal of social-ecological management and the research that informs it. When these strategies are articulated through stakeholder deliberation, as is characteristic of the Sustainable Livelihoods Approach, social-ecological systems scientists are right to seek an understanding of the conditions under which these strategies can succeed. The social dimensions of this research are still closely tethered to the ecological conditions of the systems, though. Social, economic, and political factors are understood in light of their significance for achieving management strategies that foster resilience and sustain livelihoods; some broadly social factors support resilience, while others are taken to be obstacles.

Four types of social mechanisms are diagnosed by Ferrol-Schulte and colleagues: stakeholder conflicts, corruption, lack of local capital assets and capacity, and weak institutional structures (255-256). Stakeholder conflicts occur when ecological, social, and political boundaries are blurred, and multiple stakeholders claim some right over natural resources such as coastal fisheries. Corruption, when perceived by resource users, can discourage compliance with environmental regulations; the authors note that even small-scale bribery can compromise the authority of environmental managers and jeopardize the resilience of social-ecological systems. Lack of local capital assets and capacity can impede environmental management even when stakeholders agree over the rightful use of resources and trust authorities to enforce environmental regulations; if communities lack information about social-ecological dynamics, or

lack the finances and resources to act on the knowledge they do have, then resource use can become unsustainable and the overall social-ecological system can become vulnerable. Finally, weak institutional structures can complicate efforts to manage social-ecological systems even when stakeholders and managers are otherwise capable. Governments and funding agencies can withhold power or resources from local actors who are in a better position to adaptively manage the social-ecological system. Decision-makers further removed from the local context may be suspicious of the uncertain terms of adaptive management, but the end result is less investment in the style of management that has a demonstrated record of achieving sustainability and resilience.

Ferrol-Schulte and colleagues' study provides rich ground for thinking about the valuational and normative commitments at stake in social-ecological systems science. First, I want to marshal some evidence that the terms in which social dimensions are described often reflects the disciplinary training of the social scientists participating in an interdisciplinary collaboration. In other words, recruiting a scientist with particular disciplinary expertise will produce a disciplinary description that makes particular normative and valuational assumptions. Ferrol-Schulte and colleagues' four types of mechanisms are broken down into more specific examples, and it is at this more specific level that we can see a link between the researchers' orientation and the mechanism diagnosed. Many of these studies were conducted by interdisciplinary collaborations primarily composed of fisheries scientists but also including a few social scientists. Of interest here is whether the disciplinary training of the few social scientists on a study makes a difference for how the study explains resilience or vulnerability; for any particular situation, we might expect a range of viable social explanations.

Under stakeholder conflict, Ferrol-Schulte and colleagues include “conflicts between socio-political and ethno-linguistic groups over resource scarcity in Indonesia,” a mechanism not unsurprisingly studied by a team including an anthropologist and rural sociologist (Reichel, Fromming, and Glaser 2009). Here different occupational and ethnic groups contested the use of resources of a lagoon undergoing a transformation from fisheries to wetland agriculture. Under corruption, the reviewers include “corruptibility of authorities undermines willingness to comply with fisheries regulations,” here investigated by a political scientist (Sundstrom 2012). Under the heading of “lack of local capital assets and capacity,” it was a geographer who explored how “locality, or [the] unique set of geographical conditions of a place at a particular time” bears on the vulnerability of a social-ecological system and a team including a development scholar who explored how “lack of skill, labor, and/or capital prevents livelihood diversification for risk management in East Africa” (Eriksen, Brown, and Kelly 2005, Myers 2002). An anthropologist and natural resource economist participated in an interdisciplinary study that explained how the “failure to include local stakeholders in MPA [marine protected area] design and implementation leads to socio-economic shortcomings” (Ferse et al., 2010), one example of Ferrol-Schulte and colleagues’ broader mechanism of weak institutional structure.

This quick review of authorship is merely suggestive, and it is complicated by the prevalence of authors with interdisciplinary training that is common among social-ecological systems scientists. Still, if you choose a mechanism that harkens to a particular social theory, it is likely that the researcher who discovered the significance of that mechanism is a student of that particular social theory. This is not wishful thinking on the part of these researchers; social-ecological systems really do admit of myriad theoretically-laden descriptions. Anthropologists who describe a system in terms of conflict between ethno-linguistic groups provide an

understanding of how such social relations bear on social-ecological vulnerability; political scientists who describe a system in terms of the corruptibility of authorities provide a different understanding. The task is figuring out which of these understandings is appropriate given the goals and values of stakeholders to the inquiry.

In order to warrant reliance on a particular social scientific discipline, it is important to recognize *how* different scientific vocabularies bear on our non-epistemic, pragmatic values; such an analysis supports this section's second claim that different social theories presuppose different normative and valuational assumptions. These presuppositions are built into social theories that take for granted a social reality in which practices regularly produce social outcomes. This "taking for granted" is not something that social-ecological systems scientists could easily avoid; explaining environmental change in terms of social institutions necessarily adopts a "grammar" (in the sense discussed in Chapter Three) that orients us to the world as a set of rule-governed, intentional practices. Take for instance the concept of "corruption", which is already meaningful within ordinary conversation—and, incidentally, already meaningful within moral and ethical deliberation. We can readily analyze the concept of "corruption" like we deconstructed the concept of "sharing." For instance, no behavior counts as an instance of "small-scale bribery" absent background social norms that attach different meanings to that behavior than, for instance, tipping a regulator out of appreciation, or purchasing quotas to rightfully fish a particular habitat. In studies that explain social-ecological vulnerability by attending to the prevalence of corruption, these social norms are taken for granted. Insofar as the community does uphold these norms and maintains the assumed social reality, social-ecological systems scientists can truthfully describe the system in terms of corruption or legitimacy. Some communities may uphold different norms where the behavior of paying regulators is not an

instance of bribery; they might, for instance, believe that paying regulators for access is perfectly fair, or that paying regulators produces more efficient use of the fishery. In such cases, it is simply not true that bribery is causing social-ecological vulnerability, because bribery is not part of the social reality of that system.

This simplified grammatical investigation is meant to highlight the structure of social scientific inquiry, because this structure can be much more difficult to trace in concrete situations. The grammar of concepts such as “stakeholder conflict,” “lack of local capital assets and capacity,” and “weak institutional structure,” is further removed from day-to-day conversation, and this makes it more difficult to remember what must be true in order to appropriately apply these concepts. Take “lack of capital assets and capacity”. More generally, this notion gets disaggregated into household factors related to market access, migration, and technology. More specifically, it is disaggregated into an array of conditions such as low social cohesion and asset inequalities in Zanzibar; lack of skill, labor, or livelihood diversification in East Africa; low perceptions of assets on Rodrigues Island; lack of alternative livelihoods in Bangladesh, (Ferrol-Schulte, 256). For any of these more specific conditions to explain social-ecological degradation, the following sorts of claims must be true: social cohesion can increase in Zanzibar and this would be followed by better resource management; small-scale fishers in East Africa could acquire more diverse skills, find employment for those skills, and sustain their livelihoods in the face of external perturbations; community members on Rodrigues Island can more accurately assess their and others’ capital assets, and these more accurate perceptions would lead to more sustainable behavior; and so on.

Valuational commitments, evident in notions such as “better resource management,” are *desirable* features of social explanations; if we explained social-ecological systems in terms

unrelated to our values, then our social-ecological findings would provide no guidance for how we should manage those systems and what sorts of policies we should prefer. This is a main insight from Norton's theory of adaptive ecosystem management. Normative commitments are undertheorized in Norton's theory, but normative commitments are just as essential to the development of social theories that intervene in the production and maintenance of social institutions and norms. Acquiring diverse skills, or finding employment for such skills, are possible only within particular social systems where community members uphold the norms that enable these actions (Searle 1995).

These normative commitments are laden with non-epistemic, social values, and such values ought to be explicated and subject to democratic deliberation. Citing low capital assets and capacities as a condition for social-ecological vulnerability amounts to a claim that increasing capital assets and capacity is one mechanism for buttressing the resilience of the system, and that decreasing capital assets and capacity would exacerbate the system's vulnerability.²⁴ This is a pragmatic claim about the kind of social-ecological system we can produce. Any number of these pragmatic claims are true of a particular social-ecological system—following Cote and Nightingale, we should remember that the choice among social explanations is, in part, a reflection of our values, which requires a moral and ethical deliberation about what we cherish or admonish (2012, 484). We could be confident that increasing capital assets and capacity would increase resilience while also being confident that strengthening the institutional structure would also help, or that fighting corruption would help, or mitigating stakeholder conflict. The right choice is not simply a matter of locating the true mechanism, or

²⁴ Though I don't pursue this framing here, these pragmatic features also come to light when we consider that social scientific explanations hold *ceteris paribus*, or other things equal. Harold Kincaid (2011) provides an excellent overview of *ceteris paribus* in the social sciences, demonstrating that social sciences share this with many if not all of the physical and biological sciences, and that this hardly disqualifies social science from offering explanations.

even of locating the mechanism to which the overall system is most sensitive; it is a much more involved consideration of the consequences of various interventions and their alignment with our non-epistemic, social values.

When critical social scientists insist that researchers attend to the social significance of institutions, they are in part directing our attention to the broader consequences of these various interventions. Fighting corruption is not merely a mechanism for more sustainable extraction of natural resources; it is also, perhaps, a way of instilling confidence in shared institutions, or a way of combatting pernicious social hierarchies. Increasing capital assets and capacity may provide workers with more opportunities on the labor market, and this might lead to better nourished diets, but it might also compromise traditional harvesting practices and the social systems that they sustain. These are all consequences that we should consider when warranting pragmatic claims. It is not just the statement of facts that is at stake when assessing descriptions of social-ecological systems; it is also the vocabulary in which the description is issued—“reflecting a vast network of social traditions and purposes”—that is up for debate.

Because social explanations are laden with valuational and normative commitments, social-ecological systems scientists are accountable to stakeholders’ objections both downstream and upstream from experimentation. Downstream, it should be reasonably clear that stakeholders can object to claims about social mechanisms because they do not accord with those stakeholders non-epistemic values. Social-ecological researchers might demonstrate beyond doubt that a particular intervention would produce resilience, and yet stakeholders can still deny that this is an appropriate way to describe the system, because they may reject the assumptions that these researchers made about which social practices were negotiable and which weren’t. For instance, researchers may claim that a system lacks resilience because local regulators have too little

leeway to implement adaptive management schemes, yet stakeholders could object that the strict accountability of local regulators to regional decision-makers is a legitimate constraint on environmental management. This is not an objection to the truth of the scientists' statements, at least not the truth of whether the mechanism cited would produce the consequences predicted. It is an objection to whether scientists have cited the appropriate means, among many, that would produce the desired effect, and on this view, scientists are accountable to such pragmatic concerns.

Attention to the pragmatic character of explanation should help to ward off the following sort of philosophical objection: Stakeholders are not reasonably objecting to the description of the system, even if they are well within their rights to object to management practices that are prescribed based on the description. In other words, scientists might insist that stakeholders should only object to the policies and management strategies that are based on their science, but they should not object to the science itself. The trouble with this sort of philosophical objection is that it ignores the pragmatic character of vocabularies, the sense in which vocabularies represent the world in the service of our navigating that world toward particular goals. Philosophers, and researchers who share these philosophers' intuitions, want to assert a fact-value dichotomy according to which scientists first describe the world and then policymakers decide what to do about it. But there are no value-free or even value-neutral ways of just "telling it how it is"; to describe anything is to situate it within a system of significance according to which we draw inferences and warrant beliefs and actions. Another way of arriving at the futility of this philosophical objection is to imagine the many ways that we might describe the system scientifically, each of which able to locate truths in its own terms. Now, if researchers preface their description as simply one of a large number of true descriptions, they may seem off the

hook for implicating social values, but at this point it's no longer clear that the description means anything at all. Scientists should want their descriptions to mean something for value-laden management and policy, because otherwise, their descriptions cannot provide reasons for collective social action.

Upstream, then, we would want to ensure that the vocabularies in which we describe a social-ecological system, and locate mechanisms for intervening in that system, reflect our values as a community. For environmental pragmatists, this has amounted to negotiating the terms in which we describe the objective features of an environment so that the consequences of alternative policies are immediately meaningful for democratic deliberation. Here, though, we want a vocabulary that describes the intersubjective features of the social dimensions of social-ecological systems. We want to understand these systems in ways that reflect on our “cultural commitments and political relations” and that “broaden our consideration to subjective identities and affective relationships” (Cote and Nightingale 2012, 484). Terms like ‘corruption’, which do reflect on our political relations, provide part of a vocabulary that inherits the ordinary significance of corruption from our existing moral and ethical language. Terms like ‘stakeholder conflict’, ‘lack of capital assets and capacities’, and ‘weak institutional structures’ require that we integrate these notions into our existing language. Fortunately, the agentic or dispositional grammar of these terms, through which they describe characteristics of people and how they act, does offer some hope for weaving them into moral deliberation where we are accustomed to discussing such matters. Upstream, when we are conceptualizing a study before investigation, our deliberations are not constrained to the vocabularies already operative in the inquiry of social-ecological systems researchers. When Cote and Nightingale call for new research that explores the role of cultural commitments and political relations, they correctly open the door for

new inquiries that describe the intersubjective features of social-ecological systems in ways that already matter to us. By describing social mechanisms in these terms, social-ecological systems scientists can provide us with an understanding of these systems that we need in order to legitimately intervene in “people management.”

A quick summary of this section is in order. The coastal and marine social-ecological systems literature demonstrates the ways that scientists with different disciplinary training will locate different mechanisms relevant to the dynamics of social-ecological systems. These disciplinary diagnoses are hardly rivals in the sense that only one social theory provides the true representation of the system. Rather, different social theories differ with respect to the valuational and normative commitments that they inevitably make by deploying a grammar attentive to the rule-governed, intentional structure of human action. Explaining environmental change in terms of particular social institutions involves the pragmatic decision to understand these institutions as means to effecting valued ends. Scientists who make these decisions are accountable to objections both downstream and upstream of investigation; in both contexts, stakeholders can reasonably assert that the values that inform the choice of means or ends are not their values, and that scientists should locate different means for producing the same (or different) ends.

Conclusion, Plus Three Independent Factors in Moral Deliberation

When social-ecological systems scientists first attempted to reinvent a social science of people management, they theorized social dynamics in structuralist and functionalist terms that echoed the ontology of ecology. More critical social scientists pointed out that these efforts succumbed to the previous folly of reducing social dynamics to ecological dynamics, and that this reduction missed the uniquely social significances of human behavior and institutions. These

critics were split, though, on exactly how to integrate contemporary social theories within social-ecological systems science. More hermeneutically inclined researchers maintained that fields like social anthropology and political ecology provided a truer representation of social ontology, and therefore that we must describe social dynamics in ways that attend to power and agency. More Galilean researchers instead maintained that purely extensional vocabularies were on the right track, and that social scientists should not deviate from the epistemological convictions of their ecological counterparts.

On a pragmatist theory of inquiry, we should give up the idea that our choice among social sciences amounts to finding the theory that best mirrors reality, or finding the theory that is most predictive given the social context. Rather, we should orient ourselves to the social dimensions of environmental problems through a reflection on who we are and who we want to become. These non-epistemic, social values provide structure to inquiry by subjecting particular practices to doubt, so that we can explore how changing these practices might help us realize our social and environmental goals. When social-ecological systems scientists investigate the role of corruption or ethno-linguistic conflict in producing resilience or vulnerability, they allow us to know how changes in our practices will shape the environment. In order to fully warrant these descriptions, however, we must also attend to how changing our practices will shape our identities and our communities. The mere achievement of social-ecological resilience is not sufficient justification for reconstructing our social practices.

I want to close with a parting remark about the vocabulary in which we might conduct legitimate social scientific inquiry. In the aforementioned upstream deliberations, a priority was placed on describing social practices in ways that immediately register within ethical and moral deliberations about who we are and who we want to be. This chimes with Cote and Nightingale's

call for social-ecological studies of how our cultural commitments and political relations bear on environmental change. What is notable about these sorts of variables is the sense in which a conversation about cultural commitments is in part a conversation about rights and responsibilities, while a conversation about political relations is a reflection on individuals' character and their virtues and vices. This reflects an interesting turn in the ways that environmental pragmatists have encouraged scientists to integrate facts and values; by and large, philosophers like Norton have mobilized a consequentialist ethical theory where we compare the costs and benefits of policies and management strategies (2005).

Environmental pragmatists do recognize the importance of deontological and virtue-theoretical approaches to ethics—Norton stresses that environment science must be accountable to community commitments, and Thompson stresses the virtues that constitute sustainable agrarian societies (Norton 2005, Thompson 2010). Dampening the emphasis on costs and benefits, and amplifying the emphasis on rights and virtues, opens stakeholder deliberation to more vocabularies for exploring the moral and ethical significance of our environmental decisions. Following John Dewey's 1930 essay, "Three Independent Factors in Morals," I would advance that drawing on all three of these ethical theories improves our moral and ethical deliberation and the inquiries legitimated through deliberation. As Dewey writes,

good and right [he later adds virtue] have different origins, they flow from independent springs, so that neither of the two can derive from the other, so that desire and duty have equally legitimate bases and the force they exercise in different directions is what makes moral decision a real problem, what gives ethical judgment and moral tact their vitality.

(316)

It is this vitality that sustains communities' growth (Dewey 1916). The goal of this chapter is not to resolve which ethical theory takes priority over others, so that we might enter deliberations with the conviction that virtuous social practices will justify undesirable social-ecological consequences, or that desirable social-ecological consequences will justify skirting our duties and responsibilities to one another. Rather, the goal is to create space for interlocutors to appeal to any of these three independent factors in deliberating over the terms of social-ecological inquiry. So far, techniques for facilitating stakeholder deliberation have focused on the ecological costs and benefits of various management options. As we begin to consider the various social mechanisms to implement these management options, we should also ask whether these mechanisms exemplify our virtues and uphold our moral commitments.

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CHAPTER 5

INTERDISCIPLINARY INTEGRATION AS ETHICAL INQUIRY

Abstract: After summarizing the previous chapters of the dissertation, I consider one remaining question that confronts the integration of facts and values in social-ecological systems science: the question of interdisciplinarity. Theories of interdisciplinarity revolve around their own challenge of integration, though in this case the task is one of integrating different academic disciplines. Common approaches to interdisciplinarity make use of linguistic metaphors like "pidgins" or "Creoles" to make sense of integration; I argue that these metaphors are on the right track when they stress the pragmatic character of meaning-as-use, but on the wrong track when they stress the semantic character of meaning-as-reference. Drawing from Robert Brandom's and Wilfrid Sellars' analysis of the inseparability of description and explanation, I show that the meaning-as-use account of language is the better basis for understanding interdisciplinarity. Meaning-as-use foregrounds the pragmatic character of inquiry, raising questions about what we do value and what we should value. I take these to be ethical questions that have so far been underappreciated in the theory and practice of interdisciplinary integration. This ethical view of interdisciplinarity provides an alternative understanding of integration: an interdisciplinary response to a problem is integrated insofar as it answers questions relevant to what we value, and provides an account of the problem that guides us in weighing tradeoffs among our values. I close by extending Philip Kitcher's ideal of well-ordered science to the practice of interdisciplinary collaboration, arguing that interdisciplinary integration should work outward from the disciplinary vocabulary that speaks to what we value the most. Beginning with a disciplinary description that attends to only some of what's at stake in a complex problem, interdisciplinary integration proceeds by considering in turn the remaining problems as described in the vocabularies of additional academic disciplines.

One Last Occasion for Philosophy

In the opening passages of this project, I suggested that social-ecological systems science represented an occasion for philosophical investigation (Thompson 2002). Scientists from the field confronted seemingly innocuous questions—which social scientists should we recruit for our interdisciplinary collaboration? How should we coordinate diverse and sometimes rival explanations from different social sciences?—but the trouble with these questions was that it was wholly unclear what should count as an answer. Figuring out what counts as an answer to these

questions requires a philosophical inquiry into what these questions mean. And a question about meaning, on the pragmatic account here recommended, is a question about how the practices (especially the discursive practices) of the social sciences work to orient and reorient us to the social and biophysical environments within which we find ourselves.

It's important to remember that all philosophical occasions must come to a close. Philosophers are commonly bewitched by a host of confusions that often extend philosophical inquiry beyond the limits of its coherence. With Ludwig Wittgenstein, we must appreciate that "The real discovery is the one which enables me to stop doing philosophy when I want to. The one that gives philosophy peace, so that it is no longer tormented by questions which bring *itself* into question" (1953, §133). Part of doing occasional philosophy is recognizing when the time for philosophy ends and the time for other forms of inquiry begins. Here I want to briefly summarize our investigation in order to locate the instants where philosophical inquiry gives us momentary peace and where other forms of inquiry find *their* occasion. This summary stresses the symmetry between Bryan G. Norton's account of environmental pragmatism, with which we began our journey, and my own account, but it highlights the departures from this tradition that navigate the unique geography of social-ecological systems science. The summary uncovers one last array of philosophical questions, though, that revolve around the challenges of interdisciplinary inquiry. Conceptual confusions continue to afflict the theory and practice of interdisciplinarity, in which we find one last occasion for philosophy.

Norton's environmental pragmatism arose in response to a particular pernicious problem for the practice of environmental management and policymaking (2005). Well-intentioned managers and policymakers wanted to base their prescriptions on objective scientific evidence. Such objective evidence supposedly reported "just the facts," barring ecologists from employing

any terms in their vocabulary, such as ‘ecosystem health’ or ‘integrity’, that are imbued with values. Economists would provide value-neutral appraisals of the various ecological consequences of available management and policy alternatives; these appraisals owed their neutrality to the systematic methods that economists deployed to consistently price environmental goods and services as valued by a hypothetical market. Norton’s insight is that this procedure, deeply invested in a dichotomy between facts and values, hardly delivers on its promise for value freedom or value neutrality. Indeed, unless we describe the ecological consequences of our management and policy alternatives in a vocabulary that *integrates* facts and values, the descriptions provided by our best scientists will be at best partial, and at worst irrelevant, for the decisions that we make collectively. The only way to infer management and policy prescriptions from the environmental sciences is to ensure that the language in which such science is conducted is a language that already means something for how we should act toward one another and the habitats we share. Instead of a procedure that wrests values from scientific vocabularies, we require a procedure that imbues these vocabularies with the values of an inclusive and deliberative community, one which legitimates rather than expunges the values at stake.

This version of environmental pragmatism is right, as far as it goes. But the rise of social-ecological systems science reflects a lacuna in how folks like Norton have articulated such a procedure. Though few environmental philosophers are guilty of forgetting that human beings are embedded in their environment, the mainstream version of environmental pragmatism emphasizes the language in which we describe the environment external to us (Norton 2015). The mark of social-ecological systems science is to stress that our social practices and institutions are in fact internal to environmental systems and to therefore demand a science of

people management that understands the mechanisms governing human-environment interactions. As social-ecological system science has matured, it has drawn on a wider diversity of social scientific theories to discover the range of mechanisms by which human institutions cause environmental changes and by environmental conditions cause changes in human institutions. Social-ecological systems scientists, by and large, assume a fact/value dichotomy according to which their job is complete when they provide an empirically adequate explanation of these mechanisms. They find themselves in precisely the situation about which Norton warns, where they strive for a value-neutral vocabulary for describing social practices and institutions, and assume that policymakers can infer policy prescriptions from those descriptions.

The philosophical investigations of the middle chapters of this book chart several arguments that demonstrate how we must warrant descriptions of social practices and institutions. Following Dewey's account of organism-environment interaction, I argued that inquiry is fundamentally a form of problem-solving and that such problem-solving is structured by the vocabularies in which inquiry is conducted. The ultimate aim of inquiry is explanation, according to which we cite causes of a problem. What Dewey shows, though, is that citing causes is an essentially pragmatic endeavor rather than a clear cut ontological discovery. Every explanandum is the effect of myriad explananda, and the task of locating causes is a task of assigning responsibility to particular explananda, according to which they can be taken as the basis of intelligent interventions. Dewey's discussion is meant as a clarification of what we *mean* by explanation—though we are captivated by certain ontological pictures of causality like that of one billiard ball “causing” another to move, our actual practices of explanation show a much more practical activity. A metaphysically mistaken assumption of a world comprising causes and consequences yields to an open deliberation about our values and the interventions warranted

according to our values. Correcting this mistake gives us momentary peace, and initiates the empirical inquiry of deliberative valuation.

This pragmatic account of explanation informs the third and fourth chapters. In the third chapter, I caution against a philosophical conviction that human beings are simply not the sort of thing that admits of mechanistic explanation. This conviction is central to hermeneutic approaches to the social sciences, which champion the ways that human beings create meaning in non-mechanistic ways. When explanation and mechanism are detached from ontological pictures of causality, though, hermeneutic approaches cannot find justification in a rival ontological picture that they take to be better warranted by their empirical success. They instead find their justification in the priority that we typically place on explaining human action at the grammatically complex level of norms. This alone does not resolve which social sciences should inform a “science of people management,” however, as we have available an array of social theories that recommend different interventions according to the vocabularies with which they structure inquiry. Deliberations over how to describe the social dimensions of a social-ecological system, for instance in terms of corruption or in terms of local capital assets, is a pragmatic decision about whether we would prefer to combat corruption or bolster local capital assets. This in turn requires a broader conversation about the social and ecological consequences of either intervention, and an even broader conversation about our cultural commitments, political relations, and subjective identities (Cote and Nightingale 2012).²⁵

The trouble with this account is that it suggests that we settle on *one* social science, one vocabulary to provide structure to our social-ecological inquiries. This might give the impression

²⁵ It is important to “stop doing philosophy” once we understand the meaning of social explanation—philosophy cannot resolve, in advance of deliberation and through reflection on the meaning of candidate social theories, the right vocabulary for social explanation.

that what we mistakenly took to be ontologically rival social theories remain rivals, but that their rivalry is really owed to their different valuational assumptions about who we are and who we want to become. This, though, neglects the obvious possibility that we can, and perhaps ought to, draw from multiple disciplines in the social sciences, and that we don't so much as choose one discipline as we weave together the best insights from each. This possibility is, of course, the possibility of an *interdisciplinary* approach to the social dimensions of social-ecological systems science. Interdisciplinarity distinguishes itself from the more multidisciplinary instincts of the middle chapters by promising its own brand of integration; whereas environmental pragmatism keys on the integration of facts and values, interdisciplinary theory and practice attends to the integration of disciplinary perspectives or languages.

The integration of facts and values offered our first occasion for philosophy, and the integration of disciplinary perspectives or languages offers our last occasion. Here I maintain that scholars and practitioners of interdisciplinarity make a philosophical error in grounding integration in either epistemology or ontology. Such interdisciplinarians assume that disciplinary perspectives or languages stand in a correspondence relationship with a world that is independent of our projects and purposes. Guided by this assumption, the seductive allure of interdisciplinary integration is a comprehensive representation of reality. I argue that this assumption is misguided and that careful attention to the way that we use languages reveals the ineliminable pragmatic character of description and explanation. Attention to this pragmatic character shifts our focus from representation and reality to the challenge of prioritizing our diverse projects and purposes. I take such prioritization to be first and foremost an ethical deliberation about which values we hold dearest, and I recommend Philip Kitcher's proposal for well-ordered science as a procedure for organizing disciplinary contributions to an interdisciplinary project.

Making Sense of the Metaphors of Interdisciplinary Integration

Common approaches to interdisciplinarity make use of linguistic metaphors like "pidgins" or "Creoles" to make sense of integration; in this section, I argue that these metaphors are on the right track when they stress the pragmatic character of meaning-as-use, but on the wrong track when they stress the semantic character of meaning-as-reference. I consider Rick Szostak's recommendation for breaking complex concepts down to more basic concepts, which he believes will help collaborators know what one another mean, but which I argue will actually impede mutual understanding. Following Brandom and Sellars, I argue that understanding what one another mean is a matter of understanding the rules that govern a concept's use. Interdisciplinary collaborations seeking to learn one another's language, then, must attend to the way that concepts are used—not just in what Sellars' calls "labeling," but in the rich practice of description, in the service of explanation, and ultimately in the service of action (pragmatics).

Interdisciplinary research is commonly contrasted with merely multidisciplinary research (Klein 2010). Like social-ecological systems science, theories of interdisciplinarity and multidisciplinary find common motivation in the need to solve complex problems that go beyond the scope of a single research expertise. In multidisciplinary research, specialists from various disciplines provide solutions rooted in their own disciplines for addressing the problem (Holbrook 2012). Interdisciplinary research is marked by the effort to combine disciplinary contributions in a way that weaves together the best insights of each participating discipline. Whereas multidisciplinary projects might bring to bear an anthropological understanding of cultural identity along with an economic understanding of property rights institutions, the interdisciplinary project might strive to understand the relationships between cultural identity and property rights institutions. This is clearly a laudable goal if we are going to follow the

recommendations from Chapter Four and anticipate the interactions between myriad components of the social system.

Prominent theorists of interdisciplinarity—William Newell, Allen Repko, Julie Thompson Klein, and others—take interdisciplinary integration to be the key challenge and goal of interdisciplinary collaboration (Klein 2012, Repko 2007, Newell 2001, O’Rourke, Crowley, and Gonnerman 2016). As careful readers of the literature point out, however, the enthusiasm for integration in the interdisciplinary studies literatures has produced neither clarity nor agreement about how integration is accomplished or even what integration *is* (O’Rourke, Crowley, and Gonnerman 2016). In 2001, Newell lamented that “No one I have talked to or read (including my own writings) has been able to explain clearly how to integrate disciplinary insights into a comprehensive understanding. We are not even clear on exactly what is *meant* by integration” (18). Repko echoes that “the lack of clarity on precisely *what* to integrate and *how* to integrate” has been the “Achilles’ heel of interdisciplinarity” (2007, 7). Nearly a decade after these proclamations, theorists and practitioners still debate the meaning of interdisciplinary integration.

Interdisciplinarity theorists have made extensive use of metaphor to provide guidance about what integration means. As Veronica Boix Mansilla chronicles, “A striking array of metaphors have been deployed to describe the nature of interdisciplinary intellectual activity—from working at ‘crossroads’ and in ‘trading zones’ to engaging ‘boundary objects’ and ‘bridges’” (2010, 289). Boix Mansilla laments, though, that these metaphors have failed to inform systematic accounts of the theory and practice of interdisciplinary research. Of particular interest here, a particular suite of metaphors are widespread in discussions of interdisciplinarity: metaphors that liken integration to the learning or creating a new language, such as a pidgin or Creole (Klein 1996). A pidgin or Creole is a language developed by speakers of different

languages to facilitate communication; the new language is a hodgepodge of neologisms and crucial terms or phrases that suffice to coordinate exchange between these different cultures.

Given my attention to the way that language (or the smaller unit, vocabularies) structure inquiry, I take these linguistic metaphors to be on the right track toward understanding how different disciplinary perspectives *could* inform an interdisciplinary project. And it is definitely the case that linguistic metaphors provide a dominant starting point for theorizing interdisciplinary integration in this literature. Newell asserts that, “since every discipline has its own vocabulary expressed as concepts, it is sometimes necessary for the interdisciplinarian to create a common vocabulary” (2008, 284). David Stone writes that for the dominant, epistemological approaches to interdisciplinarity, “the central barrier to effective interdisciplinary collaboration boils down to language, to our inability to communicate concepts, theories, and methods across disciplines in interdisciplinary contexts” (2013, 87). Commonly, this inability is cashed out as the inability of collaborators to know what one another *mean* by the terms that they use; sometimes, collaborators simply have no idea what a specialized term or piece of jargon means, but more worrisomely, collaborators often mean different things by the same term. Miscommunication abounds, and interdisciplinary collaborators face an uphill battle in piecing together a comprehensive understanding of the complex problem.

An example here is helpful. One characteristic recommendation for forging a common vocabulary is to break complex concepts into more basic concepts; the idea is that the meaning of complex concepts is difficult to trace, whereas the meaning of basic concepts is ordinarily shared among collaborators. Rick Szostak is one advocate among interdisciplinary theorists for analyzing complex concepts into basic concepts “whose meanings are fairly clear”, and he offers examples of collaborators using the same word in different ways (2013, 41). In one example,

economists attach a fairly rigid meaning to a term like “investment” while the economists’ collaborators interpret investment in a more ordinary, non-technical sense. In another example, various researchers assume different definitions for a complex concept such as globalization. In these situations, according to Szostak, “the key lies in breaking down complex concepts—those that lend themselves to different interpretations across disciplines (or cultures)—into basic concepts that can be understood similarly across disciplines” (35). The goal of this analysis is to separate ideological, methodological, and theoretical assumptions from what Szostak takes to be the *actual meaning* of these terms, which is the relationship between the term and states of affairs to which it refers (Szostak 2016). The fear among interdisciplinarians is that these assumptions are often times subjective, a vestige of the interpretive lens adorned by researchers who train in a particular academic discipline. On this view, real knowledge is knowledge of the facts that exist independently of our lenses, or conceptual frameworks. The key to interdisciplinary integration is to shed our disciplinary perspectives, which helped us know where to look for these facts, but which now color our representation of them.

It should be clear that these wide-ranging epistemological and metaphysical commitments are fairly standard in contemporary society, shared by Norton’s colleagues at the EPA and the social-ecological systems scientists surveyed throughout the book. I want to focus on a particular commitment: the idea that meaning is a matter of reference, or what I’ll call the meaning-as-reference account of language. In opposition to the meaning-as-reference account of language, I’ll recommend the meaning-as-use account.

The meaning-as-use account positions itself against the meaning-as-reference account by asking the following question: What would a person have to *do* in order to count as understanding the meaning of a term? On the meaning-as-reference account of language,

understanding the meaning of a term amounts to being able to label the features of the world to which the term applies. For instance, knowing what *globalization* means to the economist on an interdisciplinary team is understanding the set of conditions under which she applies the term: we might think of conditions such as international trade agreements (Szostak's main example is the flow of foreign investments into the globalizing nation), technology and information transfers, and transnational corporations. Per Szostak, if breaking globalization down into these sorts of parts does not expedite conversation, then these parts should be broken down into even more basic concepts; in especially difficult cases, interdisciplinary collaborators may need to point to a feature and announce "*this* is what I mean."

On the meaning-as-use account of language, this sort of definition doesn't do justice to what we are up to when we describe the world. The trouble is that the meaning-as-reference account of language takes its cue from the idea of labeling, where labels provide a name for the objects in experience. There's a world of difference, though, between labeling and describing, and it is describing but not labeling that allows us to develop explanations. As Wilfrid Sellars reminds,

Although describing and explaining (predicting, retrodicting, understanding) are *distinguishable*, they are also, in an important sense, *inseparable*. It is only because the expressions in terms of which we describe objects, even such basic expressions as words for perceptible characteristics of molar objects, locate these objects in a space of implications, that they describe at all, rather than merely label. (1957, §108)

We can think of mere labeling like we think of naming a pet. I have a pet cat with the eminently common name, "Luna." As friends and colleagues visit, they learn the cat's name, and (for most) become adept labelers of this cat, referring to her reliably by her name "Luna." What

distinguishes “Luna” as a mere label, rather than a description, is that no implications follow from her being named “Luna.” Descriptions of Luna include her being a pet, and more broadly domesticated, or her being a cat, or more broadly a mammal, or even more broadly an animal. Falling under a description, though, does locate Luna in a space of implications. We can be reasonably assured that, as a pet, she is looked after and doted over, as domesticated, she lives alongside human beings, as a mammal, she would give birth to a litter of kitten that would nurse, and so on. (As cat owners are quick to point out, it is not clear that we can safely infer anything from something being a cat.)

Now, how does this differ from the meaning-as-reference account of language? The contrast with Szostak’s extensional version of the account of meaning is especially pronounced. On his version of the account, the meaning of ‘cat’, or of ‘pet’ or of ‘mammal’, is the set of objects in the world to which that word, understood as a label, refers. For a purely extensional definition, the sorts of implications that we draw from something falling under a category are not constitutive of (or, as Sellars says, *inseparable* from) the meaning of the category. Szostak groups these sorts of implications under the ideological, methodological, and theoretical connotations of concepts. His recommendation for interdisciplinary practice is for collaborators to set aside those connotations and get to the scientific business of discovering which objects exist as part of a given situation. When they disagree about whether to apply the term “globalization” to such a situation, they should break it down into terms where they can agree, and then apply these more basic concepts.

Though this recommendation is suggestive, Sellars and fellow pragmatists like Robert Brandom would deny that this sort of labeling counts as understanding the concepts in question (Brandom 2015). Their critique is slightly different than the one founded on the naturalism

discussed in Chapter Two, where we take language and meaning to be functional adaptations of organisms, consonant with a metaphysics of organism-environment interaction and problem solving. Instead these analytic pragmatists point out that, according to the empiricist tradition where the meaning-as-reference account of language is at home, we must be able to verify that collaborators understand one another through some sort of observations. And following folks like Quine, whose arguments I won't rehearse here, they underscore the difficulty of verifying that understanding on the basis of an individual uttering a word in the presence of that word's supposed object. Instead, in order to verify that another individual has learned the meaning of a term, one has to observe the *use* of that term in a variety of situations. This echoes the grammatical remarks from Chapters Three and Four; there are rules that govern the use of a concept, and understanding the meaning of a concept involves observing (as Dewey puns, in both senses of the word "observing") those social conventions.

The idea of rules governing the use of a concept provides a helpful way to think about Sellars' notion of a space of implications. Things that count as cats would, by rule, reproduce by giving live birth to a litter of kittens and nursing them when young; if an organism that looks like a cat nonetheless builds a nest and lays a clutch of eggs, it will be undeserving of the description 'cat'. But the cat example is much too simplistic to locate the failing of someone like Szostak's recommendation for interdisciplinary practice. If two collaborators disagree about the application of the term "globalization," and break it down into more basic concepts, they are no closer to understanding one another. Szostak prefers more basic concepts because such concepts more easily garner agreement (this is why they are considered more basic), but what is at stake is not agreement about the basic concepts, but agreement about which basic concepts the complex concepts breaks down into. For if the development economist tries to teach an anthropologist the

meaning of globalization by breaking it down into international trade agreements, technology and information transfers, and transnational corporations, part of understanding the economic meaning of these supposedly more basic concepts is to infer, on the basis of these concepts truthfully applying, that globalization is in full swing. The anthropologist simply does not know what the economist means by these supposedly more basic concepts if they do not locate these descriptions in the same space of implications. New speakers of economics need to know that, if a nation enters into international trade agreements, then that nation is globalizing, or if transnational corporations increase their presence in a nation, then the nation is globalizing, and so on. Shedding such theoretical connotations of contested concepts does not make those concepts easier to learn; it makes them impossible to understand.²⁶

I believe this suffices to discourage an account of interdisciplinary integration that invests heavily in the meaning-as-reference account of language use. Drawing on the meaning-as-use alternative developed throughout this dissertation, I recommend a pragmatic account interdisciplinary theory and practice. The basic contours of such an account are that

- (1) description and explanation are importantly inseparable,
- (2) we describe the world in the vocabularies that we do because we are interested in the implications of these descriptions, e.g. what such descriptions allow us to explain,
- (3) different disciplines provide different descriptions in the service of different uses, and
- (4) drawing descriptions from different disciplines is a matter of deciding which descriptions are best suited to address a complex problem, where

²⁶ I find Szostak's bans on methodological and ideological assumptions to be relatively opaque, and don't pursue them here. Presumably methodological assumptions are assumptions about how one studies the system in question, but it is difficult to know how one can know whether a given term applies without appreciating the method, or procedure, for assessing its truth. I suspect that ideological assumptions are something akin to valuational assumptions, and for reasons mobilized throughout this book, very much doubt that those assumptions can be separated from concept use.

(5) the best suited descriptions are those that coordinate our practices to help us realize our values, so:

(6) an integrated, interdisciplinary understanding of a complex problem is an understanding that uses descriptions to ameliorate a complex problem in a way that correctly prioritizes among, and weighs tradeoffs between, our values.

This section is meant to establish (1), and previous chapters (especially Chapter Two) establish (2) and (3) (see also Piso 2015). In the next section, I justify and further elaborate (4) and (5), which I believe establish interdisciplinary integration as an ethical project. In the final section, I draw connections between the conclusion of this argument and Phillip Kitcher's ideal of well-ordered science, which reframes interdisciplinary integration as well-ordered interdisciplinary science.

From Pragmatics to Ethics

Though the previous section argues for an account of language that understands meaning as use, this only suffices to foreground the pragmatic character of interdisciplinary inquiry. I am here interested in foregrounding the ethical character of interdisciplinary inquiry. This character is brought to light by exploring the types of reasons that can count for or against action, and by noting that ethical reasons, qua ethical, overrule other sorts of practical consequences. In other words, someone would be wrong to act in a way that is practical, or effective, if that act were also unfair, or vicious, or otherwise unethical; the only sort of reason that could excuse the unethical act is another ethical reason, one that shows that our initial judgment was shortsighted. Here I illustrate this ethical character by carefully considering a case of environmental conservation in Menabe, Madagascar. The case study allows us to reflect on how ethical reasons

would provide a basis for favoring particular disciplinary vocabularies in issuing an interdisciplinary description of a complex problem.

The first move in appreciating the ethical character of inquiry is by stressing that part of the meaning of a description is the actions (linguistic and otherwise) for which that description can serve as a reason. This is easier to forget when engaging later pragmatists like Brandom, who is prone to place too much emphasis on linguistic meaning (Margolis 2009). It's crucial to understand that the meaning of a concept is its use in a vocabulary, or that much (but not all) of understanding a concept is being reliably disposed to express a word under appropriate conditions. Brandom offers a careful argument for how concept use involves (1) using a concept correctly in making an observation and (2) using a concept correctly when carrying out a conversation. In both settings, he is careful to conceive of concepts as *actions*, and thus to think of concept use as bottoming out in a form of know-how (2015). The trouble with overemphasizing linguistic meaning, though, is the sort of linguistic idealism that I cautioned against at the opening to Chapter 2. There is a third setting where concept use is normatively governed, and that is in our embodied re-actions to making an observation or carrying out a conversation that go beyond further language use. For Sellars, these "language-exit transitions" offer a vital connection of word to world, without which language loses its grasp of reality (1963). For pragmatists in general, the movement from correct observation, through correct implications, to correct action is the basic system for using language to coordinate and achieve one's goals. On this view, semantics is in the service of pragmatics; it is only within a practice of coordinating goal-oriented action that concepts can mean anything at all.

When language-exit transitions are ignored, we are liable to take description to be an end in itself. Confronted with the meaning-as-use alternative to meaning-as-reference, Szostak very

much endorses description as an end in itself, championing as the goal of interdisciplinary integration the twin ideals of “representational clarity” and “comprehensive understanding” (2016, 211-212). This endorsement amounts to a half-hearted concession to meaning-as-use, where the empiricist-leaning philosopher admits that the meaning of a concept is its use, but then offers “representation of reality” as that use. Szostak is hardly alone here among theorists of interdisciplinarity. Allen Repko also analyzes interdisciplinary integration as the epistemological project of representational truth, while William Newell pursues the ontological side of the coin in striving for a comprehensive account of reality (Repko 2012, Newell 2001).

Wholeheartedly embracing meaning-as-use requires that we give up the notion of “representation of reality.” That ideal is unintelligible, not least because we could never be in a position to evaluate whether we have indeed correctly represented reality (only that our descriptions “work”). Better to take on the more modest goal of ameliorating problematic situations, where an embodied sense of doubt spurs inquiry, and the calming of that doubt brings inquiry to a close. On this view, scientific disciplines provide resources for ameliorating problematic situations in the form of descriptions-explanations that provide rules for moving from observation, through the space of implications, to action. What observations are relevant to this inquiry, and what actions count as warranted, depends on our orientation to the situation as a problem for which we can mobilize better or worse responses. Recall that explanation is the “*ultimate* objective of any existential inquiry” and that description is “inseparable” from explanation (Dewey 1938, 454; Sellars 1957, §108). Because explanation is intelligible only as a means-end relation, then description is also in the service of diagnosing particular means to particular ends. A scientific study provides a good description of a system if it allows us to explain the workings of that system in ways that matter to us and allow us to realize our goals.

Now, I want to propose that appropriately appealing to specialized scientific vocabularies, as we do in the recruitment of a particular discipline to describe and explain a social-ecological system, ultimately rests on ethical grounds. The basic picture is as follows: Different disciplines provide different tools for describing a situation as a problem and locating mechanisms for ameliorating that problem. In other words, disciplinary descriptions offer alternative accounts of ends (explanantia) as well as competing accounts of means (explananda). Choosing between ends is necessarily a question of valuation, as is choosing among means. The extent to which we should value one situation over another is a matter for ethics, understood as inquiry into the set of practices that produces the most desirable outcome. To say that ethics is the ground for settling on practices is to point out that ethical reasons—for example, that an end would be unjust, or that a means of realizing an end vicious—are *prima facie* sufficient to rule out the proposed practice. Our desired ends or means can adapt through inquiry, though, as we may learn that our values have unanticipated consequences or are unsustainable. For Dewey, this learning was a matter of distinguishing desirable consequences from merely desired consequences (1939). In these cases, the full array of values that we hold can justify revising particular values that prove troublesome. What ethical inquiry strives for is a configuration of practices that allows us to realize most of our values, most of the time.

Let me draw on one last case study to illustrate this idea. The case study is drawn from an example of environmental management in Menabe, Madagascar, where an interdisciplinary team of researchers studied deforestation (Sommerville et al. 2009, 2010). The central region of Menabe is home to a 100,000-ha dry deciduous forest that Madagascar has identified as one of its highest conservation priorities, but rural development around the forest continues to encroach. Early on in the project, the problem was framed as an underprovision of ecosystem services (i.e.

the benefits of preserving, conserving, or restoring ecosystems, such as water purification or carbon sequestration) (Sommerville et al. 2010). The interdisciplinary team also decided early on that the appropriate means for rectifying this underprovision was through what's known as a payment for ecosystem services (PES) management approach, and that this PES management approach would favor community-based management. PES management mobilizes economic research to identify the true value of ecosystem services, according to which local stakeholders are paid to leave these services intact rather than engage in more extractive land uses (e.g., agriculture or poaching). This incentive is meant to encourage sustainable behavior; if the incentive is properly priced, then stakeholders will refrain from extractive uses when their value is less than conservation, and engage in extractive uses when their value is more than the services compromised. Though different from the institutional economics approaches surveyed earlier in Ostrom and Bromley's work, PES management approaches represent a standard approach to environmental conservation (Pagiola, Bishop, and Landell Mills 2012, Kinzig et al. 2011). Indeed, Sommerville and colleagues provide two studies attesting to the success of implementing a PES management approach in Menabe (Sommerville et al. 2009, 2010). At least with respect to their stated goal of encouraging sustainable behavior, their complex PES management approach achieved its desired effect.

Now, we do not know the values of the stakeholders affected by the decision to study this social-ecological system in order to understand the efficient provision of ecosystem services via a PES management approach. We would need to know these values in order to criticize the approach as unethical, and as thereby unwarranted; the only standard against which we can judge the approach unethical is the standard to which the community would agree through a fully inclusive democratic deliberation (Norton 2005). What the interdisciplinary team did provide

was an admirably reflexive report of the challenges that they confronted in taking a PES management approach, in which they took care to acknowledge potentially problematic outcomes. Such a report allows us to imagine the sort of conversation that would have to happen in order to warrant the interdisciplinary team's orientation to the problem. In other words, was it right to conceive of deforestation in Menabe in terms of the underprovision of ecosystem services, and was it right to investigate the mechanism of PES? I want to consider these two questions in turn, beginning first with the legitimation of the goals of interdisciplinary social-ecological systems science, and then turning to the legitimation of particular mechanisms for achieving those goals.

The researchers are explicit that the goal of their study was “to encourage community forest associations to actively manage [Menabe] forests for biodiversity and sustainable benefits” (Sommerville et al. 2009, 1263). A number of goals are implicit to this articulation, most obviously the goals of biodiversity conservation and sustainability. It's worth noting that neither biodiversity nor sustainability fall under the myopic approaches to ecosystem valuation that Norton criticizes in his *Sustainability*; both biodiversity and sustainability are difficult to measure through market-based valuation, as neither qualifies as the sort of excludable, rivalrous goods that markets price efficiently.

Now, some context about the Menabe case locates additional goods that are at stake in social-ecological systems science. Encroachment on Menabe's dry deciduous forest is driven mostly by agricultural expansion and by poaching lemurs and tenrecs. Though the study is meant to establish payments that incentivize conservation and disincentivize agricultural expansion and poaching, it (rightly) acknowledges this tradeoff and counts agriculture and poaching as preliminary goods. Further, the study notes “the inequitable distribution of the costs and benefits

of biodiversity conservation,” where “the costs of protected areas are born locally, frequently by poor rural communities,” and requiring “international conservation programs to consider poverty alleviation” (Sommerville et al. 2009). Though we do not know the full range of values at stake in managing the Menabe forests, we can include both poverty alleviation and the equitable distribution of costs and benefits as preliminary goals of understanding the social-ecological system.

The framing of the goal of this study also provides some initial guidance into the sorts of means that the interdisciplinary team countenanced. Most obviously, the provision of ecosystem services under a PES management approach is focused on economic mechanisms for achieving the goals stated above. Ordinarily, these mechanisms take the form of payments to stakeholders who engage in sustainable behavior—when local residents refrain from clearing forest and cultivating crops, they forego the opportunity costs of farming, and are thereby compensated for their trouble. The study notes, though, that its goal is to encourage community forest associations to handle management. It is explicit throughout that distributing PES at the community forest association level can help leave local customs intact. Sommerville and colleagues (2009) explain

In community-based conservation schemes, where local institutions control the distribution of incentives, the distribution structure and ultimately fairness may not be clear a priori. Monitoring social indicators is thus a critical, if rarely performed, component of conservation and development projects. Failure to consider distributional and fairness issues can undermine the impact of a PES or the long-term success of conservation interventions. (1269)

By handling management at this level, the researchers sought to strike a balance between incentivizing sustainable behavior and disrupting local social relations. Additional context about

Menabe reveals some alternative routes for understanding the system; prior to the implementation of a PES management approach, authorities discouraged unsustainable behavior by issuing fines and, in the case of illegal agricultural expansion, prison sentences. Enforcement of the relevant laws, though, was limited or non-existent. Again, there are certainly additional mechanisms for achieving the goals of conservation, poverty alleviation, and equitable distribution of resources, but direct payments, community-based payments, and consistent enforcement of laws offer three initial alternatives to consider.

Now we can ask: What sort of conversation is required to legitimate the ends assumed by the study and the means recommended by the study? Let's imagine this conversation as a dialogue between an emerging interdisciplinary team of scientists and the stakeholders who are affected by the management and policy inferred from the team's reports. Now, a conservation biologist may suggest that Menabe, Madagascar faces a conservation crisis, and that the community requires a better understanding of local biodiversity and ecological sustainability. Indeed, we should assume that the conservation biologist is correct about these challenges, and that it is true that biodiversity is decreasing and natural resources running low. Local stakeholders could still object that these conservation goals are not nearly as important as economic development, or that curtailing poaching would unacceptably place the largest burden on the economically worst off. At this point the conservation biologist is likely to point out that economic development will be unsustainable if funded by agricultural expansion, or that there are means of discouraging poaching that would be less burdensome on impoverished communities. One upshot of this exchange is the recognition of the need to include a development economist on the interdisciplinary team, as a development economist should provide expertise in promoting economic growth for these stakeholders. Perhaps the team should

also include a rural sociologist to provide expertise on the burden of management strategies on the rural poor.²⁷

Now, the details of Sommerville and colleagues' study indicate that the primary goal of understanding the system was to strike a balance between conservation goals and economic development, and to determine the appropriate balance by allowing communities to freely trade sustainable practices (and the opportunity costs of not farming or poaching) for payments. Still other goals informed the preference for community-based management instead of regulatory authorities paying stakeholders directly. This is not because the only true description of this social-ecological system is in terms of community-based PES management. Rather, the warrant for describing the social-ecological system in these terms is because these terms strike a balance between the plurality of goods at stake in the system's management.

A similar conversation is necessary for legitimating the mechanisms considered in the pursuit of this plurality of goods. Because Sommerville and colleagues monitored social indicators, we have some evidence that community-based PES management produced some discontent among the stakeholders. The first sort of objection concerns the fairness of PES management approaches, which can marginalize particular types of resource users. In the Sommerville case, some stakeholders reported that they were not fairly compensated for refraining from farming. What can often happen with the implementation of PES management approaches is that some stakeholders claim property rights that were previously unenforced or contested, cutting off other stakeholders from previously open-pool resources on which they

²⁷ I focus here on the legitimacy of valuational and normative assumptions like those discussed in Chapter Four, but as Michael O'Rourke has pointed out to me, the Menabe case study is also rich with social epistemic assumptions. In particular, the case study assumes that the scientists leading the study are legitimate experts, i.e. that their expertise is recognized and authorized by the stakeholders to the study. I follow Jordan, Gust, and Scheman (2005) in appreciating the role of truthworthiness in the construction of expertise, and recommend trust-building practices in cases where the legitimacy of experts is in question.

depend (Corbera, Kosoy, and Tuna 2006). The study also took for granted gender-based inequalities that tend to afflict PES management approaches (Kerr 2002, Pagiola, Arcenas, and Platuas 2005). Sommerville and colleagues (2009, 2010) focused on land uses traditionally performed by men and interviewed women with difficulty and only with respect to foraging tubers. Understanding these dynamics requires more critical social scientists to inquire into the ways that management schemes such as PES produce inequality with respect to social categories that are not within the purview of every development economist. In this case, we can imagine stakeholders asserting a principle of assisting the worse off, so that the landless poor are not victim to the PES management approach, or principles of fairness and equality, such that women are compensated no worse than men. This could require mechanisms to compensate stakeholders who are negatively impacted by an otherwise desirable management approach, but it could also require starting from a different approach without a history of privileging wealthier, male stakeholders.

So far, the imagined conversations consider broadly consequentialist dialogue, where stakeholders weigh plural goods as goals, and broadly deontological dialogue, where stakeholders assert rights or principles that constrain rightful conduct. I want to lastly consider a broadly virtue theoretic dialogue. By favoring community-based management approaches, Sommerville and colleagues placed a priority on maintaining social relations, and social relations are grammatically well-suited to be evaluated with respect to virtues (i.e., social relations produce consequences, or violate principles, but simply *are* or *are not* virtuous or vicious). In the Menabe case, community-based payments were intended to support cooperation and flexibility, encouraging stakeholders to work together to find creative ways to provide ecosystem services. Community members expressed their solidarity through their preference for in-kind, non-rival

goods such as bicycles and generators. Distributing compensation at the individual level, or even at the family level, may have produced less virtuous social relations, with Sommerville and colleagues warning that “in many rural communities, it may not be possible explicitly to direct incentives to the relative poor without upsetting local social structures” (Sommerville et al. 2009, 1268; Agrawal 2001). Yet distributing payments at the community-level raised issues of corruption and discrimination, with board members reporting the highest level of net benefits and non-members expressing reservations (Sommerville et al. 2009). In the previous chapter, we saw that interdisciplinary teams including political scientists offered an understanding of how corruption operates as a mechanism compromising the resilience of coastal and marine social-ecological systems. Here too there is a need to understand the ethical consequences of various management approaches, to be able to anticipate the rise of corruption and the sustainability of virtues like cooperation, flexibility, and solidarity.

Why should these ethical consequences provide the ground for integrating disciplinary vocabularies and warranting explanations of social-ecological systems? Mainly, it is because an ethical objection to a proposed action, or the description-explanation from which that action is inferred as a language-exit transition, can only be overruled by another ethical objection. If stakeholders propose that biodiversity is a primary environmental good, and that an ecological description of the social-ecological system is necessary, researchers cannot counter that a hydrogeological description is easier to formalize or to quantify (cf. Piso et al. 2016). At the stage of deciding between disciplinary vocabularies as tools that we use, the truth of these candidate descriptions is yet to enter the picture.²⁸ Researchers, or fellow stakeholders, can point

²⁸ I assume here that disciplinary vocabularies serve as a reliable basis to truthfully describe social-ecological systems, e.g. that there are anthropological truths to be told, or meteorological truths to be told, and so on. Since each vocabulary can be the basis of true descriptions, then the fact that we’ve produced a true description in one vocabulary is not a reason to prefer that vocabulary.

out that description in an ecological vocabulary is likely to overlook rival goods such as economic development, and recommend a study that integrates ecology and development economics. That requires an understanding of the economic consequences of ecological conditions like biodiversity, and the ecological consequences of economic conditions such as development, and there is a tendency to think that this coupled understanding is the mark of integration (cf. Hirsch and Brosius 2013). But unless this coupled understanding is in terms that matter for action, that are laden with values that the community deems significant, there is simply no way for that community to *evaluate* the alternatives. And evaluating the alternatives is necessary for meaningfully describing the social-ecological system. The situation gets even trickier as more values are diagnosed in the amelioration of a problem; how should we weigh the provision of ecosystem services against principles of gender equity against virtues of cooperation and solidarity? There is no easy answer to this—ethical inquiry is *inquiry*, and we cannot settle on the relative merit of these values from the purview of the ivory tower. In the final section I will recommend an ideal for interdisciplinary integration following Kitcher’s notion of well-ordered science. Here the intermediate conclusion is as follows: interdisciplinary integration requires that we understand disciplinary vocabularies as tools for realizing different sorts of values, and the key to interdisciplinary integration is to understand the priorities among, and tradeoffs between, this plurality of values.

One last remark: I earlier commented that this ethical construal of interdisciplinary integration is a proposal. I offer it *as a proposal* for two reasons: first, ethical inquiry is in an important sense empirical, in that settling on a configuration of practices is not the sort of thing that philosophical reflection can achieve on its own. Indeed, ethical inquiry commences once the occasion for philosophy comes to a close; careful attention to the meaning of the use of concepts

like *description* and *explanation* point toward the need for ethical inquiry, but careful attention to the meaning of ethical terms like ‘justice’ and ‘virtue’ will not settle the empirical questions concerning what concrete practices best realize a plurality of values. The second reason is that a key method for ethical inquiry is deliberation, through which community members propose ways of improving their lot in life. We are committed to deliberation insofar as we are committed to an egalitarian community where fellow members have equal standing in evaluating the value of proposals and their alternatives. Given the plurality of values that exist in any community, it is vital that our method for valuing proposals is dialogical, as no one is in a position to solely determine the truth of value claims.

An Ideal for Integration: Well-Ordered Interdisciplinary Science

In Chapter One, I suggested that philosophy can trace a series of moves from the practical question, “How should we compose an interdisciplinary team of scientists?” to the practical question, “By what procedure can we legitimate the use of particular social scientific explanations as the basis for environmental management?” In this final section, I want flesh out an earlier proposal to answer this second question (Piso 2016). My proposal appeals to Philip Kitcher’s notion of well-ordered science, a notion he mobilizes in his investigations of science in a democratic society (2011). Here I extend his notion to serve as an ideal for interdisciplinary collaboration, arguing that interdisciplinary integration should work outward from the disciplinary vocabulary that speaks to what we value the most. Beginning with a disciplinary description that attends to only some of what’s at stake in a complex problem, interdisciplinary integration proceeds by considering in turn the remaining problems as described in the vocabularies of additional academic disciplines.

Much of Kitcher's investigation is dedicated to dismantling the value-free ideal, which Kitcher locates as a problematic foundation for how contemporary society understands its relation to science. When we think of science as value-free, we tend to think of scientific inquiry and democratic decision-making as wholly separate businesses, and we tend to support lines of scientific inquiry independent of their relevance to public problems. This division of labor, however, can get us into trouble when we suppose that the truths to which scientists testify are the truths upon which we found policies. Kitcher's key argument is that science in a democratic society must distinguish between truths, of which there are many, from significant truths, of which there is a more manageable set. He reminds

virtually all of the 'whole truth' lacks any interest for anybody (think, for example, of the large infinity of truths about the areas of triangles whose vertices are three arbitrarily chosen objects). Supposing that Science aims at the complete true story of the world is as misguided as the suggestion that geography seeks to draw a universal map, one revealing every feature of the globe. (106)

As careful observers of science in society have noted, the abundance of truth hardly resolves political controversies, as competing factions simply marshal different facts in support of their preferred policies (Sarewitz 2004). The issue is not that competing factions disagree about the truth of different scientific statements—though they almost certainly do—but that they problematize situations differently in light of different values, and hence appeal to different facts relevant to these different orientations. Kitcher points out that a prior deliberation is required before science can inform policymaking. It is in this prior deliberation that citizens work out their scientific priorities, charging scientists to inquire into explanations of shared public problems.

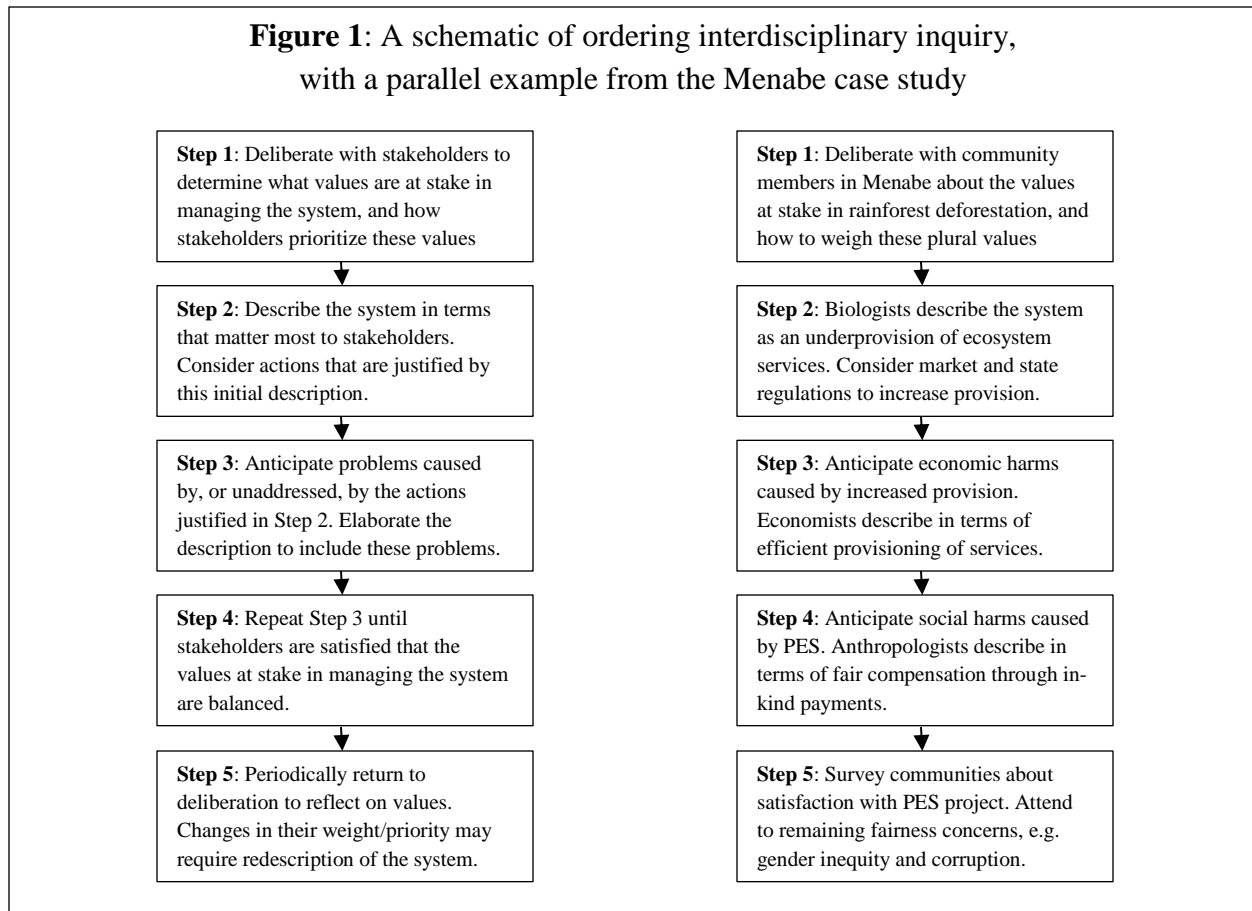
Kitcher proposes that the ideal for this prior deliberation is well-ordered science, which is effectively the prioritization of scientific problems and projects that the fuller democratic community would endorse through deliberation. He elaborates

A society practicing scientific inquiry is well ordered just in case it assigns priorities to lines of investigation through discussions whose conclusions are those that would be reached through deliberation under mutual engagement and which expose the grounds such deliberation would present. (113)

Lines of investigation are more or less scientific programs that explore a range of intersecting scientific questions pertinent to a public problem. For instance, space travel might be one line of investigation, while cancer research might be another. Our reasons for prioritizing one line of investigation over another have to do with the relative value we place on the public problems that investigations promise to ameliorate or the public projects that investigations promise to advance. Kitcher appreciates that participation in a democratic deliberation is not merely a matter of voting on the lines of investigation that address one's personal projects or problems. Rather, deliberation abides by conditions of mutual engagement, which Kitcher breaks down into epistemic conditions (truthfulness, sincerity) as well as affective conditions. Affective conditions involve taking the perspective of fellow deliberators and regarding their values with equal consideration to one's own. Now, these conditions are always subject to criticism and revision, so deliberation should be transparent about these conditions as a contingent ground for adjudicating among diverse value claims. Initially, though, conditions of mutual engagement allow democratic deliberation to embark in reason giving and taking through which a diverse community can work toward collective flourishing.

Kitcher is interested in lines of inquiry at the societal scale, but his analysis provides a sound basis for the practice of interdisciplinary integration. When we encounter a complex social-ecological problem, we cannot set forth to comprehensively describe the situation or to provide the “whole truth” of the matter. But we do not need this “whole truth” to decide which management strategy or policy is appropriate; we need only those truths upon which our action hinges. Anticipating what those truths might be requires that we order the lines of inquiry significant to the community confronting the social-ecological problem. The highest priority lines of inquiry will be those in which the community invests the greatest significance. Provided that an interdisciplinary collaboration studies the dimensions of the problem in the order prioritized by the community confronting that problem, then that interdisciplinary inquiry is well-ordered. This ordering is not as simple as first describing, say, the ecological dimensions, then setting this description aside and next describing the economic dimensions, and then the political, and so on. That would be multidisciplinary collaboration, and we are after interdisciplinary collaboration. Rather, ordering is a process of first describing (in a particular disciplinary vocabulary) the dimension(s) of the problem most significant to a community, understanding the actions that would ameliorate the problem under that disciplinary description, and then in turn considering the problems that remain (or are caused) according to other disciplinary descriptions (Figure 1). Rather than an order of considering each disciplinary understanding in turn, it is an ordering that starts with the most significant understanding, and works “outward” through additional disciplinary understandings.

Figure 1: A schematic of ordering interdisciplinary inquiry,
with a parallel example from the Menabe case study



We can consider the Menabe case study as an example of starting with the most significant description and working outward through additional descriptions provided by a diverse interdisciplinary team. The study operates under the assumption that the primary problem facing the Menabe community is an underprovision of ecosystem services. It then describes the practices of community members in terms of their consequences for deforestation and biological conservation. Now, if this disciplinary description from conservation biology were sufficient, then environmental managers could infer a range of consistent management approaches. Instead, the interdisciplinary team posed another set of significant scientific questions that would speak to the proper pricing of ecosystem services to balance conservation with economic development. Note that these scientific questions are much more focused than the lines of economic inquiry

that would address the broader questions about economic development; there are all sorts of mechanisms that would be relevant to promoting economic development that do not figure in an interdisciplinary study that prioritizes conservation.

Now, armed with an interdisciplinary description of Menabe in terms of conservation biology and development economics, the team posed another set of questions aimed at understanding community-based payments as opposed to direct payments to stakeholders. Again, this is a much more focused line of inquiry than one might pose if more broadly interested in promoting social relations that bind Menabe communities. Eventually, the team considered mechanisms like providing in-kind, non-rival compensation to communities. The general arc of the research is one of prioritizing biological conservation and moving outward through additional disciplines as they prove relevant to the values at stake in the complex problem. As long as the order through which the interdisciplinary collaboration considered these problems accords with the priority that stakeholders placed on these various stakes, then we should embrace the study as well-ordered interdisciplinary science, and therefore warranted interdisciplinary science.

It's important to appreciate that this ordering could not proceed without the sort of coupling discussed in the previous section with respect to the tradeoffs between conservation and development. We would not know to follow inquiry into biological conservation with inquiry into economic development without discovering the consequences of conservation for development. The same holds for the consequences of development for local customs. But while a coupled understanding encourages deliberators to consider the plurality of values at stake in social-ecological systems science, this coupled understanding is not yet the mark of integration. Certainly, the consequences of ecology for economics, and vice versa, are endless; perhaps there are not as many truths as the truths pertaining to the areas of triangles whose vertices are three

arbitrarily chosen objects, but there are still too many truths to be helpful. Add these to the consequences of either discipline to anthropology, or political science, or any other discipline with something to say on the matter, and we find ourselves rife with truth but short in significance. If we set forth on the impossible task of locating all of these truths, we would depart from the prevailing meaning of interdisciplinary integration, where integration marks the very possible success condition of interdisciplinary inquiry. Instead, I suggest we think of an integrated understanding of a complex problem as the understanding that is warranted in light of how we prioritize among, and weigh tradeoffs between, the values at stake in the situation.²⁹ This preserves the sense in which integration marks the highest achievement of interdisciplinary science, and frees interdisciplinary science from the Sisyphean task of producing a comprehensive representation of reality.

One final remark is in order here. This approach to interdisciplinary social-ecological systems science must be adaptive. Community members may prioritize particular goods, principles, or virtues at the onset of deliberation that they may then revise on the basis of subsequent experience. It may seem appropriate to prioritize economic development over biological conservation until agricultural expansion proves unsustainable, or it may seem appropriate to favor community-based management until its consequences for women or the rural poor become apparent. Scientists can help testify to these tradeoffs when they participate in deliberations. Now, certain values will remain the ground for deliberation; in order to *weigh* tradeoffs between economic development and biological conservation, one must appeal to shared values like security, sustainability, and opportunity. But inquiry can discover that the weight we

²⁹ For Hirsch and Brosius (2013), integration is a matter of understanding the tradeoffs between different responses to a problem. In effect, my proposal pushes integration one step farther; not only must we understand these tradeoffs, but we must also understand how to correctly weigh these tradeoffs, and hence we must know how to act given our particular priorities as a community.

placed on various values was mistaken, and that we ought to reprioritize our values. Ultimately, we are striving for a constellation of practices that sustains our plurality of values, and we must constantly return to experience to test these practices.

Conclusion

Environmental pragmatists recommend democratic deliberation as a procedure for legitimately integrating facts and values when engaged in environmental science, policy, and management. Through such a procedure, we recognize the instrumental quality of scientific descriptions. Most of the dissertation is dedicated to clarifying this instrumental quality in the social sciences, so that we can recognize the normative and valuational assumptions implicit to social theories from diverse disciplines. This chapter is concerned with a remaining challenge; once we recognize how facts and values are integrated in various social sciences, how do we then integrate these social sciences through interdisciplinary collaboration. My tack is hardly surprising—once again, we must attend to the values implicit to these social sciences, and then organize disciplinary contributions according to how we prioritize the values at stake in ameliorating a problematic situation.

The case study from Menabe, Madagascar serves as a model of how a complex social-ecological system admits of an interdisciplinary description that attends to the plurality of values at stake in describing the system. Because it is how we prioritize these values that structures how scientists and stakeholders correctly describe the system, I take interdisciplinary collaboration to have a fundamental ethical dimension. In other words, ethical reasons provide the ground for justifying what disciplinary vocabularies should be included in the project and, from those included, which vocabularies should initially frame the situation as a problem. Interdisciplinary collaboration should work outward from the disciplinary vocabulary that speaks to what we

value the most, then proceed by considering in turn the remaining problems as described in the vocabularies of additional academic disciplines. This procedure is fallible, and must be adaptive, but it has the benefit of focusing on the significant truths that should serve as a basis for action. There are simply too many true ways to describe a social-ecological system, and settling on the *right* description requires a rich dialogue about the consequences, principles, and virtues at stake in issuing a description that provides reasons for action.

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