Discipline and the Care for Nature

by

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INTRODUCTION
Knowing what you're talking about (and what I'm going to talk about)

> Ahmad, an Indonesian fabric merchant, thinks abstractly.
>
> > It never used to be so hot as this. This is probably due to cutting down the trees. Every movement has a sound. Now there are more sounds than movement, because of television, radio, chain saws, motorboats, and the like. The atmosphere is like a container; sounds are not lost from it. They travel from one place to another creating new influences. The relation between the earth and the sun changes as a result. (quoted in Tsing 2005: 116)

Ahmad's philosophy of nature reaches us after a long journey beginning in South Kalimantan on the island of Borneo and guided for the most part by anthropologist Anna Tsing who originally recorded these musings in 1997. Most environmentalists in Indonesia, Tsing says, would be unreceptive to Ahmad's opinions on nature and his "countrified" rhetoric (2005: 116). He is not embedded as thoroughly in the international discourses of conservation as many other environmental activists are. Indeed, the only way his ideas have traveled outside of his home is in the luggage of an anthropologist. But Ahmad is an educated man, and his reflections on the environment "are consonant in many ways with the spirit of engaged intellectual discussion of nature" (116). His knowledge of natural science and Islamic theology is sophisticated, and he looks to understand his own self at the confluence of those two systems of belief.

> > Human actions affect atomic particles. There are two kinds of atomic particles: those under control and those out of control. A bad heart makes things out of control. A good heart makes things under control. This is where science and religion meet. It's no different from what foreign scientists say, but they haven't given these issues enough attention. (quoted in Tsing 2005: 114)

Even though he is off the beaten path of intellectual discourse on such matters, Ahmad wants and likes to talk about how one can live responsibly in the modern world, and he

uses the languages of physics and of Islam (his two principle disciplinary footholds) to do so. But what is Ahmad talking about when he talks about atoms and the atmosphere? Or, as the cynical version of that question goes, does he know what he's talking about?

Best to respond to the cynics with another question: do we even know what we're talking about? The reason I ask this question is not because I want to dismiss the authority of scientific knowledge about atoms and the atmosphere. Physicists and planetary scientists have developed a rich and flexible system of beliefs about their objects of inquiry, and their knowledge allows them to understand a wide range of phenomena. Their research practices let them think abstractly too. The reason I ask this question is instead to underscore Ahmad's point – which is also a robust philosophical argument – that scientific issues should not be divorced from the "attention" given to them by scientists and other people.

I think what Ahmad means by "attention" is the consideration that people give to their objects of inquiry and care, and it is important to remember that this consideration is ongoing. In other words, the people paying attention to part of the world aren't alone; they are part of the tradition of paying attention to that part of the world. In the case of atoms and the atmosphere and other scientific objects, we call those traditions of paying attention disciplines. 'Knowing what we're talking about' when we use scientific concepts is a matter of being aware that those concepts have long disciplinary histories. For instance, when we talk about genes – no matter how sophisticated that talk is and even if we get some of it wrong – we are to some extent enacting what has been going on in genetics research over the past century. (People doing advanced genetics research are enacting virtually that entire history.) As we enact disciplinary histories, we simultaneously gesture toward disciplinary trajectories and the

possible significance that scientific concepts might take on for us. What genes will (be allowed to) do, insofar as they have *potential* or are somewhat *unpredictable*, is to a large extent determined by what geneticists (are allowed to) do. Many of us expect genetic effects to trickle down from research into our lives.

Knowing what you're talking about means paying attention to the traditions and trajectories of the paths you're on. Attending to these tracks requires discipline; it is with discipline that you know what you're talking about. Discipline also allows you to think deeply about where it is that you are – in that tradition and along that trajectory – and about whether or not that is a good place to be. Accordingly, knowing what you're talking about means caring about where you are; discipline means nurturing what you know into existence where you are. Deborah Bird Rose, a theorist of social and ecological justice, discusses what it means for part of nature to belong to somebody like this.

> Persons are immanent in those portions of the world which are theirs, and those portions of the world are immanent in them. Ephemeral persons are embedded in the world, and by the work of their lives they bring forth the life of the world. This process is nurtured and sustained through cultural procreation. (2004: 173)

Discipline, then, is the authority over and responsibility to and for the life of a particular set of natural phenomena. You are not alone in a discipline, if only because you belong to the very natural phenomena that belong to you. Moreover, disciplinary belonging is a cultural phenomenon, which means at least that it's a multigenerational affair, and that it will probably involve some sort of ancestor worship. However, this belonging, although it begins with bringing forth, is also a letting go, as natural phenomena extend into other disciplines and even fade out of existence. How is the "life of the world," once it is

brought forth by disciplined practice, managed as a 'public interest'? And how is the world managed when its very existence seems to be threatened?

Insofar as knowing nature and caring for it are thoroughly entangled, the traditions and trajectories of conservation science, most notably the discipline of conservation biology, are of central concern for me (a student of Biology and the child of a Biological culture). It is in these disciplines where the care for nature is made explicit as a norm of practice, and other disciplines wishing to express a similar care are often compelled do so in the language of conservation. Although these projects are distinctly Western endeavors, their target is the globe, understood as the domain being threatened by irresponsible human activity. The ubiquity of ecological degradation makes everyone everywhere a stakeholder in conservation. Earth is the only planet we've got. This inclusiveness is augmented by the fact that anthropology and other globalizing exercises have discovered practices around the world, even and especially in isolated or traditional cultures, that seem to count as environmental stewardship. The terms of conservation may not be appropriate in every circumstance, but nearly everyone seems to care about something we can call 'nature'. The alleged universality and primacy of nature as a human concern gives grip to global projects of conservation. Or is it the other way around?

Either way, the care for nature (whatever it is) often sparks abstract thinking and claims to the universal. And it is through disciplined practice that these claims reach the globe. Discipline allows us to think abstractly and to express conceptually – that is, to communicate powerfully and intelligibly – what we know and what kind of place and world we want to live in. As such, discipline is just as much about sophisticated knowledge as it is about effective politics. The union of knowledge and power in

scientific discipline isn't a tension where one must be sacrificed for the sake of the other, but rather a coordination where one is pursued for the sake of the other. The intuitive understanding of disciplinary knowledge is expert knowledge, and this makes sense in our scheme because experts are the kind of authorities that engage effectively in political debates. But politics also require the building of alliances, and this necessitates making expertise intelligible to the other constituencies it would (or should) be affecting. Therefore, discipline often entails the synchronization of different knowledges in order to respond appropriately to certain events and to make politically-informed differences.

In other words, disciplined practice is not only about the deepening of knowledge (on multiple, specialized paths) but also about the 'unification of science' (on a unified political platform). This unity is not the logical unity of reduction (see Oppenheim and Putnam 1958), but the contingent coordination of sciences to understand complex events and serve the common good (see Neurath 1983). Jordi Cat, Nancy Cartwright, and Hasok Chang (1996) interpret Otto Neurath's conception of 'unified science' in terms of his vision of social unity[1]:

> There is no possibility of constructing a single, abstract, unified science. But the sciences can be unified at point of commonly planned action, and they should be unified, to build a socialist economy that works. It is in this sense that 'unified science' was, in Neurath's words, 'the great task of consciously cultivating the future and the possible.' (1996: 362)

It is easy to read Neurath's conception of 'unified science' as a programmatic statement in favor of mission-oriented interdisciplinary research that breaks down disciplinary borders in the interests of the task at hand. Indeed, this is how many critics understand the practice of conservation biology: a field of study that transcends the rigidity of

[1] Otto Neurath was a leading figure of the Vienna Circle. I will use quotes around his term 'unified science' to remind us that Neurath means something very different by 'unified' than what reductionists mean.

discipline in order to connect a variety of knowledges. As a matter of fact, Neurath illustrates why sciences should be unified by describing a situation that nowadays might call for the expertise of conservationists:

> For example, whether a forest will burn down at a certain location on earth depends as much on the weather as on whether human intervention takes place or not. This intervention, however, can only be predicted if one knows the laws of human behavior. That is, under certain circumstances, it must be possible to connect all kinds of laws with each other. Therefore all laws, whether chemical, climatological, or sociological, must be conceived as parts of a system, namely of unified science. (Neurath 1983: 59)

While the interdisciplinary nature of 'unified science' is immediately apparent, it is important to recognize two ways in which discipline is preserved in 'unification'. First and foremost, disciplinary knowledges – the specialized skills and know-how of trained professionals – instead of being erased, are exactly the kind of knowledges that are called upon in 'unified science'. Neurath's opposition to disciplinary reductionism, readily interpreted as a defense of the special sciences against the dominion of physics (Cat, Cartwright, and Chang 1996), can also be read more broadly as a defense of disciplinary expertise in general. Without disciplines, there are no laws to connect. Second, and this point is more subtle, the task of unification calls for its own kind of expertise, that is, its own discipline. This kind of discipline is often forgotten in accounts of the politics of knowledge production, if only because it doesn't really seem to have much of a tradition. However, the intellectual commitment it takes to unify different knowledges and interests – however loose, complex, or awkward that unity is – is the discipline that grounds every knowledge practice. It is this discipline that orients us toward deeper questions, not simply about the abstract characterization of natural phenomena, but

about what is at stake in that abstract characterization, and how to proceed with and through those phenomena.

So how should we think about the discipline of conservation biology? Unlike other scientific practices that are more entrenched, conservation biology is young enough that we know it isn't simply a body of knowledge. Nor is it the coordinated application of other disciplinary bodies of knowledge to a new set of phenomena. I believe that conservation biology is its own discipline, and that is a contestable claim. But what does it mean for a field of study to be a discipline? A discipline has its own domain of inquiry that belongs to it and that it belongs to. A discipline also has its own standards of practice and its own issues to debate and terms of debate. A discipline is an authority over the objects in its domain, but it is also liable for the existence of those objects, and it will held responsible for any misconduct with or misrepresentation of those objects. In other words, disciplinary authority is held accountable to other practices that have a stake in its domain. Simply put, if conservation biology were a discipline, it would mean that conservation biologists had better know what they're talking about – and if not, they'd better figure it out soon – because there is something at stake in their practice which matters to a lot of people. Discipline is what we need when we need somebody who knows what they're talking about.

The first chapter will lay out the basic question of my inquiry, which is what it means to participate in a scientific practice. This question will be a springboard into the language of normativity, and I will begin articulating how normative accountability is a constitutive aspect of building knowledge. The second chapter will address the question of discipline more directly, using the terms and issues introduced in the previous chapter. What makes disciplines an operative unit of scientific practice? How is normative

accountability managed and directed in disciplinary practice? It will become clear that my interpretation of discipline does not turn on a sociological description of academia's disciplinary structure (as if academia were the whole, and disciplines were its parts). I am more interested in the dynamics of disciplining that lie beneath any given cluster of intellectual projects. What kind of accountabilities does disciplinary normativity embed in disciplinary knowledge? The third and final chapter will extend the discussion outside of the campus limits and try to understand disciplines not just as fields of study but as cultural movements. The underlying motivation for this broader interpretation is an awareness of the fact that intellectual practices cannot be isolated from their embeddedness in other practices. How do these broader accountabilities – to other cultural and political practices, to society, to the planet, to nature – affect the way knowledge is produced and consumed? It will become clear that because of these accountabilities, knowledge travels, often establishing a strong point of contact between different practices. However, what these 'accountabilities' are isn't a question that just belongs to the philosophical discipline (of science studies); many scientists and other knowledge makers are quick to acknowledge the different 'responsibilities' that are built into their practice. But no matter who's talking about it, 'accountability' is not a magical explanation of what knowledge is, or how knowledge travels. It is a category that we invoke to make normative claims about what knowledge *should* be, or how knowledge *should* travel. As such, beneath every knowledge claim, in addition to all of the responsibilities we must maintain, there is a 'politics of accountability', and we should engage it.

CHAPTER ONE
Normativity and accountability in scientific practice

Scientific knowledge and scientific practice

For many philosophers – professional and otherwise – the wonders of science are wonders of knowledge. This intellectual tradition in science studies inherits much of its vocabulary and attitude from the philosophical discipline of epistemology. The products of science are bodies of knowledge that explain what is going on in the world, if only approximately. The products of philosophy of science, then, are accounts of what is going on in scientific explanations that makes scientific knowledge so authoritative. In this scheme, the object of inquiry is whatever does the explaining – a theory, a set of laws, or whatever linguistic formulation it might be. Even while this epistemological tradition was becoming entrenched in science studies, it became apparent that many things called 'science' – most notably, biological sciences – don't proceed by articulating systematic theories or universal laws. In the absence of a general epistemological description of science, scholars began to look beyond the structure of theories – to social relations and the material conditions of science (e.g., laboratory practices, mundane affairs) – to account for the authority of scientific knowledge. In this scheme, the objects of inquiry are not truth claims with some special explanatory power, but the effects those claims had in the context of human life and social relations. As this alternative tradition has it, scientific activity is oriented toward the practical construction of the world – where the social and political influence of knowledge is at stake – and not toward some set of theoretical abstractions – where the accurate representation of the world is at stake.

These two competing traditions have figured objects of inquiry that are very different from each other – scientific knowledge versus scientific practice. The scholarship produced within each tradition follows correspondingly different trajectories. To identify units of philosophical analysis, the concepts to be articulated, is to struggle with very basic questions. What is it about science that matters? What about science needs to be considered and written about? Nowadays, it is often considered most savvy to talk about scientific practice without having to forfeit the intuitive sense in which scientific explanations are unusually compelling. This trend in science studies inherits much of its attitude from the pragmatist tradition in philosophy. However, pragmatic approaches to understanding science are not the same as sociological accounts that dismiss many serious ontological considerations and ground their analysis in 'social construction' of knowledge. Social constructivists often theorize social 'practice' in the attempt to delegitimize the epistemic authority of science. Pragmatists, on the other hand, take seriously the epistemic authority of scientific knowledge (without trying to legitimize it or delegitimize it), but look to the wider settings in which that knowledge is used authoritatively in order to figure out what 'practice' is.[2] Both schools of thought are challenges to the epistemological story told by traditional philosophy of science, but unlike social constructivists, pragmatists deny that scientific practice is just about human social affairs, consisting only of the doings of human agents. Pragmatists, like the scientists whose activity they are engaging, acknowledge that scientific practices include the broader material settings in which science is conducted. Inquiry into scientific practices is not a matter of assessing or analyzing the structure of truth claims, nor is it simply a matter of theorizing social and political power relations. If scientific practices

[2] See Rouse 1996 for an extended critique of the de/legitimation project.

themselves are about the material arrangements of the world and the discursive rearrangements of it, then the philosophical inquiry into those practices is about the same thing.

A normative conception of scientific practices (part I)

As is the case with many abstract concepts, 'practice' has been invoked in a wide range of scholarly settings to mean a variety of things. The most intuitive interpretation of practice is a loosely defined set of *regularities* in the actions or beliefs of some group of human beings. For instance, we might identify the aforementioned epistemological tradition in science studies as a particular practice because of the more or less shared beliefs (e.g., that scientific knowledge is an interesting thing) and actions (e.g., writing about scientific knowledge) of that group of scholars. These epistemologists might go on to identify another practice (e.g., the practice of cell biology) by reference to a shared vocabulary, method or pattern of reasoning among a given community of scientists.

But other scholars, especially those with an ear for the historical and temporal rhythms of a practice, have a hard time swallowing an argument that sees practices as grounded by some continuity of method or conceptual scheme. The flexibility of an intellectual practice is evidenced by the fact that even those scholars and scientists who think their predecessors were totally wrong are nevertheless compelled to locate their work in and acknowledge their debt to a tradition of thought. This is especially true for radical scholarship, which, in looking for the unarticulated 'root' of an already articulated problem, can completely reorient a tradition from a position that is squarely within it. In order to overturn existing theories or laws (racist theories or sexist laws, for example), intellectuals must first embed themselves in the practice of formulating theories or

making laws. Within the history of any readily identifiable tradition of thought, we recognize a series of disruptions and disagreements over core concepts, the characterization of a domain, and the nature of things. In the history of a practice, then, is there any real unity? Or is the continuity between components of a practice (e.g., Aristotelian *ethics* and Kantian *ethics*, Darwin's *gemmule* and Mendel's *gene* and Crick's *gene*) a nominal link that is founded by historians of a practice rather than the practitioners themselves?

What's more, we recognize variation and perhaps even inconsistency not just in the history of a practice but in its contemporary instantiation. Consider the practice of conservation biology and its focus on evaluating and protecting 'biodiversity.' This term, coined in 1986 simply as shorthand for the existing concept of 'biological diversity,' remains highly flexible (Sarkar 2002). It has been used not only in biological research (from genetics to ecology) but also in the global environmentalist movement to refer to almost any kind of diversity that should be cared for. There is the species diversity of a community, the phenotypic diversity of a species, the genetic diversity of a population, and even the genetic diversity of a community (a phenomenon studies in the relatively new field of metagenomics). Many conservationists are also quick to cite the importance of the cultural diversity, especially in the tropics, of human beings who are supposed to be the stewards of biological diversity, especially rich in tropical climates. Beyond the issue of definition is the problem of measurement. Is community diversity equal to the number of species present? Or do we have to account for the composition of a community, measuring the rarity and commonality of different species?[3]

[3] See Maclaurin and Sterelny 2008 for a treatment of the philosophical problems associated with the definition and measurement of biodiversity, especially in the context of conservation biology.

Despite the definitional ambiguities and conceptual heterogeneities of bio/diversity, the practice of protecting it has flourished. And this practice is a matter of disciplining ourselves. What I mean to say is that conservationism and environmentalism more generally are often talked about as a way of life, with new habits of consuming food, processing waste, and commuting to work, and even a new spiritual connection with nature.[4] Conservationism is also a call for social reform in the spheres of law, urban planning, and economic development, and, above all, a call for new kinds of knowledge. We are disciplining ourselves to be and to think green. Indeed, the practice of conservation biology, as it has coalesced into a discipline, has aimed to become an 'obligatory passage point' for those who have a stake in the conceptualization and preservation of biodiversity (Latour 1987). It is clear that conservation biology is not only an epistemic practice aiming to account for its objects of inquiry but also a cultural and political practice that aims to account for and respond to these objects in ways that matter to broader concerns.

But despite this expanding discourse on the importance of conservation and biodiversity, conservationists cannot easily make generalizations about how those things are important. Likewise, they know that they want something, to *save* something, but it has been notoriously difficult to articulate what that something is or how they should go about saving it. And yet, conservationists march forward. So what is it that unifies their

[4] Conservationism and environmentalism are not the same practice, but their conceptual links are undeniable. One might argue that conservationism is a smaller, more 'scientific' movement, whereas environmentalism is a broader 'humanitarian' movement. Even though the scientific principles of conservation are somewhat autonomous from environmentalist ethics, conservation is still held accountable to the culture of environmentalism in many ways. This kind of accountability is an issue I take up in next two chapters. But in the meantime, I often slip from talking about conservation science to talking about conservationism or even environmentalism (sometimes without making that slip explicit). Hopefully the arguments presented in the next two chapters will account for these slippages.

march? Why is it worth identifying as a conservationist? If there is no set protocol for research or action, what do conservationists do when they get together?

The answer is that they argue. Beneath the flickering reality of biodiversity and contested conceptions of it is the overwhelming presence of a common concern for what it is, such that conservationists will listen carefully to what others have to say and to the ways that nature speaks up for itself. Caring about a practice is what makes someone a part of that practice. Without having to discover or articulate any regularity in action or belief, we can account for the integrity of a practice by appealing to the sense of responsibility that all practitioners have to participate in that practice in a way that is sensible, intelligible, and accountable to the claims of other practitioners, to broader cultural concerns, and also the natural phenomena they seek to understand. I am drawing this normative conception of practice from the work of Joseph Rouse (1996, 2002).[5] What binds a practice together in his description is not some uncontested formulation of key concepts or methods of inquiry; nor is it some common set of inarticulable competences or unspoken rules of action; practitioners need not share a common understanding of what the practice is, where it is going, or even who participates in it. "Not all practitioners perform the same actions or have the same presuppositions," Rouse writes, "but practitioners and other constituents of a practice are accountable for performances or presuppositions that are inappropriate or otherwise incorrect" (2002: 169). Normative accountability within a practice constrains what a practitioner can do and think and say, but never so entirely that a particular doing, thinking, or saying becomes a permanent, anormative regularity. It is, of course, possible that a succession of normal activity generates a recognizable pattern. We call this pattern a norm. If this

[5] Rouse himself draws on the philosophical work of Donald Davidson, Alasdair MacIntyre, Charles Taylor, Robert Brandom, and John Haugeland among others.

norm is a pattern of social activity, we call it a social norm; if this pattern is 'naturalized' – that is, if it is interpreted as a natural phenomenon, outside the direct influence of social norms – we call it part of nature. Social norms, nature, and other patterns are real, but beneath every one of these regularities is the responsibility we have to recognize it as a pattern.

Irregularities or abnormalities that do crop up don't undermine the integrity of a practice, but their peculiarity must be held to account.[6] This radical accountability often reorients the norms of a practice so drastically that practitioners come to wonder about the past, "Why did we ever do such a thing? What did we think we were doing?" In post-Kuhnian science studies, these practical reorientations of science came to be understood and studied as 'paradigm shifts.' However, to read the history of a scientific practice as a series of 'revolutions' and to ask how a scientific community could rework its norms without falling apart is to assume that the practice is grounded by the sharing of normal beliefs or activities. As Rouse explains, what a more adequate inquiry into the norms of a practice should reveal is that the correct understanding of what those norms were in the past and what they are right now is itself a normative matter. In other words, the correct understanding of the norms of a practice is itself a part of that practice and, therefore, open to normative assessment within the ongoing development of the practice, such that the practice outruns any particular formulation of what that practice is all about. This pervasive normativity of practices is perhaps captured by the image of scholars and scientists trapped in their heads, paralyzed with a fear of getting it wrong. However, we should also recognize this pervasive normativity in the hyperactivity of scholars and

[6] It is a true that not all irregularities are absorbed or explained away or even noticed. The normativity of practices extends to the very habits of accountability that would recognize and identify the incorrectness of some actions or beliefs.

scientists as they try to figure out what's right. I don't mean to suggest that scientists never really figure out the answer to something (so they keep trying until they give up and switch questions), but rather that, in the course of solving their current problems, they often notice others which can become the focus of attention. The generating of new questions, then, is intricately linked to the process of answering old questions. In turn, the significance of the old answers is affected as the new questions and concerns are addressed.

In practice, there is never a common ground that straightens out matters once and for all; there are always other issues to settle. We see this kind of ongoing constitution of the cutting edge of a practice in the history of conservation biology. While conservation biology has only recently become a recognizable scientific discipline, its practitioners work through a much longer tradition of biological conservation and care for nature. It was the 1980s when biologists began to articulate a disciplinary identity for conservation around the concept of biodiversity. For North American conservation biologists, preserving biodiversity was a matter of preventing the extinction of endangered species. Bryan Norton (2008: 376) points out that this species-by-species approach and also the Endangered Species Act from the previous decade are reiterations of the ancient practice of protecting game species (for the tribe, the crown, the economy). In the modern iteration, protection was simply being extended to non-game species, as an acknowledgement of their value.[7] The principle task of conservation biologists was to design not "game" reserves, but "nature" reserves to protect threatened species. Using island biogeography as a theoretical guide – that is, interpreting nature reserves or parks as high-functioning, insular regions of Nature – these biologists

[7] The ostensibly non-biological tropes of 'modernity' and 'value' are frequently used by conservationists trying to make sense of their work (see Soulé 1985, Sarkar 2002, and MacLaurin and Sterelny 2008).

conducted eco-risk management. The fate of a species was a statistic, and the prayer of a species was the wilderness.

In North America in the 1980s, the nature reserve was expected to be the active ingredient of diversity conservation, but often times, it failed to do any more than protect the habitat of a few populations. This species-by-species approach came under scrutiny very quickly, in large part due to challenges by conservationists working outside of North America where the ideology of wilderness was less influential. Charis Thompson (2002) writes about the management of Amboseli National Park in Kenya, tracking the debate over the conservation of elephants in the 1990s. According to conservation biologist David Western, elephants, isolated in the confines of the park, were consuming and trampling the park's vegetation faster than it could grow back, slowly razing their supposedly protected 'island'. He demonstrated this with a simple long-term exclusion experiment, showing that an area of the park fenced off from the elephants grew to be significantly more lush than areas where the elephants could roam. In their seclusion, the elephants were destroying their habitat and the habitat of other species, and the ecological integrity of Amboseli itself became an issue. Western, together with representatives from the Maasai villages surrounding the park, formulated a protocol for conservation – conservation of the park ecosystem, of the "processes that maintain biodiversity," and of the Maasai's way of life – that dissolved the park boundaries, allowing elephants to come out and humans to come in. By connecting the interests of a wide range of stakeholders into one scheme that looked 'beyond the park', Western introduced a vision for place-based conservation. Other scientists, namely elephant behavioral ecologists worrying about the future of a fenceless park and clinging to a species-by-species approach to conservation, criticized Western and demanded that

he open up his science to the academy for intellectual debate. Western's experiment demonstrating the harmful effects of the elephants was designed as a "see-it-for-yourself" event (Western is the founder of the International Ecotourism Society), and not something he intended to publish in an international journal. Western refused to "expatriate" his knowledge in the canonical form of peer-reviewed publications, because what mattered for him was the place called Amboseli where elephants, plants, and humans live together. But why do we call Western's science conservation biology? Should we call it a 'science of Amboseli'? How far does his science extend, and in what directions? What are biologists doing when they practice conservation?

There are no foundational principles of conservation biology that secure the integrity of its practice once and for all. There can be legitimate attempts to articulate generalizable principles (see Soulé 1985, Sarkar 2002, Norton 2008), but these attempts are made from within the practice, and they reorient conservation to a new set of issues which demand understanding. That there are no foundational principles of conservation biology does not mean that it is a pseudo-science or that conservation biologists float free of the discipline it takes to do science. I'm making the much simpler claim that conservation biologists (just like theoretical particle physicists) don't all do the same thing. They don't even have the same opinion about what conservation is or what it can be. And yet many people call themselves conservation biologists and recognize others to be conservation biologists, even people who disagree with them about serious issues. In other words, the unity of their practice is not founded on the correctness of their beliefs and actions. This claim is not the same as the claim made by historians of science who, often in an attempt to invalidate or relativize the epistemic authority of science, document the paradigm shifts that have reworked the principles or values of a given

practice. On a normative account of practices, the temporality of a scientific practice is not about definitive shifts in its past, but the dynamic constitution of its present. The constitution of the cutting edge is iterative, characterized by the constant appeals and repeals of the norms of the practice. Moreover, even if nobody is in disagreement and even if nobody is wrong, what counts as the *right* course of action can be different in different contemporaneous instances of conservation. So what biologists are doing when they practice conservation is not merely employing a given set of theories, methods, and norms in order to restore natural biodiversity, but working through an entire tradition of conservation practice (whatever that is) and gesturing toward a world (whatever it might be) that is more hospitable to whatever it is that dwells with/in it.

This 'working through' and 'gesturing toward' extend the cutting edge of a practice well beyond the present, in both directions. This temporal extension is not simply the recognition of historical discontinuities or the awareness of future opportunities. It is the literal incorporation of those discontinuities and opportunities into one's practical engagement with the tradition in its present constitution, such that one's performances and interpretations are responding to and anticipating the entire practice. Philosopher Alasdair MacIntyre, in a discussion of the kinetic theory of gases, explains how 'working through' the history of a practice is necessary in order to participate in that practice.

> [At any given time], the [kinetic] theory bears the marks of its previous history, of a series of encounters with confirming or anomalous evidence, with other theories, with metaphysical points of view, and so on. The kinetic theory *not merely has, but is an history*, and to evaluate it is to evaluate how it has fared in this large variety of encounters. Which of these encounters have been victories, [and] which defeats? To evaluate a theory … is precisely to write that history, that narrative of defeats and victories. (1980: 71, emphasis added)

Moving a scientific practice forward (what 'scientists' do), on this account, demands engaging its past (what 'historians' do) and constructing a story not just about what we know, but about why we know what we know. In practice, the effort to understand the historical conditions of possibility for knowledge – the 'working through' – is undertaken simultaneously with the effort to grasp what the future might possibly hold – the 'gesturing toward.' The orientation of a science toward things that aren't yet known to it (e.g., in the case of conservation biology, the world(s) more hospitable to whatever it is that dwells with/in it) is indicative of what it means to grasp possibilities. The things we gesture toward in practice are what Hans-Jörg Rheinberger (1995, 1997) calls the 'epistemic things' of an experimental system. Epistemic things, as the objects of inquiry, demand the full attention of a practice despite the fact they show themselves "in a characteristic, irreducible vagueness" (1997: 28). They are the gaps in knowledge, which, in their absence, become conspicuous.

Interlude on the history of genetics

In early 20th century genetics research, the 'gene' itself was the principle epistemic thing. The term was coined in 1906 by Wilhelm Johannsen, not to name something already discovered but to crystallize into one word the concerns of a group of scientists who were trying to flesh out the implications of Mednel's laws of inheritance:

> The word 'gene' is completely free from any hypotheses; it expresses only the evident fact that, in any case, many characteristics of the organism are specified in the gametes by means of special conditions, foundations, and determiners which are present in unique, separate, and thereby independent ways – in short precisely what we wish to call genes. … The 'gene is nothing but a very applicable little word, easily combined with others, and hence it may be useful as an expression for the 'unit factors,' 'elements' or 'allelomorphs' in the gametes … As to the nature of the 'genes,' it is as yet of

> no value to propose any hypothesis; but that the notion of the 'gene' covers a reality is evident in Mendelism. (Johannsen quoted in Keller 2000: 2)

When the 'gene' was born, it wasn't an actual thing so much as the condition of possibility for normal Mendelian inheritance, whatever that was. For almost an entire century (the century of the gene, as Keller (2000) calls it), the 'gene' was what geneticists (as practitioners of genetics) were oriented toward as something that they wanted to know more about. Within the first decade of research into genetics, Thomas Hunt Morgan was able to pinpoint chromosomes as the material basis of inheritance, the carrier of genes. But in the course of figuring out the mechanisms of gene *transmission* (from cell to cell, and from generation to generation), the problematic of gene *expression* (the translation from genotype to phenotype, from genes to proteins) became much more apparent. If the process of gene transmission is such that all cells of an organism have the same genes, how is it that the cells of an organism differentiate? Morgan was well aware of this problem, and in 1934, two decades before the explosion of molecular genetics and the arrival of the tools needed to answer the question of gene expression, he made a prophetic hypothesis:

> The implication in most genetic interpretation is that all the genes are acting all the time in the same way. This would leave unexplained why some cells of the embryo develop in one way, some in another, *if the genes are the only agents in the results*. An alternative view would be to assume that different batteries of genes come into action as development proceeds. (Morgan quoted in Keller 2000: 56, emphasis added)

Even before the advent of molecular genetics (widely considered the golden era of genetics research, during which the concept of the 'gene' came to be fetishized as the

basis of "life itself"[8]), Morgan was gesturing toward "agents" other than genes (now known to be transcription factors and other proteins, the very products of gene expression) in order to account for the *ostensibly genetic* phenomena of heredity and development. That is, the significance of epigenetic phenomena was first anticipated by someone interested in genetic phenomena. The concept of the gene was already beginning to undo itself as the supreme bearer of life itself.

The 'gene' interested Morgan and other geneticists not as a unitary, clean cut entity with a readily identifiable function within the life of an organism, but as an epistemic thing presenting itself in a "characteristic vagueness." The objecthood of epistemic things should not be construed simply as materiality, as something already there that we handle, however complex or hidden it might be, but rather as mattering, as something we concern ourselves with. As such, we cannot say that what Morgan studied was what genes were actually doing, because he did not know what genes were actually doing. What he was concerned with, and therefore what his research was actually about, was what genes could *possibly* be doing. Today, after much research on the molecular mechanics of gene function that Morgan could barely even gesture toward, the 'gene' has become, for many geneticists, more of a 'technical object' – Rheinberger's term for an entrenched part of the world, which serves not as an object of inquiry, but as tool that can be used in reliable ways to explore something else. Technical objects become the experimental conditions of possibility under which further epistemic things can show up and be studied. Today, genes are mostly technical objects that many geneticists 'work through' in order to inquire about other things, such as the 'genome.' As was the case in classical genetics, genomics is not trying to grasp what genomes actually do (as if the

[8] The concept of gene fetishism as an orientation toward the genome as a map of 'life itself' is taken from Haraway 1997, chp. 4.

molecular mechanics of gene function is all there is to know), but what else they could possibly be responsible for (e.g., disease susceptibility, developmental plasticity, evolutionary potential, etc.).

A normative conception of scientific practices (part II)

The range of possibilities toward which a practice is oriented is what a practice must account for, and therefore, what binds that practice together.[9] Beneath patterns of normal activity, and beyond the issues that arise when concepts and methods are called into question, is the accountability we have to something we don't even know, to something which we necessarily conceive of as a set of possibilities. That feeling of responsibility that we feel in practice comes from our awareness that there are stakeholders in the realization, or coming into being, of those possibilities. We recognize that there is something 'at stake' for us and for others in our ongoing activity as scientists, humans, or whatever it is that we are.[10] As the material-discursive objects of scientific attention, epistemic things embody these stakes. Rouse explains that what is at stake is "not some definite, already articulated goal or value, but something that matters in ways we may not yet fully grasp" (2002: 338). The focus of a practice on stakes – the possibilities that might just be and the ways that current performances matter to these possibilities – does not mean that those stakes are fixed or regular, nor does the integrity of a practice turn on the possibility of everyone agreeing on what the stakes actually are. It is often the case that people doing the same thing (e.g., cancer research) have a

[9] See Rouse 2002: 330-350 on 'real possibilities'. A real possibility isn't the same as a logical possibility or a 'possible world,' a state of affairs that does not actually exist, but could have reasonably existed if things had gone differently. A real possibility expresses the normative issues that are entangled with research.
[10] The phrase 'at stake' is borrowed from Rouse 2002 to "express a normative accountability that outruns any definite formulation of what one is accountable to" (357).

different view of where their practice is headed. In fact, even though both stakes and standards can be at issue, Rouse explains that it is "precisely because what is at stake … [is] assumed to be what 'everybody knows' [that] these stakes often remain only partially articulated and therefore ironically less susceptible to normative constraint" (1996: 143). Our conceptions of what is at stake are often more fuzzy and more divergent than our standards for appropriate research.

It takes great discipline to pursue something which you don't understand. You don't know what you are touching when you touch an epistemic thing, and sometimes you don't even know when you are touching it. Nevertheless, because there is something at stake in understanding it, intellectuals assume the responsibility to try to discover and articulate what their object of inquiry is, to realize its potential in the context of their research. However, that articulation, especially if it is successful, transforms the research situation itself, and it opens up new possibilities for action and interpretation that weren't recognized before. It is in these transformations where the stakes of the practice – the very horizon which held everyone's gaze – recede into the future to reveal something further at stake, something else that beckons for a response. The normative authority to which a scientific practice is accountable always outruns present scientific understanding. Consider scientific discoveries which break through an epistemic logjam and fill in lot of knowledge gaps; we call these discoveries *groundbreaking* because they lay the foundation for further research and reveal a future where the potential feels limitless. Scientific practices matter not because they close the case on unsolved mysteries, but because they open the world to a field of possibilities which beckon to be understood. Thus Rouse says, "what is at stake [in scientific research] is not so much the correct discovery of objects and their properties, but the disclosure of epistemic things as

binding upon us through what is at stake in their disclosure. The stakes of research are not what nature is, but what it is to be nature" (2002: 340). In upholding their responsibility to what is at stake in their practice, scientists discover new parts of nature to deal with and interpret (e.g., new proteins, new stars, new magnetic fields, new neuronal connections, new evolutionary mechanisms). And it is important to recognize that the injunction to deal with and interpret a new part of nature is a normative injunction – for us to respond to it in ways that are accountable to how its being 'there' affects (and could affect) the stakeholders involved. The correct perception of anything turns on a responsibility to what that thing can possibly be.

Being a stakeholder in the unfolding of the present

What does it mean to be a stakeholder in the ongoing constitution of a practice? Stakeholding is a concept that should remind us of subjectivity and agency. It's about having standards of living and reasoning and hopes for the future, and maintaining those standards and hopes in the face of complexity and adversity. In the intellectual tradition extending from Kant, the realm of normativity – where free beings hold themselves to standards or claim to have stakes – is wholly separate from the realm of causal phenomena. The normativity of scientific practices, it is assumed, is about the agency of humans, and any disagreement over norms is a social affair, where rational agents work out their issues. Since nonhumans don't play in this game of giving and asking for reasons, their world is to be explained in terms of causes, not norms. Moreover, disagreement over causes is a scientific affair, to be resolved objectively by reference to laws of Nature, not norms of appropriate behavior.

This philosophical bedtime story will lull us to sleep, as well it should. We are comforted knowing that humanitarian problems (e.g., natural disasters) come before other problems (e.g., the regulation of money trading). And we settle down knowing that the solution to either kind of problem is a problem for humans to resolve in conversation with other humans.

But this familiar story is not a story about practices, nor is a story about practices just a philosophical story (as will become clear in upcoming chapters). This is because the principal characters in a story about practices are not human agents, but stakeholders. And stakeholders aren't the same as humans. For instance, it was always understood that American women and American blacks were 'human' (we never thought they were barbarians), but it wasn't until 1870 (for blacks) and 1920 (for women) that the Constitution recognized that they have a stake (i.e., the right to vote) in their own government. In a more pertinent example, orcas (killer whales) have never been human, but their status as a stakeholder in the contemporary conservation of the Salish Sea (and other parts of the ocean) is quite clear to the humans involved.[11] In contrast to the exclusive practice of being human, being a stakeholder is a matter of there being something at stake in your existence. At stake in the future of orcas, according to many conservation biologists, is the very being of their species. Conservationists inferentially link the species being of orcas to the stability or health of the ecosystem in which they dwell, which is also at stake.[12] "Something would be lost if orcas became extinct," conservationists agree. That something is something that matters to orca and should

[11] The Salish Sea is a term for the waters of Puget Sound up through the Strait of Georgia and the Strait of Juan de Fuca out to the Pacific Ocean. Named for the Coast Salish Native Americans, the Salish Sea is a complex set of waterways that was carved by glaciers, and it is a term used to convey the interconnections between these bodies of water, which would otherwise be chopped up into 'sounds', 'straits', 'inlets', etc.
[12] See Sarkar 2002: 132-3 for a discussion of the trope of health in conservation.

therefore matter to us; that something (whatever it is) is what makes the orca a stakeholder. For those still skeptical about the distinction between stakeholders and humans, let us consider a third example about a situation where it seems obvious that the stakeholders are clearly, only, and without exception, humans. These situations are emblematic of the practices of humanism and of modernity. Consider Auschwitz and the crises it sparked in German culture, Jewish culture, and philosophical culture. At stake in Auschwitz and in the responses to it was what it meant to be human (among other things, including most notably what it meant to be a Jew). And yet, in the course of struggling with these issues, the conception of 'human' changed – what it meant to be someone who could commit, suffer, or even respond appropriately to the atrocities of Auschwitz. As such, the principal stakeholder in the debate was not humanity-as-we-know-it, but humanity-as-we-couldn't-have-imagined-it.

But the shift in attention from humans to stakeholders is not just recognizing the other-than-human as a significant element of scientific practice. This shift entails a rejection of human activity as an appropriate 'component' of any analysis of practice. One cannot break down a practice into the following neatly-bounded components: the beliefs of humans, the performances of agents, and the material settings which contain those beliefs and performances. Instead, one must identify mechanisms (not just 'components') of the practice that that cut across those boundaries in ways that respond to the stakes of the practice. This kind of situated responsibility demands the use of expressions that capture not only what we know but what we care about.

In scientific research, a stakeholder is a placeholder for an epistemic thing, an unambiguous manifestation of what is at stake in the research. In that sense, a stakeholder is a kind of technical object in Rheinberger's scheme. As the equipment through which

epistemic things are grasped, technical objects 'contain' epistemic things: "they embed them, and through that very embracement, they restrict and constrain them" (Rheinberger 1997: 29). Stakeholders are part of the apparatus which allows and simultaneously constrains inquiry into the subject matter. But unlike regular equipment such as tools for measurement, stakeholders are objects whose future is of concern; they are the immediate conduit through which epistemic things show themselves. Stakeholders are concentrated bundles of potential whose realization must be handled carefully, simply because we don't know what the limits of that potential are or exactly how much those stakeholders matter. For instance, in the conservation of the Salish Sea, both humans and orcas are figured as stakeholders by conservationists. Humans are stakeholders because their livelihoods and future activities – and who knows what those future activities might be? finding a cure for cancer? creating a new genre of music? – depend on the health of the ocean and the bounty of food and pleasures that it provides. Orcas are stakeholders for a different reason. They are a conduit through which we evaluate the biodiversity and ecosystem health of the Salish Sea, perhaps not the most rigorous scientific conduit, but one that matters in the public domain. The future of the Salish Sea that conservationists gesture toward and press into are futures that are more hospitable to orcas and humans, because these two stakeholders are clear-cut, unambiguous, albeit synecdochic embodiments of what is at stake in the conservation. Disagreement and uncertainty may be the characteristic features of scientific practice that define its scope, but stakeholders often serve as a more definite checkpoint through which practitioners must move to get things done. Stakeholders are, of course, still of indefinite significance since they open up the practice to what is at stake, but they keep things real, so to speak, by unabashedly demanding people's attention (even if the attention they get paid is to strip them of their stakeholder status). Any conservationist who decides against saving

orcas better have a good reason why. Since the stakes of a practice are often conceptualized as abstract things (like biodiversity, cultural memory, the future), stakeholders are usually objects that are more readily recognized and individuated and more easily touched (like trees, a native species, a crisis victim). It is true that we are bound to stakeholders through what is at stake, but it is often the stakeholders which move us and move us to act in the first place, if only because we can't even fathom the stakes of our actions.

Stakeholding in conservation biology is made explicit, and this explicitness is part of a larger trend in scientific practices away from assuming everyone knows what the stakes are. In addition to all of the nonhumans who are stakeholders in conservation biology, there are many heady tropes used in conservationist discourse that call upon our ethical and sociological imaginations to conceive of what it is at stake in our relationship to nature – tropes like 'biodiversity', 'biophilia', 'sustainability', 'place', and 'landscape'. It is clear that the meanings of these concepts are not entirely determined by scientific research; indeed, it is often expected that nonscientific practices consider these same stakes as well (e.g., 'sustainability' has become a highly valued feature in architectural and engineering practices; and 'biodiversity' is practically begging for philosophical analysis). The conceptual flexibility of stakes testifies to another trend in many scientific practices, not just science studies, toward acknowledging the ways in which science is embedded in wider cultural and political practices. 'Stakeholder', after all, is a concept that originally belonged to social scientific disciplines as a way of modeling the interests at play in conflict resolution. But if scientific practices are open-ended – not only into the future and the unknown, but also to ostensibly non-scientific practices like national politics, popular culture, and philosophy – what constrains scientific research, such that only some knowledge counts as 'scientific'? Surely some good philosophy has affected the

way scientific research is done, but why isn't science simply subsumed under its philosophical critique? This isn't a question about demarcation or the difference between epistemic normativity and social normativity; we recognize and appreciate the interconnectedness of different kinds of practices. But why aren't scientific practices lumped together with other practices into one unified force of History? If normative accountability binds practices together and opens them up into each other's worlds, how is that some discontinuities and ambiguities exist unproblematically? For example, economic evaluations of biodiversity and genetic assays of biodiversity figure very different kinds of objects and are subject to different norms of practice. These intellectual conceptions of biodiversities are also very different from populist views of biodiversity (Wilson 1984) as well as indigenous notions of biodiversity (Apffel-Marglin 2005, Tsing 2005). Using Annemarie Mol's argument (2002) about the ontology of the body in medical practice, we can see quite clearly that biodiversity is a multiplicity of objects. Sometimes this multiplicity hangs together on a single 'coordinated' platform of conservation. Other times, the management and conceptualization of biodiversity is 'distributed' over a number of separate practices. What are the patterns of normative accountability that hold some practices together and keep other practices separate? How does normativity cut up and stir up practices such that sometimes a practice proceeds on its own and sometimes it is engulfed by another?

CHAPTER TWO
Inside the whale's mouth, or, what it's like to be in the grip of a discipline.

> *Oecology* is the etymological spelling of *ecology*. The *logos* of *oikos* is the *law* of the *house* in the deepest sense: meaning the speech by which the house is known to itself, meaning that the house is subject to the proportions of its rule. It's house rules I'm talking about, just and unjust, or finally JUST. I want to recall to you the house, of which the planet is one aspect, language is another, body is another – house in the sense that oeconomy is the management of the house and oikumene is the known, inhabited, LIVED IN world.
>
> – Richard Grossinger (1970: 7)[13]

The dis/unity of science and disciplinary coherence

One aspiration of the classic epistemological approach to science was to conceptualize and promote the unity of science. If science consisted in different bodies of knowledge representing different domains of nature, the unity of nature (which was taken for granted) should make possible the unity of science. As a 'working hypothesis', the unity of scientific knowledge was to be approached by reducing one body of knowledge to another.[14] Epistemological reductionism turned on the possibility that the universal languages of physics could, in the future, dissolve the particularities of the 'special sciences,' even if such dissolution could not be achieved at present.

Fueling the debate about reductionism was the preoccupation of philosophers with scientific knowledge as a systematic representation of the world. Anticipating the completion of all empirical research, reductionists saw their task to be the stitching together of all theories into a unified body of knowledge, whose systematicity was

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[13] This epigraph is taken from the preface of Richard Grossinger's book of prose-poetry that grew out of his notes for an ecology course. The themes of his book are the mythology and magic through which we come to understand the *oecology* of a place.

[14] See Oppenheim and Putnam 1958 for a powerful image of scientific unity as the telos of science and philosophy of science. But see Neurath 1983 and Cat, Cartwright, and Chang 1996 for a vision of 'unified science' that is much more relevant to the normative account of scientific practices offered here.

matched only by the world itself. After the turn to scientific practice and the rise of interdisciplinary science studies, most aspirations to Unity were ditched, and scholars were able to critically examine the patterns of disunity that had always been there. Why is it that biologists studying the thermoregulation of deep-diving marine mammals don't necessarily have to engage directly with a rigorous physical understanding of thermodynamics or pressure mechanics? Alternatively, why is it that other biologists studying the thermal adaptations of intertidal barnacles are readily able to adopt principles of mechanical engineering in order to generate mathematical models predicting the effects of climate change on barnacle communities?

"Within the complex of issues generated by the disunity of science," historian of science Timothy Lenoir writes, "discipline emerges as a crucial site" (1997: 46). What does it mean for research to belong to a discipline? And what does it mean for research to be inter/disciplinary? The separation of disciplines is reflected in the plurality of university departments, academic journals and professional societies. But the sociological state of epistemological affairs is not what concerns me. This essay is not about disciplines as distinct practices about distinct domains of nature. It is about the practice of disciplining ourselves as knowledge makers such that nature can show up intelligibly in domains at all.

Using my arguments from the previous chapter about the normativity of scientific practice, we should recognize that the domain of inquiry instituted by a disciplinary practice cannot be defined by shared commitments or methods. Likewise, the domain of nature constituted by a disciplinary practice does not consist of a static set of objects. Just as the possibility of a reduction of all knowledge into one systematic 'image' had to be relinquished, so too must we give up the assumption that disciplines

are grounded in a fixed set of beliefs or even objects. Disciplining one's practice is a question of training the mind to think in certain way or training the body to have certain skills, and it is a matter of disciplining the complexity of nature and beating it into an intelligible pulp. Discipline is also about getting (and keeping) a job. But the intuitive sense in which disciplines are about the *regulation* of knowledge making should not be collapsed into an interpretation of disciplines as *regularities* of knowledge making. To see strict regularities within a discipline is to abstract the science away from its ongoing practical engagement with the world.

Despite the heterogeneity of outlook and subject matter among its constituent research programs, a discipline coheres as an operative unit. Disciplinary coherence – understood as some connection among different intellectual activities – is a normative phenomenon. As mentioned above, this connection is manifest in the consolidation of university departments and academic journals and conferences and, more generally, in the everyday dealings with people called colleagues. This connection is also manifest in the openness and love that disciplined practitioners show to the things dwelling in their domain of concern. The same way a naturalist will take delight in seeing a rare animal, so too an embryologist sees beauty and meaning in the dividing of cells, even through a microscope. Beneath the disunity of disciplines, we can see these relationships of accountability – practitioners holding themselves open to the work of others and to the objects within their domain.

This deep-rooted accountability – the normative connection between people and between people and things – is what holds a discipline together. To acknowledge the normativity of disciplines, however, is simply to recognize disciplines as recapitulations of practices in general. Moreover, the accountability we feel toward our colleagues and

our objects of inquiry can presumably extend beyond disciplinary boundaries. This kind of boundary crossing is evident not only in interdisciplinary research, but in the social and cultural dimensions of all scientific research. Indeed, we can imagine situations of crisis – an outbreak of disease, an unexpected act of war, a mass extinction event – where it is possible that disciplinary boundaries and, more generally, cultural boundaries are overlooked. In these situations, outstanding disciplinary commitments are set aside, because what matters now is the future of the world itself – not necessarily the globe, but the 'world' we call home (which, in more and more instances, actually is the globe). So if all scientific practices are tied together by this accountability, even if it only manifests in crisis, what distinguishes disciplines as operative units among the variety of practices?

In a way that I cannot yet fully grasp, there is a sense in which disciplined practice is the only kind of practice that can possibly account for everything that matters to it. With a disciplined understanding of a domain, we often get the impression about our practice that 'this is what it's all about'. Even and especially in interdisciplinary practice, we get the feeling that what we're doing just might be getting at the heart of the matter. This feeling of anticipation is not something privy to the experts of the discipline. It is shared by the entire disciplinary practice, and it is often felt most strongly by its students. This is the case of the disciplined Christian theologian, who is in awe of the God he articulates, knowing that his understanding of God is tantamount to his understanding of himself and his world. This is also the case of the marine biologist who observes a sea anemone at the bottom of a tide pool and feels a pleasure that allows her to glimpse a deeper meaning. Perhaps in this glimpse she sees the telos of the sea anemone or the complexity of the ocean, or maybe she just thinks about how

wondrously inarticulable the scene is. In any case, she turns to her colleague and says, "This is why we're marine biologists."

In disciplined practice, our appreciation of our domain of inquiry can be conflated with our concern for the world itself. For the same reasons that such a conflation is possible, a discipline is characterized by its *stability*, relative to other more ephemeral practices, and its *autonomy*, relative to other disciplines. We can recognize these two features of disciplines, while also recognizing the open-ended responsibility a disciplinary practice has to ongoing affairs outside of its boundaries. This simultaneous recognition has itself been recognized as an issue, which is analogous to the tension between internalist and externalist accounts of science. Internalist accounts focus on how a domain of nature is articulated in scientific discourse, while externalist accounts generally tell a story about the social and cultural (i.e. external) concerns that bear upon or even become a part of that articulation. Some externalist accounts go so far to argue that domains of nature are actually constructed by human interests, and that the domain's boundaries appear stable and autonomous only because they have been "naturalized," cleansed of their artificiality (Pickering 1984). What these externalist accounts share with their more traditional internalist counterparts is the presumption that the stability and autonomy of disciplined practice (i.e., its internality) come from the practice's foothold in 'nature'. Internalists defend this foothold, assuming a problematically simple conception of nature – a singular foundational domain disconnected from human affairs. Externalists attack this conception of nature, assuming a problematically simple conception of the foothold – the power of social construction. Clearly, in the interpretation of the stability and autonomy of science, the conceptualization of nature is at stake.

Disciplining nature to follow rules

Natural laws have played a prominent role in the popular conceptualization of nature and in the philosophical interpretation of how scientific practice is stable and autonomous. The conventional understanding is that laws of nature are a special kind of truth that characterize parts of the world that are necessarily the way they are.[15] In contrast to accidentalness of other truths, the necessity of natural laws grants them an explanatory power which makes them significant to science. Natural laws were targeted by logical positivists as crucial factors in the unity of science; the fundamental truths which will reduce all scientific knowledge to a unitary theory will be laws of nature, whatever they turn out to be. Since the domain of physics was governed most strictly by laws, it was to this domain that nature would be reduced.[16] The domains of biology are typically assumed to be too riddled with contingencies to be governed by necessary laws.

For instance, consider Mendel's principles of inheritance which were rediscovered in 1900 and taken to be generalizations governing all organisms. Early geneticists conducted research on that assumption, attempting to articulate the mechanisms of Mendelian inheritance and sort out any implications. As we know, genetics was a wildly successful field of study for the rest of the century (Keller 2000). And yet there are exceptions to Mendel's laws; not all genes are inherited as regularly as Mendel thought. His laws only appeared true in a controlled experiment where he could not account for many of the genetic phenomena we are now aware of. Indeed, geneticists today do not conduct research where the validity of Mendelian principles is at

[15] The necessity of laws isn't the same as logical necessity or metaphysical necessity. It is reasonable to ask about what the world would be like if laws were different.

[16] The phrase 'domain of physics' belies the disciplinary plurality within physics itself. For instance, Cartwright 1994 discusses the distinction between mechanics and fluid dynamics, arguing that they are different domains with different laws.

stake. One might also argue that Mendel's principles are an accident of evolution, and that inheritance doesn't necessarily have to happen in this way. According to Marc Lange (2008), the problems of exception and contingency are the issues most philosophers cite when they refuse to interpret biological generalizations as laws. Who says life had to be this way?

But as Nancy Cartwright (1994) argues, even the laws of physics only apply in certain circumstances. The precise laws and equations well known to students of physics are generated in highly contrived settings where all inputs into an experimental system are controlled. We tend to think that those same laws apply everywhere, and that the controlled system just makes the calculations simpler, but strictly speaking, argues Cartwright, laws should be understood as *ceteris paribus* claims: if nothing interferes with the system except those factors identified, then the law holds true. For instance, Newton's third law, F=ma, can describe the motion of a penny dropped from the roof of a building. For other situations, however, like the motion of that same penny falling to the bottom of the ocean, Newton's third law cannot provide an adequate description. In the ocean, there are factors influencing the motion of the penny that she argues cannot be modeled as a force, and that penny's motion will fall outside the domain modeled strictly in Newtonian mechanics. It is not that Newton's third law is not true, but its truth is limited to its domain. And the domain of Newtonian mechanics is not a domain of universal scope.

If we pay attention to the disciplinary practices in which laws are invoked (as Lange and Cartwright both do), laws don't appear to function as absolutely necessary

truths, but as relatively stable generalizations that are norms for further generalizing.[17] In other words, laws aren't simply emblems of already collected scientific knowledge, but tools in ongoing scientific reasoning. Since scientific reasoning is always conjoined with an experimental system (Rheinberger 1997) or a set of models (Cartwright 1994) – some material setting in which reasons can be embodied – at issue in research is the characterization of the specific domain indicated by the experimental system or models. This limit to the generality of scientific reasoning marks the scope of a discipline and its laws. If domains and their laws are finite, then the stability and autonomy of scientific disciplines cannot be founded upon a singular Nature with universal foundations.

Then what kind of nature is it that grounds a discipline? One answer has been that domains and laws are social constructions, and that 'nature' is a persuasive fiction of power and language. But constructivism and antirealism, in bypassing serious ontological consideration about what nature *is*, cannot provide an adequate account of the stability of disciplined practice. More than a collective delusion, nature is visible and audible and complex and dangerous. We can recognize aspects about it that we don't yet know, and sometimes aspects that we might not even want to know. In short, nature is real (Cartwright 1994, Barad 2007), and that reality needs to be theorized.

As I have argued, the reality of natural domains should not be theorized as a set of pure regularities that human intellect stumbles across. Domains – of Mendelian

[17] That there is no special truth value attached to lawful propositions is exemplified by laws about abstracted objects. For instance, the Hardy-Weinberg principle of genetics – that gene frequencies in a given population remain the same from generation to generation – is never true of any population. It invokes a highly abstracted population of organisms where there is no natural selection for specific traits, no genetic mutations, and no emigration or immigration. The Hardy-Weinberg principle, which became a central law of the newly forming discipline of genetics, is not appealed to in ignorance of how real populations work. (The principle wasn't originally formulated to be *about* real populations, so getting it wrong about real populations wasn't an issue.) Instead, it is invoked as an abstract situation of genetic equilibrium against which to monitor genetic change. It is norm of genetics and of reasoning about genetics that reveals a host of possible factors that do disturb the norm. Other laws about abstracted objects include Hooke's law about the restoring power of springs and the ideal gas laws.

genetics and of Newtonian mechanics – aren't simply found, but painstakingly figured. This is why their associated laws only apply *ceteris paribus*, within that figured experimental system or set of models. But the traditional rhetoric of *ceteris paribus* retains the naïve sense of nature-as-regularity(-at-least-when-all-other-things-are-the-same) and allows us to sidestep the ontological consideration of the broader nature beyond the confines of the experiment or model.[18] This philosophical refusal to theorize the ontology of an irregular nature is problematic, because scientific research is usually oriented toward something which is not well understood. As Rheinberger (1997) argues, the object of inquiry in any research program is an epistemic thing whose behavior isn't quite regular, if only because it's material and conceptual definitions are unsettled. Moreover, this encounter with something presented in 'characteristic vagueness' (and the ensuing struggle with what it could possibly be) is exactly the context in which laws are invoked. What is interesting and useful about laws is their extension over a wide range of possible situations, especially counterfactual situations that go beyond whatever the actual circumstances were in which the law was first articulated (Lange 2000, 2008). As such, laws are a basic tool for reasoning inductively about epistemic things as possibilities. Even Cartwright acknowledges that some invocations of laws (and following Lange, I would say all invocations of laws) aren't just significant within their modeled domain, and that these laws make bona fide ontological claims about 'nature' at large. When we take Newton's third law, F=ma, to be a law, we commit ourselves to the belief that

> [it] is in the *nature* of a force to produce an acceleration of the requisite size. That means that *ceteris paribus*, it *will* produce that acceleration. But even when

[18] However, see Lange 2002 on why we shouldn't worry about *ceteris paribus* clauses. And see footnote 25 for an overview of Lange 2002.

> other causes are at work, it will 'try' to do so. … To ascribe a behaviour to the nature of a feature is to claim that that behaviour is exportable beyond the strict confines of the *ceteris paribus* conditions, although usually only as a 'tendency' or a 'trying'. (Cartwright 1994: 285-6)

Such talk about laws reveals two things about the nature of domains. First, it gestures toward a certain degree of agency held by nature, and it does so without being polemical, that is, without begging much of a critical response. Second, it suggests that domains are constituted in significant part in their openness to other situations and other inquiries that have not yet been subsumed into the domain. In other words, the *internal* character of a discipline – its stability and autonomy and other features associated with 'nature' – is achieved in large part by maintaining a relationship to external matters. In accord with a normative conception of practice, this relationship of a domain to its outside is one of accountability. As such, when we interpret the nature of something (e.g., with a natural law), we must be aware of the human concerns that exist (not 'outside of' but) alongside that nature, and which must account for that nature.

The stability and autonomy of disciplinary laws

Marc Lange (2000) has offered an interpretation of natural laws as rules of inference that are rooted in the commitment to understanding a certain range of possible situations. When we take a hypothesis to be a law, we commit to it as a reliable assumption to make in the context of our inquiry. Law-like assumptions indicate a belief that the phenomenon being studied would go on in the same way as the same kind of phenomenon in other circumstances. In other words, the reliability-in-context of law-like assumptions means the stability (or invariance) that they maintain under various counterfactual suppositions. The set of counterfactuals that are considered indicates the

range of possibilities that a given field of research is trying to understand. By projecting hypotheses over a range of possible situations, laws are crucial tools in the conceptualization of epistemic things.[19] [20]

The notion of stability as the reliability of an inference rule in a host of possible situations makes intuitive sense for laws of physics, but do biologists use laws to reason about their epistemic things? As mentioned above, biological generalizations tend to have exceptions and many of them (e.g., orcas are black with white spots) can be construed as accidents of evolution. Biological processes are so messy, the positivist sentiment goes, that one cannot extract necessary laws of nature. (Even if one could, the reductionist sentiment adds, they would be laws of physics.) But messiness – or variability, as one calls it in the profession – doesn't matter to some biologists. More accurately, variability can matter in different ways in different contexts of inquiry.

For example, two organismal biologists studying the functional requirements for developing marine invertebrates recently published a paper in *Science* titled "Predators Induce Cloning in Echinoderm Larvae" (Vaughn and Strathmann 2008).[21] In the middle of the article, they report their results – that 40% of sand dollar larvae cloned themselves

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[19] Lange doesn't use the term 'epistemic thing,' but he makes a similar point about the projection of laws in his explanation of why the laws of nature are important to science (2000, chp. 3 and chp. 5). By projecting over a range of counterfactuals, laws are the beginning of inductive reasoning. Moreover, "there are no prerequisites for a scientist to be justified in launching an inductive strategy. It is a free move—she is entitled to pursue a given inductive strategy even if she lacks any relevant prior opinions—precisely because in making a discovery bear confirmationwise upon some other claim's truth, an inductive strategy does not depend on any prior opinions" (2000: 154). Reasoning about an epistemic thing, we recognize, is inductive reasoning.

[20] Rheinberger also wouldn't use the term 'epistemic thing' in this way. For him, epistemic things are embedded in material experimental systems, not a field of study. Indeed, Rheinberger argues that "in following the development of 'epistemic things' rather than that of concepts, topics, problems, disciplines, or institutions, boundaries have to be crossed: boundaries of representational techniques, of experimental systems, of established academic disciplines, and of institutionalized programs and projects" (1995: 53). This is an important observation – that in the course of research, disciplinary boundaries are crossed – but it should be clear that Rheinberger is using a more conventional sociological conception of discipline.

[21] Echinoderms are a phylum in the animal kingdom that includes sea stars, sea urchins, sea cucumbers, sand dollars and their relatives.

when exposed to seawater with a given amount of fish mucus (predator cues), compared to 0% in control groups which weren't exposed to fish mucus. This claim – that predators induce cloning in sand dollar larvae – certainly does not purport to be a physical necessity; only 40% of larvae actually underwent the physical process of cloning. Nor does this claim purport to be a biological necessity, as if all biological research turned on the validity of this hypothesis. Nevertheless, this claim does bear a kind of necessity (i.e., it is law-like) for some researchers. Together with other law-like hypotheses in sand dollar ecology and larval functional biology, this claim is necessary insofar as it is projected over the phenomena being studied in those fields and will be taken into account in some of the ongoing research there.[22] Its being necessary does not mean that its truth will be confirmed in subsequent research, but only that its truth is confirm*able* by instances discovered in other research. And if it is demonstrated to be false (or to be somehow inappropriate or incomplete), such a demonstration will be noteworthy and will rework the scope of the law's projection. For instance, if other researchers were to find that the only sand dollar larvae to clone in response to predator cues were the ones whose parents had been exposed to the same predator when they were larvae, then we would recognize that the original researchers were studying a more specific phenomenon than the original law-like claim suggests.[23]

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[22] Lange explains that the necessity of a law is derived from its being part of a set of laws, such that the set is stable under any counterfactual hypothesis consistent with that set. See next paragraph and footnote 24 for an example of how laws come as a set.

[23] Note that such a specification of the original claim doesn't necessarily narrow the scope of the law or make it less of a law. In this hypothetical case, in the course of reworking the scope of the law (i.e., reassessing which counterfactuals are relevant), we might recognize that this updated claim – that larvae only clone in response to the predators that their parents had been aware of – now bears on the field of invasive ecology and the phenomenon of introduced species. If a new sand dollar predator is introduced into its habitat, larval cloning is no longer a possible form of defense for the species, making sand dollars less adapted to their environment.

Lange also argues that laws are explanatory in a way that accidental generalizations are not (2000: 16-18, 268-271). The explanatory power of a generalization – the sense in which the fact being explained *had* to be so and could not have been otherwise – is closely related to its necessity – its bearing on a range of possible situations. But whereas necessity highlights the projectability of a claim and its stability therein, explanatory power highlights the sense in which that range of invariance, no matter how narrow or gerrymandered it is, is *right* or appropriate. The correctness of a claim's range of invariance is indicated by phrases like 'no matter what'. Suppose I have an urn full of marbles that are all green. The claim, "choosing a marble from the urn, I would have gotten a green marble *no matter what*," is correct, and it explains the situation (Lange 2000: 269). But the 'no matter what' cannot be interpreted literally; if I had added red marbles to the urn, I might not have picked a green one.[24] But the context of the claim supplies an appropriate range of possibilities to be subsumed under the phrase 'no matter what', and the claim becomes explanatory. In contrast, it is not quite correct to claim that "predators induce cloning in echinoderm larvae *no matter what*," because the relevant research has not yet defined an appropriate range of invariance.[25] This law-like claim does not yet bear the same explanatory power as does a more entrenched law.

[24] This example demonstrates the fact that laws come in sets. With 'no matter what' claims, there is a tacit understanding of which other laws are in the relevant set. It is this set as a whole which limits the stability of the specific law being considered to an appropriate range of counterfactual hypotheses.

[25] In this context, there is clearly a role for a *ceteris paribus* qualifier that constrains the applicability of the law without associating the law with a strict regularity, that is, without divorcing the claim's necessity from the ongoing research into the matter and the interests therein. Lange 2002 argues that this is how all *ceteris paribus* qualifiers work, and that they can be easily recovered in a normative account of scientific practice. As he argues, *ceteris paribus* qualifiers introduce an open-endedness into a law-like claim that allows for pragmatic concerns to identify which other (*cetera*) situations are relevant and which aren't. In this sense, 'no matter what' clauses and '*ceteris paribus*' clauses are used in similar ways, although the former certainly indicates a higher degree of confidence.

What these examples show is that laws and law-like claims are put to work not just in physics, but in inexact sciences and everyday reasoning.[26] Like physicists, biologists (and everyone else) are interested in generalizing their claims such that they bear on a range of possible situations and phenomena.[27] Since different sciences are committed to understanding different possible situations, they make different generalizations and use different laws. However, it is wrong to assume that laws of everyday reasoning are really just low-level versions of more complicated laws of physics, or that laws of biology derive their necessity from physical necessity. Such assumptions amount to a hangover from naïve conceptions of nature as a causal universe that is unassociated with the normative attentiveness of science. Such assumptions also weaken the intuitive sense in which disciplines are *autonomous*, free of the concerns of other disciplines. Lange (2000, chp. 8) defends the autonomy of disciplines and their laws as a function of the 'interests' of a discipline insofar as those interests pick out a relevant range of possibilities to understand (i.e., a relevant range of counterfactual invariance). For instance, consider the laws of island biogeography (IB) as rules of inference in ecology (2000: 264-5, 2008: 497-501). What the discipline of ecology is 'interested' in is the distribution of species across the landscape, and the laws of IB help us make generalizations about the species compositions of various places based on their situatedness within the larger landscape (insofar as the landscape can be conceived of as a composition of 'islands' or 'patches'). As norms for generalizing, the laws of IB are stable under a certain range of counterfactuals, but some of these counterfactuals are

[26] See Lange 2000: 30-33 for a variety list of laws of nature.

[27] One might object to the example about sand dollar larvae on the grounds that such a generalization is just a statistical association and not indicative of a causal regularity. Lange (2008: 492-4) addresses this issue with the basic claim that laws about particular species (which generally come in the form of "The S is T" or "S's are characteristically/typically T") are about norms not causes.

inconsistent with the laws of physics. For instance, says Lange, "had certain birds possessed antigravity organs assisting them in takeoff," the laws of IB would still be true insofar as the distribution of species does not depend on whether or not birds have such organs (2000: 263-5, 2008: 499). Since the laws of IB can be projected onto physically impossible situations, their necessity cannot be derived from the necessity of the laws of fundamental physics. Possessing an irreducible kind of necessity is what makes a discipline autonomous.[28]

We can recognize the autonomy of disciplines in two other less fantastical examples. Lange (2008) discusses how accidents of evolution can be laws of functional biology (e.g., morphology, sociobiology, etc.). Generalizations about individual species – such as "the orca has a large black dorsal fin" and others modeled as "the S is T" – are exposed, correctly, as evolutionary accidents that didn't necessarily have to be that way. When piecing together the evolutionary history of the dolphin family (of which the orca is a member), one cannot assume that orcas always had a large black dorsal fin. Nevertheless, when tracking down orcas in the field to examine social behavior of pod members, one had better be confidently equipped with the counterfactual supposition that "if I see a large black dorsal fin, it is an orca." Depending on what the disciplinary interests are, the same inference rule can be more or less reliable. In a similar example, Lange posits that it is a law of cardiology that "epinephrine will help a patient overcome anaphylactic shock" (2000: 229). A cardiologist counts on epinephrine having the same

[28] Disciplinary autonomy is not the severance of all ties to other intellectual projects; that would be intellectual suicide. Even though the laws of island biogeography can be projected of physically impossible situations, they still require some sort of continuity with the laws of physics and chemistry. For example, the distance law of island biogeography – stating that the farther away islands are from the mainland, the lower the rate of immigration – would be different if the amount of energy a bird needed to fly over a given distance was a wildly nonlinear function (Lange 2000: 265). As I will go on to argue, all disciplines have interdisciplinary connections, even if they remain latent. Intellectual autonomy is more like an all-access pass than a license to make unilateral decisions.

effect on every patient. Evolutionary biologists see this fact to be contingent on selection pressures during the early evolution of mammals, but a cardiologist doesn't care about different evolutionary possibilities. Indeed – and this is where the normativity of disciplinary concerns shows up prominently – a cardiologist *shouldn't* care about other evolutionary possibilities, because to address this evolutionary concern would be to stray too far from the domain of cardiology and to waste time. Even if our current understanding of evolution were false, the laws of cardiology would still hold with their appropriate range of invariance: had God created the human heart just the way it is, epinephrine would still work the way it does. These two examples demonstrate that the stability of generalizations in functional biology and medical biology is autonomous from the principles of evolution.

Disciplinary interests versus disciplinary stakes

The stability and autonomy of a discipline – exemplified in this case by way law-like claims work in larger patterns of reasoning – are, as Lange argues, entangled with the 'interests' of that discipline. Even the most enduring claims of science are normatively constituted within disciplinary practice. But Lange's use of the term 'interests', chosen to reflect the purposes and intentions of a group of researchers, misconstrues the normativity of practical reasoning. As I argued in the previous chapter, there need be no common beliefs or desires that tie a community of scientists together. Within a discipline, there can be disagreement over what the research should be oriented toward and what the underlying purposes of the research are. Lange is quick to tell us that

patterns of scientific reasoning are not founded simply on causal regularities in Nature.[29] But it would be a mistake to assume that they can be determined only by the interests of a community of researchers, as if those interests were not contested. Because laws are norms of reasoning *in practice*, what counts as a law isn't entirely up to the human participants, nor is it ever fixed once and for all. All of the stakeholders of the practice, including the other-than-humans that are involved, contribute to the constitution of laws. As such, the normativity of reasoning is not just a matter of the interests *of* a discipline, but of what is at stake *in* the discipline (Rouse 2002). The laws of cardiology are distinct from the laws of evolutionary biology because the stakes in the treatment of human heart disorders are not the same as the stakes in the ongoing diversification and adaptation of living things.

This conceptual tension between the interests of humans and the stakes in a discipline can be clarified by considering the normative issues of conservation biology. In an early manifesto for conservation biologists, Michael Soulé (1985) outlines a disciplinary identity for conservation by articulating what its objects of inquiry are, which basic principles govern them, and why conservationists are concerned with them. In order to isolate a discrete domain for conservation biology, he carefully distinguishes between conservation and traditional sciences that manage natural resources. Unlike the utilitarian management of the handful of species that humans use, conservation biology, he argues, is concerned with nature for nature's sake and with nature as a macroscopic, complex system. It is not just about human interests in parts of the natural world, but also about the responsibility humans have to other stakeholders in the conservation of the environment. Because of this ecocentric ethic and holistic ontology, the stakes in

[29] See Lange 2000: 5-9 on how metaphysical appeals often trivialize the power of laws in scientific reasoning by trying to prove metaphysical claims independent of the concept of a law.

conservation biology are vague – not necessarily matching up with what everyone or even anyone thinks they are – and in the course of being articulated, they can radically reorient conservation practice (see chapter 1).

For all their vagueness, the stakes in conservation are what allow conservationists to perceive the issues at all. Soulé explicates the stakes of conservation as a series of 'normative postulates': diversity of organisms is good; ecological complexity is good; evolution is good; and biotic diversity has intrinsic value (1985: 730-1). These postulates offer a flexible and powerful image of a world where everything that should be conserved is conserved. With this orientation toward 'the environment' as a counterfactual world that is more hospitable to whatever it is that dwells with/in it, conservationists can go about interpreting the actual environment as it relates to the counterfactual environment. This interpretation proceeds via a series of law-like claims that Soulé calls 'functional postulates.' Two of these claims are concerned with the scale of ecosystem and population processes. The first – "many ecological processes have thresholds [of space] below and above which they become discontinuous, chaotic, or suspended" – emphasizes the size of habitat as a definitive factor in the survival of a species (729). The second – "genetic and demographic processes have thresholds [of size] below which nonadaptive, random forces begin to prevail over adaptive deterministic forces within populations" – emphasizes that small populations are not controlled by natural selection, and that they're more vulnerable to extinction (730). These functional postulates suggest a protocol for conservation: to attend to small populations (read 'endangered species') by supplying them with an appropriately sized habitat (read 'nature preserve').

However, Soulé's general laws are not sufficient for reasoning practically about how to restore damaged ecological phenomena. The nuances of each situation are often too salient to be ignored. These 'local' considerations necessitate using more specific rules of inference in order to conceptualize the problem and potential resolutions. In a recent article in *Aquatic Conservation*, marine ecologists report that in the Salish Sea, two invasive species of algae "achieve higher densities in [marine] reserves" (Klinger et al. 2006). There is nothing overtly law-like about this statement; it is about individual species in a particular place. Nevertheless, it is a generalization about those species in that place, and we recognize it as part of an inductive chain of reasoning that might reveal something significant about patch dynamics of the Salish Sea ecosystem.[30] The stakes that ground this pattern of inductive reasoning are also much more specific than Soulé's 'normative postulates' suggest. In a way that is unfamiliar to many scientists, most of the stakeholders in any given instance of conservation biology are quite literally members of a local community. In some circumstances, a conservationist's concerns (i.e., what her science is about) barely extend beyond the horizon.

Sahotra Sarkar (2002) underscores the necessity for conservation biologists to localize their concerns. He introduces his essay in the same way that Soulé (1985) introduced his, by comparing conservation biology with medicine. Whereas physiology and medicine are constrained first and foremost by the "imperative of generalization that conventionally marks good science," medicine and conservation have the primary objective of "ensuring that the peculiar individual entity that they are treating survives"

[30] 'Patch dynamics' is a concept in landscape ecology and conservation biology that is concerned with the large-scale heterogeneity of the environment. Patches are portions of land (or sea) that can be distinguished from adjacent portions of land (e.g., farmland next to forested land) by any number of criteria. Nature reserves are generally considered to be significant patches since they are ostensibly free of human development.

(Sarkar 2002: 133). Sarkar goes on to articulate 'place' as that entity which is being treated in conservation, noting that 'place' is not like other objects of scientific inquiry.

> A place is geographically rooted and loses its sense of place exactly as it is generalized about: a place is a specific region on Earth's surface filled with the particular results of its individual history. To generalize about a place involves abstraction from these particulars: the more we abstract, the more we lose the peculiarities that made it that place. But places – precise biogeographic locations – are what matter for conservation: they alone retain the heterogeneity that provides the intuition for bio*diversity*. A preference for a place is not merely a preference for an ecosystem or even a habitat, both of which are supposed to admit abstract characterization: the same habitat at different places may hold a different complement of genes, species, communities, or whatever other unit that may be of conservation interest. As noted before, worry about unique entities such as places takes us along a direction opposite to that of conventional scientific generalization. (2002: 134)

Concern in conservation is concern for a precise biogeographic location. At stake in conservation is, among other things, a place. Since places denature as one makes generalizations about them, one cannot pursue an understanding of place without norms of reasoning (i.e., laws) that account for the particularities of that place.

Disciplinary constraint and intellectual freedom

Participating in a discipline entails learning the rules of the game and following them. This reflects the intuitive meaning of discipline, as a constraint on one's actions and beliefs. Being initiated into a disciplined practice is largely a matter of specialization, or learning how to follow these rules well. This is why university departments, the channels through which students must pass, often correspond to recognizable

disciplines.[31] Even if we slip into a traditional understanding of natural laws – rules that just describe the way things turn out – it is easy to appreciate how a student must struggle with natural laws as normative rules that govern behavior; the student knows that she really ought to learn those rules in order to be a good student. The student is told that learning the rules of the game isn't just an exercise in self-discipline, but that there really is something at stake in the practice. Nevertheless, the student occasionally wonders, maybe learning the rules of the game is just a matter of getting a job or fulfilling the wishes of others.

This distinction between two kinds of rule-following is analogous to Alasdair MacIntyre's distinction between the two kinds of reasons there are for participating in a practice, namely, internal goods and external goods (2007 [1981], chp. 14).The example he gives is about a young boy participating in chess. At first, while he's just learning the rules, we encourage his participation by offering him candy to play with us, and more candy if he can win (in a game played at the appropriate level of difficulty). The candy is an example of an external good, a reason to participate that not endemic to the practice of chess but something that will motivate the boy to learn. However, the hope is that the boy will eventually begin playing chess not for candy, but for the love of the game, for the joy that comes with strategizing and competing. MacIntyre calls these internal goods, and they're internal for two reasons: "first, we can only specify them in terms of chess … ; and secondly because they can only be identified and recognized by the experience of participating in chess" (188). Disciplined practice should be done for the sake of internal goods. The trope of internality, first discussed in terms of a discipline's stability and autonomy, shows up again in terms of a disciplined practitioner's raison d'être. Indeed,

[31] This is also why many recognizable disciplines are denoted with the suffix –logy, from the Greek *logos*, which, among many other things, can mean 'rule'. See the epigraph of this chapter.

the internal goods of a discipline are like the stakes of a practice. What they are isn't always predictable beforehand or clear to those involved. Thus, playing chess for the sake of internal goods isn't just about winning, but about "excelling in *whatever way* the game of chess demands" (188, emphasis added). Despite being vague, stakes and internal goods often come to be very important parts of a person's life. Just like threatened parts of nature really matter to a conservationist, so too does the integrity of the game of chess matter to a chess master.

It is true that disciplined rule-following is necessary in order to really appreciate the internal goods of a practice and to even glimpse what's at stake in that practice. But it is also true that in the course of figuring out what the stakes are (and by upholding our responsibility to the stakeholders), a practice can reorient itself so drastically that the rules of the game themselves are changed. Such reorientation often occurs within a state of crisis, where no one is sure what the rules are or even why the rules were what they were. But such crisis does not signify the breakdown of discipline or the collapse of rule-following. On the contrary, it is only disciplined practice that has the capacity for such crisis and such reorientation. Likewise, any reorientation is premised on the most basic kind of rule-following there is, where the reason for continued participation is not a good internal to the discipline (since the discipline has no firm boundaries in crisis). The reason one participates in a discipline in crisis is because one is committed to the possibility of there being rules at all.[32] It is these not-yet-discovered rules that one is steadfastly trying to articulate and follow at the same time in crisis. Thus, participating in

[32] This notion is drawn from John Haugeland's discussion (1998, chp. 13) of the various kinds of rules and rule-following that constitute scientific domains as well as the domain of chess. He calls this commitment an 'existential commitment'. He likens it to faith and love, as the "capacity for individual freedom: the freedom, namely, to take responsibility for the norms and skills in terms of which one copes with things" (1998: 2).

a discipline entails not only learning the rules and following them, but also making sure that the rules are working and reworking them if necessary. Such is the intellectual commitment.

If one side of disciplinary normativity is constraint by norms, then the flipside is the freedom to evaluate those norms and articulate new ones. This tight relation between disciplinary constraint and disciplinary freedom can be understood as a normative interpretation of the internality and externality of a science. Just as nature and its laws are only intelligible in relation to the 'interests of humans', so too is a discipline always held responsible to matters outside of its domain. This responsibility to 'the outside' – the other things that are also there – comes to the fore in crises of knowledge, when disciplinary boundaries are at stake. The basic accountability that binds disciplines together is what we call interdisciplinarity. Contrary to conceptions of interdisciplinary research as moving beyond the stifling rigidity of disciplinary structure (once and for all!), interdisciplinary imagination is only possible from a disciplined stance. And contrary to conceptions of interdisciplinary research as the wishy-washy proliferation of 'academic interests' or 'subject matter,' disciplinary territory is only secured by the ongoing maintenance of congenial or at least outgoing relations with other disciplinary powers. That is to say that discipline and interdisciplinarity function together in the production of knowledge.[33] Such an all-seeing gaze is characteristic of intellectual culture in general, where the focused governing of internal affairs is always coupled with a vigilant

[33] Wholesale backlash against disciplinarity as well as interdisciplinarity is often grounded in the assumption that disciplines and interdisciplinary fields are communities of intellectuals sharing the same (old-guard, free-floating, etc.) beliefs, unchanging in their ways. Having paid attention to the normative issues of disciplinary coherence, we recognize all fields of study (indeed, all research endeavors) to be sites for disagreement and compromise. But for an interesting debate over these issues (where both parties make this assumption that intellectual communities are tied together by common beliefs), see Jerry Jacobs' article (2009), "Interdisciplinary Hype," and the weblog response of Tenured Radical (2009), "We're Here, Because We're Here, Because We're Here, Because We're Here! Or, Why Disciplines Rule the University Roost."

orientation toward everything else out there that someday might be settled by reasoning through it.

Disciplinary crisis and crisis disciplines

There are (at least) two kinds of disciplinary crisis. One is the kind that affects 'normal' science, in which anomalies are discovered and can only be accounted for from a more 'revolutionary' stance. In this kind of crisis, which has sometimes been theorized more generally as 'epistemological crisis,' the boundaries of the original domain(s) show themselves to be mistaken; as a result, a new domain is formed.[34] The revamped discipline is often the culmination of the development of the original discipline, insofar as it steals the thunder (i.e., attention, funding, etc.) of the original discipline. This culminating is a shifting of the stakes and a reorientation of the practice. Such was the case for physics in the beginning of the 20th century when quantum phenomena began to show up in experiments. In order to account for these phenomena, many domains of physics that had been figured to be fundamental were supplanted by the quantum domain. Moreover, the project of Newtonian mechanics culminated in quantum mechanics, insofar as much of the exciting research was being done in the quantum domain. This kind of disciplinary culmination and reorientation is often characterized by the consolidation of preexisting domains into a singular hybrid domain. The concept of

[34] Despite the Kuhnian terms used in the previous sentence, epistemological crisis should not be conflated with the more extreme case of 'paradigm shift.' For a careful treatment of the subtleties of epistemological crisis, see MacIntyre 1980. As he explains, disciplines – indeed, all traditions – are sites of disagreement, and the history of disagreement itself helps to constitute that tradition (62). As such, epistemological crisis does not entail the sudden and total abandonment of all disciplinary norms, as if there were a fixed set to begin with. In fact, in crisis, "what is carried over from one paradigm to another are epistemological ideals and a correlative understanding of what constitutes the progress of a single intellectual life" (70). Crisis demands that we 'work through' (see chapter 1) the history of the tradition and construct a "dramatic narrative" that explains why we thought what we did. Crisis resolution, MacIntyre argues, proceeds not only by the justification of new claims, but also by the justification or explaining away of our mistakes.

consolidation has played an important role in the interpretation of intertheoretic relations, most notably as a metaphor for reduction. William Bechtel (1993, 2006) interprets the formation of modern cell biology in the middle of the 20th century as an 'integration' of science from many different domains.[35] George Palade, a Nobel Prize-winning cell biologist who did pioneering work in cell fractionation, compared the new field of cell biology to a bridge connecting the two "bridgeheads" of cytology and biochemistry:

> The cautious and the careful [researchers] have stayed close to the bridgeheads because the area around them had been consolidated over centuries by the work of their predecessors. The bold and venturesome have ventured on the bridge itself from both directions, because they believed that there was where the action was going to be. … As in the old Latin proverb, fortune favored the bold: the bridge proved to be strong enough to support the intense, occasionally frantic activity of whole armies of explorers. (quoted in Bechtel 1993: 286)[36]

In disciplinary reorientations like the quantum revolution and the founding of cell biology, it is clear that any interdisciplinary connections are being made from a disciplined stance (the most disciplined stance!), as intellectuals seek to stabilize and modernize the place they call home, even if it means rewriting the house rules.

There is another kind of disciplinary crisis that is not about the reorientation of preexisting disciplines, and, therefore, not often theorized in connection with scientific

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[35] Bechtel uses the metaphor of integration rather than unification to emphasize the sense in which the connections between disciplines are local, not universal. However, see Neurath (1983) and Cat, Cartwright, and Chang (1996) for a more subtle interpretation of the universality of unification in terms of the normative concerns that bear on any given instance of unification.

[36] Cytology was the 'study of cells' – the anatomical and morphological characterization of cells using microscopes – before cell biology commandeered its domain of inquiry. Biochemistry, itself a hybrid discipline (see Bechtel 1986), also studied cells – the chains of chemical reactions involved in a cell's life – but biochemical techniques destroyed their anatomical and morphological characters. Modern cell biology was formed by connecting biochemical phenomena (unreachable by microscopes) with cytological phenomena (just reachable by microscopes). Whereas biochemistry continues to thrive as a field of study, cytology became a dead discipline as modern cell biology took shape. In other words, cell biology culminated the project of cytology.

'revolutions'. If culmination and consolidation are the tropes of disciplinary reorientation, this second kind of crisis is all about inception and diversification.[37] It still involves the staking out of a new domain, but not in such a way that replaces old domains or steals their thunder. At stake in this kind of crisis is the birth of a discipline, the inauguration of a new voice of nature. It's not that we were hearing this part of nature incorrectly before or that we weren't listening to it at all. Rather, a new part of nature is just beginning to speak, and we are scrambling to find disciplinary ground from which to hear it. It is not simply an epistemological crisis, but an ontological one as well.[38] In addition, the issue is no longer about how intellectuals deal with disciplinary crisis, but also about how intellectuals participate in a 'crisis discipline.' This is the kind of crisis in which conservation biology finds itself – not as traditional (disciplined) as the rest of the academy, but desperately in need of intellectual attention. This is also the situation of many other interdisciplinary fields of research, including, for example stem cell biology and black studies. It is no surprise that for disciplines like these ones, the normative issues of their ongoing practice are more salient. Crisis disciplines are keenly aware of the stakes (and the time).

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[37] Rheinberger (1997, chp. 9) makes a similar distinction between two kinds of 'junctures' that can happen to experimental systems. The first kind is the hybridization of two experimental systems, which he describes as the "bringing together [of] things whose articulation, amalgamation, or even blending was not assumed to lie in the nature of the things so brought together" (136). The second kind is the bifurcation of an experimental system, which "occurs when [that experimental system] has reached a certain complexity that allows researchers to pursue slightly diverging epistemic tracks but which are sufficiently different to enable them to arrive at significantly different results" (136). Hybridization corresponds to a crisis in which disciplines are consolidated, and bifurcation to a crisis in which disciplines diversify. He too stresses that hybridization and bifurcation are not as extreme as 'paradigm shifts,' and that they occur much more often.

[38] This use of the terms 'epistemological' and 'ontological' is not technical. There is an important sense in which all crises are both, or 'epistemontological,' as Barad (2007) says. What I mean to say is that the ontological dimensions of the crisis are much more salient in the birth of a discipline. The crises that seem to be just 'epistemological' are the crises in which we can convince ourselves that this was the way things have been all along and that we just didn't *know* it.

Disciplinary commitments in interdisciplinary research

So what is it like to participate in a crisis discipline, in which "one must act before knowing all the facts…, before [being] completely comfortable with the theoretical and empirical bases of the analysis?" (Soulé 1985: 727) First and foremost, our disciplinary loyalties become an issue. In ontological crisis, it is much harder to distinguish between intellectual responsibilities (grounded in some disciplinary commitment) and instinctive responses to the situation at hand. In contrast, it is clear that scientists dealing with an epistemological crisis are being intellectually responsible (disciplined) because it was their disciplinary commitment that allowed them to see the crisis in the first place. In such circumstances, we are usually confident that the best and the brightest are at the frontlines trying to figure out what's going on, even and especially as they make interdisciplinary connections. When the crisis is ontological, we still recognize the necessity of an interdisciplinary imagination, but it is harder to tell whether this interdisciplinarity is actually grounded in a new discipline or whether it's just the responsible collective response of established disciplines to the crisis at hand (Cat, Cartwright, and Chang 1993). Either way, our disciplinary commitments are an issue.[39]

This is why discipline is an important question for conservation biology and other interdisciplinary research centers that are task-oriented.[40] Sharon Traweek (2000) interprets the proliferation of interdisciplinarity and mission-oriented intellectual projects as part of broader shifts in the geography of intellectual authority spurred by postwar,

[39] This is why so much of the talk about inter/disciplinarity is talk about these crisis disciplines. Discipline is not quite as salient an issue for, say, historians of quantum physics. The disciplinary loyalties of quantum physicists were never doubted. An exception is the work of Bechtel 1986, 1993, 2006, who looks at the disciplinary dynamics of scientific 'revoultions.'

[40] The list of interdisciplinary research centers is long. They exist in the natural sciences: astrobiology, bioinformatics, cancer biology, epidemiology, invasion biology, nanotechnology, risk analysis, stem cell biology; and also in the social/human sciences: area studies (e.g., Latin American studies, East Asian studies, etc.), black studies, feminist studies, international relations, science studies, urban studies. It is clear that some of these fields are more entrenched than others. But discipline is clearly an issue for all them.

postmodernist sensibilities. And yet, despite the fact that there are "new kinds of knowledge [being] made by new kinds of researchers at new kinds of universities" (38), Traweek notes that "the departmentally based disciplines still appear to control the definition of intellectual authority" (45). One cannot make knowledge without a clear disciplinary stance, and this can become problematic in interdisciplinary research. Even after all the stakeholding disciplines have come to the table, the conversation can still devolve into "disciplinary turf war" (Norton 2008: 383). These turf wars attest to the significant overlap of disciplinary domains that happens in contemporary research.[41] If research is to avoid a turf war, it must resist relying too heavily on the protocol of any 'home' discipline and adopt a pluralistic stance that can hold together various epistemic practices that might otherwise be at odds. The hope is that in crisis (e.g., climate change, de/colonization), we might be able to set aside our individual differences and act collectively for the sake of everyone involved.

Pluralism has been an important trope for the intellectual negotiation of incommensurability.[42] Since knowledges and values are often in conflict in ecological crisis, it is no surprise that many conservationists have promoted a pluralistic approach to their subject matter. Early programmatic statements of conservation biology (e.g., Soulé and Wilcox 1980, Soulé 1985, Primack 1993) stress the breadth of conservation's domain – "as broad as biology itself" with elements that are "not all biological." For

[41] For example, climate change is a phenomenon studied by many disciplines, from fluid dynamics to economics. Climate change negotiations, like debates about biodiversity, are host not only to conflicting data, but also to conflicting values and priorities. In turf wars, the normativity of disciplinary concerns becomes very prominent.

[42] Not only has pluralism been developed as an alternative to fundamentalist visions of the unity of science in general (allowing us to interpret disciplines as 'autonomous'), but it has also been recognized as an important characteristic of sciences and practices that are already unified. Thus, one can talk about the pluralistic foundations of quantum mechanics, mathematics, or genetics (see Kellert et al. 2006). The normative account of scientific practices and disciplines (see Rouse 1996, 2002) offered in this essay, which does not assume shared beliefs or standards, can be said to espouse a very broad kind of pluralism – of knowledge, of values, etc.

David Western and the Maasai, what the extent of the ecological crisis in and around Amboseli National Park revealed was the sheer number of human and nonhuman stakeholders in the conservation of the place (Thompson 2002). Western's strategy was to bring these stakeholders into conversation and to connect their interests as much as possible without pushing an agenda that would offend any of the parties involved. If you were concerned with Amboseli – if you were a Maasai, a government official, a non-governmental official, an elephant, a bush, a scientist, a tourist – Western was concerned with you. By bringing together such a plurality of values and knowledges, Western faced a problem that was, without a doubt, complex.

But its complexity – the sheer number of connections among different stakeholders in the process – was not something that Western's conservation tried to 'deal with' or 'move beyond'. As Charis Thompson argues (2002:184-187), Western's strategy stands in contrast to the two most common intellectual approaches to complexity: reductionism and holism. Reductionism recognizes the sheer number of connections and attempts to trace those connections back to an original domain that is understood to be the most basic or powerful. This strategy is one of "modernist frugality," approaching complexity as a "difficult" problem that must be dealt with. In contrast, holism recognizes the sheer number of connections and attempts to follow those connections up to an all-encompassing domain that includes every detail and cannot be analytically understood. This strategy is one of "antimodern romanticism or nostalgia," approaching complexity as a "complicatedness" that we must move beyond. As most would agree, the complexity of Amboseli's case could not have been tackled with a reductionist strategy. Indeed, Thompson says, "the political compromises and alliances did not get reduced to the truths of science or vice versa" (186). But as

Thompson astutely points out, the complexity of Amboseli's case could not have been resolved with a holist strategy either:

> The alliance between the science and the politics … was successful insofar as the science was not compromised by the politics (if seen as sui generis, with its own standards of truth and efficacy) yet was conceived over land, time scales, and forms of economic activity to which those with political clout at the time could be convinced to aspire as integral to their vision of the nation. … [In other words,] the political and scientific aspects of the case [did not] coalesce into a holistic frame of reference; indeed, *the separation of the politics and the science was what kept the linkages strong*. (185-6, emphasis added)

Instead of reducing the linkages to one basic domain, or pondering the singular domain that emerges from those linkages, Western chilled out and let the separate domains be separate. Nevertheless, he exposed the linkages for what they were, and he carved out a new niche in which people could explore the meaning of those linkages.

> The model instituted by David Western and the Maasai triumphed when they managed to connect, but not completely convert or align, more stakeholders to their [platform of conservation biology]. Critical to this success was the creation of enough space to engage without running roughshod over other moral universes. (186)

Western's pluralist strategy for conservation was to create a space of reasons in which to negotiate the ecological situation of Amboseli, a space that was distinct from the epistemic practices from which the various stakeholders were coming. As I read it, Western's pluralism is his means of avoiding a 'disciplinary turf war' by establishing a discrete disciplinary identity for conservation. In disciplining his practice, he is not trying to institutionalize it, but rather to stabilize it – so it can move forward via reliable rules of inference – and to make it autonomous from other disciplines – so they won't object to conservation as an unwarranted reorientation of their own domains, or a covering over of their 'moral universes.' Western was not in the business of telling politicians what they

should be doing as politicians, or scientists what they should be doing as scientists; he was telling both groups what they should be doing as conservationists.

If David Western was carving out a new space of reasons for conservation biology and showing it to be a discipline in its own right, what can we make of his resistance to publishing his results for an international audience (see chapter 1)? Did his disciplinary community only include those people who wanted to "see for themselves"? Countering the demand that he publish in peer-review journals, Western argued

> that conservation science had priorities that were not possessed by the academic science for which peer review and the ideal of disinterestedness had been developed. He argued that all science serves constituencies and that peer review is a means of serving the interests of academic science, where uniformity of opinions throughout a disciplinary community is a principle aim of the process. The constituents of relevance in Amboseli, he maintained, were necessarily local, as well as national, and only secondarily international. … If peer review worked by removing the links between the locally specific production and application of scientific knowledge and the knowledge itself, this was a separation not appropriate for conservation science. (Thompson 2002: 176)

Why was Western not interested in the backing of a disciplinary community? Is his science disciplined, or is it a tourist event? Does he practice conservation biology or just a 'science of Amboseli'? How far does his science extend? Can the conservation of Amboseli inform the conservation of other places?

Where does the domain of end? When does the crisis end?

If the first problem for crisis disciplines is the question of what their disciplinary commitments are, the second problem is the question of where their domains end and how general their claims are. It would be a mistake to assume that all of the stakeholders in the conservation of Amboseli were living in and around Amboseli around that time,

or that it was strictly a 'local' science. If my essay is working, it should be clear that the conservation of Amboseli matters to me, perhaps to you as well. It should also be clear that the internality of any science is always coupled with an accountability to outside affairs. Western used his experience in Kenya to inform the larger practice of conservation on how to manage wildlife without parks and to preserve ecosystems as they fit into the broader landscape (Western 1989, 1994). Talk about 'local science' is supposed to point out the specificity that biological conservation projects take on – the limits of inductive generalization and the focus of concern. But for conservation biology the discipline, the project begins and ends in the world at large, the world we call home.

We concern ourselves with 'places' because we care about our 'world.' For ecological practices that are indigenous to a place, the 'world' often is a precise biogeographic location. Such a tight fit between home and place generates astonishingly rich ecological knowledge and promotes responsible ecological practices. Conservationists have recognized the importance of indigenous ecological practices, but conservation biology is an intellectual discipline of the West.[43] As such, the world that most conservation biologists call home is planet Earth. That the predominant environmental concern nowadays is global climate change testifies to the fact that most intellectuals understand their responsibility to their home as a responsibility to their planet. This is why urban-dwelling Americans can think about arctic-dwelling polar bears when they recycle their waste. But even the disciplined conservation of a specific biogeographic location is undertaken for the sake of the Earth.

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[43] For a good example of indigenous ecological knowledge, see Ellen 2007 on the agrarian expertise of 'traditional' societies in southeast Asia. For a good example of the responsibility of indigenous ecological practices, see Deborah Bird Rose's work (2004) on the Aboriginal understanding of 'country' and 'land.' Also see Sanga and Ortalli 2003. See chapter 3 for an extended treatment of indigeneity and the care for nature.

But the planet is not a practical object of inquiry for conservation biologists. Properly speaking, the Earth – as in, 'Save the Earth!' – is the domain disclosed by conservation biology the discipline. This Earth is not synonymous with the terrestrial/celestial object discovered and rediscovered by astronomers and cosmologists since prehistory. For conservationists, Earth isn't simply a spherical object or even the collection of objects that make up the biosphere. As the domain of conservation biology, Earth is the space in which biological and ecological things show themselves to be damaged or threatened. Said in more explicitly normative terms, Earth is the space in which those threatened things can matter to us. Knowing that we live on Earth is what compels us to attend responsibly to the conservation of threatened populations and damaged habitats.

As was noted before, the domain of conservation biology is broad, extending from the labs of geneticists to the fields of anthropologists. With an understanding of that domain as Earth, the space and home we want to save, we can more properly understand what it means for the domain of conservation biology to be broad. What the disclosure of Earth has revealed are patterns of ecological degradation that are extraordinarily widespread. Nature everywhere and at all scales, including the genomes of germs and aether beyond our sight, has been affected *potentially harmfully* by human activity. I take this to be the single most important fact that the discipline of conservation biology has contributed to cosmopolitan intellectual culture.[44] The extent

[44] I am not claiming that conservation biology is the only discipline that has discovered human impacts on the natural world. Conservation biology is generally focused on the same things as ecology – species, communities, habitats, and ecosystems. And it is quite clear, for example, that the understanding of climate change has never fallen solely under the jurisdiction of conservation biologists. What conservation biology has done is disclosed all of these phenomena to be part of a unified process: the anthropogenic alteration of Earth.

of the ecological crisis and the breadth of conservation's domain are two sides of the same coin.

The ubiquity of ecological degradation does two things. First, it makes nearly every place on the planet accessible to conservationists. One can imagine a conservationist plea for saving anything anywhere since the future of everything everywhere seems to be at stake. Second, since all of Earth cannot be targeted at once, it forces conservationists to choose which places to save. This decision is the part of conservation practice where objectivity and methodological concerns come roaring in, and the object whose objectivity matters here is biodiversity. The objective evaluation of the biodiversity of different places is conservation biology's principal technique for prioritizing which places should be conserved. In the early history of conservation biology, the conceptual articulation of biodiversity took a backseat to the theoretical understanding of nature preserves, which were presumed to be hotspots of diversity. But as biodiversity began to roam outside the boundaries of the preserve – indeed, outside the boundaries of the discipline – conservationists recognized the need to struggle with biodiversity itself as that thing, whatever it is, which conservation biology is trying to conserve. There is a general consensus that biodiversity is a plurality of phenomena, existing at many scales and resisting straightforward techniques of measurement. Sarkar 2002 and Norton 2008 describe a sophisticated algorithmic approach called prioritization-management that uses an open-ended number of surrogate phenomena to estimate the biodiversity 'value' of different places and to prioritize those places such that the amount of biodiversity being conserved will be maximized.[45] This place-prioritization is then subjected to the nuances of the chosen places to determine the

[45] The pluralism of biodiversity concepts is captured by the open-endedness of what counts as a surrogate. "By focusing on *new* surrogates," says Sarkar, "it naturally captures our intuition of bio*diversity*" (2002: 148).

political 'feasibility' of a conservation project, and to adapt the management protocol to the target landscape. Although this model of biodiversity conservation has yet to be applied thoroughly or widely, the pluralistic sensibilities behind it are clear. There is an extended network of interests and objects that needs to be coordinated if conservation biologists can even choose a place to begin working.

After these algorithmic and political acrobatics, when conservationists finally arrive at a place, the rules of the game change. The same is true for clinical medicine once there is a patient in the room. What matters to any good doctor is the patient, and what matters to any good conservationist is the place. Both sciences are sciences of case studies. But in both practices, before the treatment comes the choice of whom to treat. Before negotiating the particularity of place, conservationists must cross the threshold into Earth and navigate the ethics and metaphysics of biodiversity. Even after conservationists settle, even though they never have to look beyond the horizon to address their concerns, they know that their domain extends indefinitely, into new worlds, into alien territory, to any place where humans coexist with nature. Conservation takes to an extreme the vigilance that disciplinary cultures and 'settler cultures' (see Rose 2004) maintain toward the outside and the future. This inclusiveness is exemplified by the sprawling space of reasons that David Western carved out for the negotiation of Amboseli:

> [Other aspects of the situation]— legal issues, land-use disputes, economic and moral concerns, and so on – did not disappear but were all easily recoverable within the scientific dispute itself. The scientific dispute was a way of packaging and then managing the much more unwieldy set of conflicting views. (Thompson 2002:179)

Likewise, the space carved out for the negotiation of Earth is so expansive that to be in nature at all almost necessitates the conversion of one's Land into Earth. At some point,

we give up conceptualizing where the domain ends, and start wondering when the crisis will end.

CHAPTER THREE
Riding on the whale's back, or, what it's like to be in the grip of nature

> Every motion, wind thru trees, blue war-paint on the forehead, taste of pig flesh, is as real, is universal. There is no science that is not an ethnoscience, and oecology is the way we plant our fields, the way we contact those who seem to have put us here
>
> – Richard Grossinger (1970: 15)

Intellectual cultures and settler cultures

The responsibility of scientific disciplines to matters outside of their borders – a responsibility exemplified by interdisciplinary research and the migration of concepts from one domain to another (e.g., the ubiquity of 'mathematical tools' and 'philosophical foundations' and 'social consequences') – demonstrates the impossibility of isolating a scientific practice from its embeddedness within other practices. The discipline of a science is only sustained in relation to other traditions, not only by knowledge that has been mobilized by other scientific disciplines but also by patterns of government funding and trends in social consumption. Disciplinary borders are well trafficked, as intellectuals bring in the raw materials to make knowledge and export the authoritative product. This traffic is the basic stuff of intellectual practice, and academics know it. As such, the relationship between disciplines and their outside is one of accountability. Intellectuals better get it right, because at stake in the correctness and efficacy of their knowledge are the futures of other people and larger histories, and not just the fate of their discipline. Not all scholars and scientists engage the social and political dimensions of their research, but many of the best ones do. The best ones also recognize that, because of this accountability to the outside, the authoring (and not just the authorizing) of knowledge is a social affair.

The entanglement of an intellectual discipline with its constitutive outside – the rest of the academy and the world at large – encourages the interpretation of scientific practices as cultural practices.[46] Despite its heterogeneity, a discipline maintains a cohesive normativity which identifies it to other disciplines and cultures as an epistemic authority that might need to be dealt with at some point. For instance, it is conceivable that cultural anthropologists could discover details about indigenous ecological practices that are of interest to agronomists studying the benefits of crop rotation and techniques of intercropping; meteorologists certainly forecast details about weather patterns that farmers are sure to take note of. On this account, disciplines, insofar as their practice is intelligible by others, are cultural movements. They align a wide variety interests into a recognizable force and onto a vehicle for changing the world. Like other kinds of cultural movements, scientific disciplines aren't simply in charge of public opinion, but simultaneously answerable to it. New epistemic authority can shake things up, and it can also be enlisted as the intellectual wing of a pre-existing political or cultural trend. This reciprocal accountability is what it means for scientific practice to be entangled with other practices.

Quite appropriately, then, the question of discipline is best approached from an interdisciplinary standpoint. By critically engaging disciplines as cultural movements, I hope to reveal how the philosophical issues raised in previous chapters have always also been anthropological and political issues. At stake in our discussion of disciplinary normativity is not simply the adequacy of an account of epistemic authority, but the

[46] See Rouse (1996, chp. 9 and 2002, p. 180-183) on what cultural studies of science are. Unlike positivist philosophy of science and postpositivist sociology of science, cultural studies of science do not seek to legitimize or delegitimize the authority of scientific knowledge. Far from any attempt to "ascertain from the 'outside' what science 'really is'" (2002: 180), cultural studies of science are critical engagements with scientific practices and participate in the making of scientific meaning.

livelihoods, efficacies, histories and futures of particular epistemic authorities, including ones that have been marginalized as nonmodern and nonintellectual. Likewise, at stake in interdisciplinarity is not simply the basic accountability that binds disciplines together, but the politics of interculturality, which, following social anthropologist Marilyn Strathern, I take to be the politics of "our already inhabiting one another's cultures" (2004: 1).

This essay will develop an account of disciplinary culture by watching the flows of knowledge at three scales. First, we will reengage interdisciplinarity, this time as the obligation that different disciplinary cultures have to each other in the pursuit of knowledge. Second, we will take a step and observe the larger flow of knowledge into and out of intellectual culture, a flow that expresses the obligation that intellectuals have to their society and to other cultures in the pursuit of responsible knowledge. Finally, we will tend to knowledge as it flows into and out of the known, into and out of the present. This move will not abstract the issue of knowledge production back to epistemological debate. Rather, we will be striving to understand where knowledges come from how they travel through our world, and where they go to when they leave.

It will become clear that the production of knowledge is an intensely collaborative affair, and that the main issue is (the looseness of) interdisciplinary and intercultural accountability. I will show how the orientation that intellectual cultures maintain toward the outside and the unknown is a temperament that has coevolved alongside the vigilance with which 'settler cultures' approached the frontier and the wilderness. Deborah Bird Rose, a theorist of social and ecological ethics focused on Australian Aboriginal relations to land, describes the confidence that settlers had that

uncharted lands and unknown peoples were things that someday could and should belong in the settlers' own world:

> Frontier narratives … define what exists on the 'other' side of the frontier as awaiting transfiguration. The violence proceeds from here: the 'other' side is appropriated – from being people who live among and for themselves, they are forced into being people whose lives and deaths are for others. (Rose 2004: 61)

Disciplines, like settler societies, are always ready to extend their domain into new territory. Scientific research and intellectual work in general is oriented toward things which are not yet understood, and this orientation is founded on the basic intellectual commitment to the possibility of there being (reliable) rules (of inference) at all. In other words, the unknown is something that can be known, even though nobody knows it yet. Rose tells the parallel story about settlers' orientation toward that which is not settled: "The coloniser celebrates his pre-presence in glorifying 'wilderness' as a place where he can encounter his own absence" (65).

To what are we held accountable in unmapped territory and in the face of the unknown? This is a favorite topic for philosophical investigations. The issue is often framed as one of objectivity, or, as the question of how we can perceive and understand objects in the world for what they are. Far from being the kind of objectivity pursued by a disinterested scientific method, the possibility of holding ourselves accountable to something we don't know (e.g., an epistemic thing) is, as the previous phrase suggests, premised on our reflexive capacity to 'hold ourselves' open. At the root of objectivity is the accountability we have to ourselves.[47] The world which we recognize out 'there' only

[47] This kind of reflexivity is a major theme in the traditions of phenomenology and existentialism. John Haugeland's (1998, chp. 13) conception of 'existential commitment' illustrates the kind of self-awareness and self-possession that philosophers understand to be the condition of possibility for being aware of the rest of the world.

gets its characteristic 'thereness' because it matters to us (Barad 2007). As such, we cannot account for everything in the world without accounting for who we are such that the world matters to us. However, this (interdisciplinary) essay is not being held entirely accountable to the history of philosophical inquiry into these matters, so we will interpret these issues in a discourse that is more anthropological, in terms of how humans care for nature. How do we manage our responsibilities – to each other, to the unknown – when it seems like the future of the whole world is at stake? With whom are we collaborating when we go about preserving the natural world?

The flow of knowledge within intellectual culture

For a short time right before modernist circuits began to spark, disciplinary border crossings were philosophically sanctioned as movements toward the unity of science. Without going into the logical details of reductionism, we can recognize that at stake in this kind of interdisciplinarity was the unification of all knowledge. However, scientists conducting interdisciplinary research are not worried about the epistemological problematic of reduction. What matters is that someone else's expertise is bearing on their own, and the issue for them is how to collaborate. Contrary to the verticality suggested by the powerful image of a hierarchical ordering of scientific disciplines with fundamental physics at the root, flows of knowledge within the academy are more typically (and metaphorically) horizontal.

At stake in these disciplinary border crossings is not the unification of all knowledge but the integrity of the disciplines involved. For example, many marine biologists studying the ecology of zooplankton use acoustic techniques – the same ones developed by marine engineers for navigation, communication, and the detection of

enemy submarines (i.e., sonar) – to probe huge swaths of ocean for the presence and density and even identity of zooplankton.[48] In order to carry out these 'ecological' studies of zooplankton, biologists rely on the sophisticated techniques of acousticians and engineers who are sensitive of the stakes of ecological practice and make their own knowledge available and applicable to the context of ecological research.[49] In an intuitive sense, it is *right* for ecologists to utilize acoustic techniques and for acousticians to address ecological issues. Said another way, it would have been too bad if ecologists had eschewed acoustic techniques as the stuff of physics with no significance for the study of life. Although the overlap of domain had always been there, only disciplined ecologists and acousticians can recognize it for what it is and see the potential benefit of disciplinary exchange. Marine bioacoustics, as this field of research is called, is not a discipline separate from marine ecology, nor has bioacoustics reoriented what is at stake in its discipline. Unlike the interdisciplinarity surrounding conservation biology – where a new domain is being articulated – the disciplinary exchanges in bioacoustics are executed as the relatively straightforward and uncontroversial borrowing of techniques. These more mundane disciplinary border crossings offer a better illustration of the interdisciplinarity that is latent in every discipline. Even when the boundaries of a domain are not at issue, traffic flows across them.

Generalizing from this example, then, I understand interdisciplinarity to be the norm of intellectual practice that keeps disciplines accountable, aware of the wider

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[48] Plankton species are the tiny floating organisms that can't really move around except with the ocean currents. Zooplankton are big enough to scatter the emitted signal in ways that make the backscatter (i.e., the echo) intelligible as something caused by a given density of zooplankton. (Phytoplankton are too small to scatter the signal in ways that can be detected.) The differences in size and shape and chemical make-up of zooplankton (e.g., jellyfish are squishy and round, krill are rigid and long) can also show up as differences in the characteristic kinds of backscatter they produce.

[49] There is a very small group of acousticians whose career work is advancing the mathematical models used by ecologists (Dr. Tim Stanton, Woods Hole Oceanographic Institution, personal communication).

implications and ramifications of their research. Interdisciplinarity is about understanding the connections that are there even when those connections don't call into question the limits of anyone's epistemic authority. In fact, argues Marilyn Strathern (2004), interdisciplinary connections are often an index of the success of disciplines – a measure of how transparent their authority is and how intelligible and useful their knowledge is. How would anyone appreciate a discipline if its knowledge was unable to travel outside of its borders? As literary critic Gillian Beer argues, "Ideas cannot survive long lodged within a single domain. They need the traffic of the apparently *im*appropriate audience" (quoted in Strathern 2004: 36).[50] One might speculate that this is why esoteric knowledge – about supersymmetric string theory, for example, or about shamanic healing – makes some people skeptical: what on Earth could those practices be held accountable to other than the concerns of the practitioners themselves?

If interdisciplinarity is an index of a discipline's accountability, what keeps interdisciplinary practices accountable?[51] Is there some way to measure exactly how interdisciplinary a particular research program is, just how far-reaching its knowledge will be? Moreover, is there a point at which a discipline – whose knowledge has been peer-reviewed, published, and projected into the intellectual public sphere – a point at which that disciplinary knowledge oversteps its bounds, so to speak? I think the answer to this last question is unequivocally yes, and the two examples I cite are both about the inappropriate extension of evolutionary theory into social theory: social Darwinism

[50] Beer's work (1996) tries to make sense of the overlap of domain and the two-way conceptual traffic between 19th century evolutionary theory and other schools of thought. Was Darwin's use of the language of kinship to describe evolution (in terms of origins and species relations) a matter of metaphor, or is there a real connection between evolution and other phenomena of kinship?

[51] Strathern makes the conjecture that since interdisciplinarity is itself understood as an index of accountability, not many people are worried about making interdisciplinary practice accountable; it is "an evaluator rather than the subject of evaluation" (2004: 79).

(whose imperialist undertones will not be explicated here, but see Beer 1996) and sociobiology. By extending the chain of biological causation out to social phenomena, sociobiology actually attempts to erase those phenomena, insofar as it tries to reduce large chunks of the disciplines of anthropology and sociology. This attempted reduction belies the "isomorphism" that exists between evolutionary principles and economic principles (Sahlins 1976, chp. 1). That is, by subsuming human social behavior under a biological explanation, sociobiology fails to recognize how its own concepts are indebted to the work of other disciplines.

The need to keep inter/disciplinary practice accountable can be appreciated with an understanding of how disciplines, especially crisis disciplines like conservation biology and medicine, can disclose sprawling domains for their normative authority to roam. The normative authority of crisis disciplines does not manifest in a singular expertise that takes precedence over others, but rather in a flexible overseer who gets people to collaborate in such a way that they can value each other's distinctive expertise. Drawing on the work of sociologist Joanna Latimer (2004), Strathern describes the ability of hospital consultants and medical clinicians to align a diverse array of interests and narratives.

> [The consultant or clinician] can hold people to account even when they have no formal authority over the person concerned. [They] offer joined-up government in a world of distributed clinical process, by giving all the members of the multidisciplinary team their own place. What is effected is a 'continuous calling to account and a continuous division of responsibility'. Latimer emphasis that authority is asserted not through exclusion or silencing of others, but through the clinicians or consultants commandeering discourses and materials that belong to multiple domains. (Strathern 2004: 82)

The parallel to David Western's conservation platform is immediately apparent. Conservation and medicine exist in a space of reasons that is separate from the disciplines that participate in them (e.g., ecology, pathology). The rules are different when the life of humans or the demise of nature is at stake. Already, we have slipped out of the academy and into 'Society' and 'Nature', so let us acknowledge it with a new section and a step back.

The flow of knowledge into and out of intellectual culture

The accountability of a disciplinary culture to the concerns of others is, more fundamentally, a responsibility to 'Society' and the world at large, including cultures which may seem from the standpoint of some intellectual traditions to be nonintellectual, conformist, and 'backwards'. This orientation to the outside is more basic – or at least more illustrative of the intellectual commitment – than the orientation toward other disciplines. Many intellectuals, especially today, explicitly cite this relation as the one that they care about most. For all their training and expertise, intellectuals are often the first to make the humble acknowledgment of interculturality: 'we're in this together'. This is why the stakes of research – the normative accountability that binds a practice together – are so often figured in terms of the social and political consequences that their work has for other people. The difference between good science and bad science turns on the sensitivity a disciplinary practice has to its embeddedness in other cultural practices.

Strathern explains that in the reflexive sociology of academia, interdisciplinarity is a trope that emerges simultaneously with an attention to the involvement of 'Society' in

the production of knowledge.[52] Indeed, if interdisciplinarity is an index of intellectual accountability, then intellectual accountability is an index of public interests and humanity at large. For the responsible intellectual, Strathern says, "reaching beyond disciplines merges with reaching beyond academia" (2004: 72). We should be careful not to herald intellectual responsibility to the public as a new feature of knowledge production. The history of science is replete with stories about how scientific knowledge mimicked the cultural values of its time (Haraway 1989). What may be different about intellectual practice today is its appreciation of the variety of cultural values and the range of social interests that are always in play. Indeed, Strathern (2004: 1-11) cites practices of audit (e.g., institutional review boards) and fields like bioethics as attempts to anticipate the broader implications and consequences of research before it is done.[53] 'Disinterestedness' is no longer a viable research ethic, and the claim to objectivity is no longer a magical scientific license. That knowledge serves interests has ceased to amaze us. The issues now – addressed best by anthropological methods (including practices of audit) – are whose interests it is serving and what that service is. At stake in the answering of these questions are, of course, the corresponding normative issues. Whose interests should it be serving? And what should that service be?

One 'interest group' that has posed an interesting challenge for intellectuals is the group comprised of nonintellectual and nonmodern cultures. How do the most 'forward-looking' people relate to cultures that sometimes seem 'close-minded' or even

[52] I understand this trend in sociology and related disciplines as a trend that was responding to (and partly responsible for) the proliferation of interdisciplinary and mission-oriented projects that Traweek (2000) discusses in her essay on the geography of knowledge production.

[53] Practices like these that monitor accountability – some of which, like practices of funding, have an authoritative say in whether or not knowledge is pursued – are engaged with the same epistemic thing as the research is. Scientists ask, how can we figure out what this thing is; auditors ask, how can we figure out what this thing will become.

'backwards'? This is the classic jurisdiction of anthropology and ethno-disciplines (e.g., ethnobotany), but it is also a salient feature of policy making, clinical medicine, resource management, conservation science and other "applied" disciplines that get their hands dirty with social work, as well as any research that struggles with globalization and the intensifying of intercultural affairs. In scientific research, the responsibility to nonintellectuals is often expressed through collaborations with 'lay experts', on-the-ground authorities whose skills and know-how are valued for their situatedness. Like any trained professional, the lay expert has specialized knowledge. In some contexts, lay expertise is only figured as something that needs to be 'respected' – an interest to be served, not engaged. But innovative intellectuals are often able to collaborate with lay experts to do science that is not only 'socially robust', but also often dazzlingly intelligent.

For example, consider two examples of 'traditional knowledge' that seem to fall right on the cutting edge of Western science. In his essay about the long history of Aboriginal landscape burning, Dave Bowman (1998, reported by Rose 2004: 169) discusses the much shorter history of intellectual debate surrounding it – which includes ethnographic, archaeological, paleoecological, geological, evolutionary, and conservationist perspectives. Correcting the radical ecocentric view of burning as Aboriginal 'pyromania' that had wildly destructive effects on the landscape, Bowman explains that burning practices were skillful engagements with nature that created a cycling pattern of habitat mosaics which sustained biodiversity across much of the continent and allowed important food plants to flourish for humans and other herbivores. Pushing the intercultural bill, Bowman argues that "what is required is an advance from the poetic concept of fire-stick farming to a coherent scientific analysis of

Aboriginal burning that can be used to buttress land-management prescriptions" (1998: 404). In a similar example, Cori Hayden (2003), examining the variety of 'interests' and 'publics' that emerge in the American bioprospecting of tropical plants for pharmaceuticals, discusses the authenticity and authority that ethnobotanical studies have actively sought to confer upon traditional healing practices. In one glittery study, an NIH-funded ethnobotanist screened the plants used by one healer in Belize for anti-HIV activity and found four times as many 'hits' as a screen of a random sample (Hayden 2003: 34).

The 'interests' of nonintellectuals give them a dual role in scientific research. Not only are they figured as an 'interest group' to be served, but their 'interests' in the matters at hand seem to be wrapped up with an authority that makes them co-collaborators in production of knowledge. In these kinds of intercultural alliances, Western ethnoscience vindicates traditional knowledge and, in so doing, makes the promise to preserve or support the cultural practices whence that knowledge came. In ethnobotanical bioprospecting, as Hayden demonstrates, the vindication and defense of indigenous cultures are explicitly and positively linked to the discovery of drugs and the extraction of resources. On a single scientific platform, 'traditional knowledge' is proven; local plants are turned into sophisticated medicine; and the profits are distributed fairly among scientists and lay experts.[54] On top of that, since effective pharmaceuticals are hard to come by even in the tropics (just like gold is hard to find even in a gold rush), bioprospecting also has a stake in the preservation of biodiversity, figured as the rawest of the raw materials that are channeled into biotechnology (Helmreich 2009). In that

[54] Hayden 2003 shows that this scientific vision is hardly realized in bioprospecting practices, which have had trouble coordinating human, natural, and economic resources. The promises of bioprospecting, she argues, have remained promises, generating different effects on indigenous and pharmaceutical practices than anticipated.

sense, technological innovation and biological conservation are powered by the same conceptual engine. It is no surprise that 'diversity' is what ultimately matters in the hybrid agenda of bioprospecting-cum-conservation(-cum-development). Biological diversity and cultural diversity – linked not simply by metaphor, but by a history of responsible human ecological practices (as Bowman (1998) shows) – need to be preserved, not only for their own sake but for the potential knowledge and benefit that they offer to global problems.

In these intercultural spaces of engagement,[55] science is diluted with a strong dose of liberal sentiments of inclusion and responsibility. In the case of cultural misunderstandings or conflicts of interest, these sentiments ground the debate in a keen attentiveness to the fact that 'we're in this together.' In this orientation toward the big picture, intellectual accountability to others (other disciplines, other cultures) shows itself to be rooted in a responsibility to the world we share. It is the grip of the world that compels us to make responsible knowledge. For conservationists, that world is Earth; at stake in the conservation of nature is the future of the life-sustaining planet that we all depend on.[56] Other biological disciplines as well as geology and planetary science have contributed a rich knowledge of what Earth is and how it works, but it is in conservation science that a care for the Earth is explicitly invoked.[57] And yet, we wonder, is the conservationist care for the Earth the same as, say, the Australian Aboriginal care for the

[55] Remember that my use of the term 'intercultural' means something very different than the relation between the West and the rest. Intercultural engagement is the encounter between intellectual/scientific practices and the other practices they are connected with. Many intellectuals are either obligated to, or make it a point to, affiliate with anyone who has a stake in what their work is about. This is what makes their practice intercultural.

[56] In a similar way, bioprospecting is pursued for the sake of the 'public domain,' a safe zone where no one is excluded, by definition (Hayden 2003: 44-47).

[57] In order to express a similar care for nature, researchers in other disciplines re usually compelled to do so in the language of conservation. As 'care' has become a more and more worthwhile epistemic value (a trend indicated by the rise of audit and bioethics), it makes sense that many different kinds of biologists have begun to demonstrate the connections their work has to conservation.

Land (Rose 2004)? If the globe disclosed by seafaring Europeans includes the continent of Australia, does the Earth likewise *include* the Land that is disclosed by Aboriginal relations to their ancestors and Dreamings? Our anthropological instincts say no to these questions, especially in light of all of the ways Aboriginals have been *excluded* from the modern management of land (Rose 2004). Anthropologist Anna Tsing (2005) argues that the 'global' is not a category under which the 'local' ranges as a mere part. All global projects originate from some set of local circumstances, and local circumstances often disrupt the smooth expansion of global projects. Moreover, global projects aren't the exclusive hobby of Western intellectuals. Aboriginal land ethics are about caring for nature the concept (the Dream), not just Australia the continent. This is what anthropologists – professional and otherwise – mean when they say, 'We can learn from them.'

Local and global are two sides of the same coin (like discipline and interdisciplinarity), and the tension between them is a normative issue. How global should conservation biology be? The conceptual understanding of biodiversity certainly targets Earth as the domain in question, but many conservation projects in non-Western nations have exposed the imperialist undertones of conservation practice (Argyrou 2005, Apffel-Marglin 2005). Many international efforts to save charismatic mammal species (e.g., tigers in India, elephants in Kenya) have used the distinctly American technique of nature preserves, which is often coupled with the displacement of local communities of humans and their livestock from the rural landscape (Guha 1989, Thompson 2002).[58] Even conservation projects that don't espouse an ideology of wilderness (e.g., efforts to

[58] Even the creation of American nature preserves in the 19th century, most notably Yostemite, was coupled with "a strategic amnesia about the [Americans Indians] who lived in these lands who had been evacuated onto reservations" (Apffel-Marglin 2005: 2).

control invasive species) as well as those that make a point of it to defend indigenous rights (e.g., bioprospecting) slip into a politics of Nature that strips 'local' cultures of their agency in the 'context' of the global scientific debate (Helmreich 2009, Hayden 2003).[59] In giving these examples, I don't mean to dismiss the merit of all global aspirations; for instance, I think that Aboriginal land ethics can and should inform many conservation practices around the world. Rather, I want to describe situations that highlight a tension between the specificity and universality of our responsibility to nature. Struggling with this tension allows us to refine our concerns into more thoughtful questions: is the care for nature a good starting point from which to build intercultural alliances? The care for nature should be a care for home, but how do we take care of our home when we share it with so many other people?

Since I don't know the best way to answer these questions, I am going to start more slowly and proceed less anxiously than I did in the previous paragraph. I will try to illustrate the significance of these questions by recounting a story that Deborah Bird Rose (2004, chp. 11) tells about the intercultural effort to protect a mountain in New South Wales from logging. The mountain – really an extinct volcano – is named Gulaga, and, despite a long colonial history of mining, logging, and farming, it is home to three types of rainforest ecosystem as well as various kinds of Eucalypt forest. Such heterogeneity of habitat makes Gulaga a biodiversity hotspot, and the state government

[59] Stefan Helmreich 2009, chp. 4, discusses efforts to control invasive species of marine algae in Hawai'i. Marine invasive ecology, and marine conservation in general, is often conducted as a global science, not only because the ocean is big and because much of it is international territory, but also because ocean currents and shipping practices can connect distant parts of the blue planet very tightly. For instance, to protect the California coast and its fisheries from biological invasions, international agencies must regulate the water ballasting practices of ships around the globe. Helmreich theorizes the kinds of 'contexts' that must be produced in order for scientists to interpret which species are invasive. Invasive species – also called 'alien' species or 'exotic' species – are usually considered those which were introduced to a place by human activity and are now harming the species that are 'native' to that place. With hardly any critical analysis, we can recognize that the politics of indigeneity are entangled with the politics of global nature.

acknowledged this by designating the eastern face of the mountain a botanic reserve. But when state authorities later began a timber removal project on the western face, Aboriginals and Whites rose in protest. Most white folk cited issues of water supply, erosion, and conservation as reasons to prohibit logging.[60] In contrast, most Aboriginals cited the fact that Gulaga is a Dreaming woman and that by logging the mountain (her body), the state is compromising her capacity to teach and nurture the people who love her. Nevertheless, Rose saw a general consensus among Aboriginals and Whites that the issue at hand concerned both the ecology and sanctity of Gulaga, and she reports that the efforts to protect the mountain were largely successful. What was the common ground that allowed for a dialogue between these startlingly different worldviews? Where were the connections being made?

The issue for intercultural collaborations premised on inclusion and a care for nature is that the 'world' everyone shares is not the same world.[61] Intercultural disputes about land management and social policy are ontological debates about what reality is, which reality should be pursued, and how and when to pursue it. This is how philosophical concerns become anthropological issues, and vice versa (Mol 2002, Rouse

[60] 'White' should be glossed here as 'intellectual.' That is not because all white people fighting on behalf of Gulaga were intellectual (indeed, see Rose 2004: 203-212 for an extended treatment of a nonintellectual settler's concern for Gulaga). But we should remember that being 'intellectual' is not about education but about culture. Citing issues of 'erosion' and 'conservation' indicates that one has been steeped in scientific discourse on the matter.

[61] That different people inhabit different worlds is a phenomenon that has been noticed from a number of disciplinary perspectives. Anthropologists often talk about the 'other worlds' of indigenous cultures (see Ingold 2004, de Castro 1998 on 'multinaturalism', and Blaser 2009 on 'politicaly ontology'). Many anthropologists turn this ontological critique back onto modern science and their own culture (see Raffles 2002 on the 'dreamlife' of ecology, and Helmreich 2009, chp. 6, on cyborg anthropology). Across campus, philosophers have also been struggling with the question of 'other worlds', especially in the terms of the phenomenological tradition. John Haugeland (1998, chp. 13), discussing how objectivity depends in part on an individual's responsibility to the integrity of the whole domain in which objects appear, cites Thomas Kuhn's sentiment that "the proponents of competing paradigms practice their trades in different worlds" (quoted in Haugeland 1998: 352). In Haugeland's simple interpretation, they are dealing with different objects.

2002).[62] These ontological debates cannot be settled honestly by a 'multiculturalist' politic, because different things count as nature in different practices, and people aren't caring for the same thing when they are caring for nature. However, the multiplicity of nature, like the multiplicity of the body, isn't a fact; nor is it a fiction. It is an issue. When is it appropriate to 'coordinate' the multiplicity of natures into one world, settling disputes along the way? When is it appropriate to keep the multiplicity of natures separate, 'distributed' so as to avoid dispute?

Ontological discrepancies over what nature is are brought into relief by intellectual practices like conservation that target worldly phenomena like patterns of global connection. Facing such profound incommensurability, many intellectual practices, for all their sensitivity to 'the outside', often make a move that reveals a striking lack of self-awareness: they cast the problem of interculturality in intellectual terms, the terms of their own culture. In intellectual culture, complex global problems are tackled with the disciplinary opening of a space of reasons in which the concerns of all stakeholders can be accounted for. But this notion of knowledge as a network of interests is not a magical (i.e., nonnormative) explanation of what knowledge is; it is a notion that intellectual cultures make explicit in claims about what knowledge *should* be. This norm is manifest as the "politics of accountability" that characterizes not only practices of audit and interdisciplinary research, but also intercultural and global efforts to conserve the natural world (Hayden 2003, Strathern 2004). But when 'traditional knowledges' are called to account, and when nonintellectual cultures are asked to express

[62] While many scholars have recognized the ways anthropology bears on scientific *and* metaphysical issues, it is missing the larger point to emphasize the former and not the latter – like social constructivist critiques of scientific facts. It is also missing the point to stress the latter and not the former – like many ethnographic accounts of 'other natures' which dismiss scientific practices as fundamentally flawed and irrelevant to their 'indigenous' metaphysical articulations of nature (I believe Ingold 2004 makes this mistake). Metaphysics is political (Blaser 2009) and experimental (Barad 2007).

their 'interests', they must do so in the terms of debate that have been chosen for 'intercultural' negotiation. Solicited for speech, the Other scrambles to find a position of authenticity from which to assert a cultural identity. Just as practices of audit demand that intellectual cultures be able to describe themselves in some kind of reflexive analysis, so too do intercultural practices demand that a 'culture' be able to account for itself as such. This injunction to own up to a tradition often thrusts indigenous cultures into an unfamiliar self-consciousness as one-culture-among-many. Nevertheless, many indigenous people choose to adopt a cultural identity (even if it seems to be already 'there', laid out by Western preconceptions) as a platform for their politics. To be sensitive to other cultures, Westerners need to be able to identify them.

Marilyn Strathern (2004: 91-95) tells a story, similar to the story of Gulaga, that details the politics of accountability in intercultural efforts to protect nature. In Papua New Guinea, a group of indigenous activists (hereafter the Group) raised a complaint against a mining company (hereafter the Company) about the ecological damage a river system had suffered as a result of the mining practices. The Company claimed that the rivers were too remote from the mine for the damage to have been caused by their activities, and, after consulting a government agency, found that there was no reason to investigate the Group's claim scientifically. The Group was outraged, especially because they knew that the mine was connected to the river system through a vast underground spirit tunnel. However, they did not cite this evidence in any of their claims because, as they explained later, they didn't want to introduce cultural details that would raise skepticism. Indeed, when a social scientist (here, the intellectual par excellence) decided to open the space of reasons up to details about the spirit tunnel, the conversation devolved into accusations aimed by the Company at the 'made-up' stories of 'culture'.

Besides, the Company argued, it was already well aware of its responsibility to 'society' and the 'environment', having set up various sustainable development projects even before the indigenous population raised the complaint. However, as Strathern sees it, the Group was not calling for the Company to uphold its accountability to other people or even the world at large:

> [The Group] did not appeal to 'society'. Rather, they wanted to impress onto the miners self-knowledge of a kind – to cause them to realise the effects of their actions – and did so through giving description of its own agency. While they explicitly omitted any direct reference to exotic details of culture that they thought would be a distraction, they were acting as a people in possession of a common self-knowledge, and they assumed that the goal of self-knowledge (that is, the realisation of one's effects in the world) would apply as much to members of the company as to themselves. (2004: 94)

The accountability that the Group was calling for was not a responsibility to 'the outside', but a kind of self-consciousness – the very same kind of self-consciousness that indigenous people are forced into in order to participate in the debate at all. The Group reminds us – only a 'reminder' because philosophers assure us that we already knew this – that we cannot account for the world properly without also accounting for ourselves.

With the politics of accountability twisted inside out like this, the prospects for collaboration don't look very good. With so many responsibilities at so many different scales, it's hard to keep track of everything that matters, especially when what we think matters most (e.g., the demise of Nature) does not offer a common ground for collaboration. In the course of taking care of nature around the globe, it becomes clear that people figure their relationship to the natural world in very different ways – as ecosystem, as Dreaming woman, as Earth, as Land. How can environmental scientists or conservationists possibly account for the underground spirit tunnel that is allegedly involved in the ecological degradation of Papua New Guinean land? They can ignore it

(as 'made-up') or respect it (as a 'cultural thing') or even believe in it (if they have 'anthropological' inclinations), but a spirit tunnel cannot emerge as an object of inquiry in the experimental systems of the disciplined practices that evaluate the ecological health of a biogeographical place. Moreover, how can indigenous Papuans possibly enter in this debate when almost everyone else at the table cannot even grasp what is at stake for Papuans in the 'conservation' of their 'environment'? Some might make a point of it to address the spirit tunnel – a beautiful image of the interconnectedness of nature – but indigenous Papuans are in its grip.

However, the Group raising the complaint also knew that they were in the grip of the intellectual debate about their land. Even in the refusal to share details about the spirit tunnel, "they were acting self-consciously with (scientific) knowledge of themselves as a people with a culture" and they called for a similar self-consciousness on the other side (Strathern 2004: 92). Moreover, the Group was willing to enter into the debate on these terms, without a commitment to the possibility of finding a common ground. For them, the dialogue could not have been held accountable to the science behind the Company's stance or the story of the spirit tunnel. They suggested rather that a successful collaboration could only fall out of the responsibility to self and to the tracks one makes in the world. Rose highlights a similar responsibility of ecological footprints, which she understands as the signatures of the ancestral care for nature, those enduring marks that sustain and guide living generations (2004: 177). Instead of emphasizing a responsibility toward the future in terms of sustain*ability* (the usual rhetoric of 'ecological footprints'), Rose advocates for an Aboriginal land ethic which orients responsibilities toward the sustain*ing* activities of our ancestors in whose footsteps we follow.

The reason I highlight the 'collapse' of our responsibility to the world into a responsibility to ourselves is not to *explain* how intercultural dialogue works or suggest a new and improved method for global conservation. I just want to present self-awareness (or self-possession) as yet another norm that constrains the thoughts and actions of human beings, especially in intercultural affairs. The normative authority of self-knowledge interests me because it folds the politics of accountability inside out, where the difference between one's self and one's world suddenly becomes an issue. However, unlike the taking up of other responsibilities – to other knowledges, to other cultures – the challenge of being accountable to oneself doesn't make things (i.e., knowing things and taking care of things) more 'complex.' It's no longer a matter of articulating a sophisticated pluralistic space of reasons in which everything (in its complexity) can be accounted for. That may be the case for interdisciplinary affairs within intellectual culture, but the politics of accountability in intercultural affairs aren't complex so much as they're awkward. When the hope of resolving differences is given up, but cooperation is still necessary, the relationships we form are awkward.[63]

The flow of knowledge into and out of the known

The politics of accountability are about the travels of knowledge – across disciplines and places and cultures. The image of knowledge traveling comes from a keen anthropological sensitivity to the fact that knowledge is born into the world in a specific location – within a discipline or the space of a culture. This sentiment is sometimes used to dismiss philosophical questions of truth and universality on the broad grounds of

[63] For example, beneath all of the 'complex' dialogue between the Group, the Company, and officials in Papua New Guinea, and beneath Strathern's sophisticated reading of the issues of accountability in that dialogue, there was a simple and relatively awkward request being made by the Group: they wanted money (roughly 100 million pounds). Such is the connection between spirit tunnels and late capitalism.

cultural relativism.[64] But for knowledge to travel at all and to leave its place of origin – and who is the intellectual that does not want this? – it must have a vision of what's beyond the boundaries of its discipline or culture. Anna Tsing (2005) argues that this vision is manifest in the universal and global aspirations of knowledge claims like laws of nature and declarations of human rights. With an understanding of the normativity of epistemic practices, we can recognize claims to the universal, claims about 'nature', as visions of what the world should look like, and not just the quirky rituals of natural scientists. Feeling the grip of the world, we are compelled to articulate worldly knowledge.

Disciplined intellectual practice in particular, with its vigilant orientation to 'the outside', cannot help but make claims to the universal. However, I believe that most 'knowledge' that is talked about as such – the general or abstract descriptions or rules that are generated in all cultures (e.g., scientific principles, religious or spiritual convictions, 'traditional ecological knowledge', 'folk' philosophies of life or nature) – is born into the world with universal aspirations. These kinds of worldly knowledges are the knowledges that travel and make connections and spark collaboration. For that reason, they are the kinds of knowledges that change the world – not because worldly knowledge is universally true, but because in its universal aspiration, it comes to matter to a lot of people. However, on its travels, worldly knowledge proceeds with 'friction', Tsing's metaphor for "the awkward, unequal, unstable, and creative qualities of interconnection across difference" (2005: 4). Friction is what launches worldly knowledge, but it also transforms its significance along the way. In other words, because

[64] One might point out to cultural relativists that, for all their sensitivity to other cultures, their refusal to confront philosophical issues amounts to a kind of disciplinary arrogance, or the denial of responsibility to other disciplinary cultures.

of the friction it encounters, worldly knowledge – the theory of nuclear fission, for example – doesn't always matter to everyone in the same way.

However, many intellectual practices have assumed that, for the most part, the extension of knowledge is a frictionless process. Tsing expresses this concern with the claim that "those who claim to be in touch with the universal are notoriously bad at seeing the limits and exclusions of their knowledge" (2005: 8). This is a provocative claim, and it gestures toward the connection I want to make between intellectual practices and colonizing practices. But we can think of many different kinds of 'limits' that intellectual practices run up against, some of which are actually very easy to see. Tsing, of course, is talking about a specific kind of limit (which she calls 'gaps'), but since her claim has been taken out of context, let us use it as a vehicle to interpret how different intellectual practices deal with and ignore different kinds of limits and exclusions. It will become clear that the negotiation of epistemic limits, no matter what kind, entails discipline.

The first kind of limit that intellectual practices run up against, and the easiest limit to recognize, is a situation or problem that they have never actually handled before. In fact, most intellectuals actually pursue these limits as a basic goal of their practice — to enact their knowledge over a range of situations they see fit. This kind of limit is exemplified by the arrival of conservation projects in new locations that they have deemed crucial in their vision for the preservation of global biodiversity. The primary mode of discipline that is brought bear on new situations like these is the application of established procedures and categories – the rules we already know. Sometimes new situations are incorporated into a practice very smoothly, but other times the encounter is slowed down by some sort of friction. It has often been the case that friction prompts

intellectuals to impose this primary mode of discipline even more forcefully. This is the kind of discipline that is often criticized for its rigidity, because it figures limits to be unexplored, unfamiliar, or more complex instances of the Same. Disciplinarity rigidity reorders the new in terms of the known, and in so doing, it fails to see another kind of limit, namely the unknown. Before articulating what that limit is, we should pause to appreciate how disciplinary rigidity is a classic intellectual technique of imperialism. Disciplinary rigidity is not simply the ignorance of the unknown; it is the erasure of it. Especially in conservation practices, where intervention into material situations is the name of the game, the ignorant understanding of something mandates its silent destruction.[65]

But disciplinary rigidity is in disrepute in much the academy today. The alternative and decidedly more trendy mode of disciplinary attention is to allow new situations to interrupt our own procedures and categories – to open ourselves up to the possibility that there might be more to learn. The second kind of limit that intellectual practices can run into, then, is the region of unintelligibility that is excluded from their own knowledge. Although these limits are harder to see than the first kind, many intellectuals take this zone of exclusion to be the compass of their practice. This mode of discipline entails opening a space for the unknown to show itself in such a way that we can account for it. We should recognize how this classic scientific orientation toward the unknown is reiterated today in the liberal, postcolonial orientation toward social interests that are excluded by intellectual practice. Exclusion is precisely the kind of issue that is

[65] See Tsing 2005: 182-202 on how some global conservation and development projects attempt to make the Indonesian rainforest intelligible by imposing on it a model of nature inherited from European land-use practices that make a sharp distinction between populated land and wild land. These categories don't apply in rural Indonesia, but disciplinary imperialism "requires landscapes that [are] empty of people and their property rights, … [because] 'forests' [are] by definition empty and wild" (195).

negotiated in the politics of accountability, and it is one of the problems that current interdisciplinary and intercultural practices often aim to overcome. As such, the political debates about 'socially robust science' and 'multiculturalism' are echoes of older epistemological concerns with 'fundamental science' and 'the unity of science'. Even if all of nature can't be represented perfectly in knowledge, all of society ought to be. Friction encountered is usually interpreted as the existence of something that hasn't yet been accounted for. Having taken this interpretation of exclusions this far, we realize that intellectuals are in fact very good at seeing the limits of their claims to the universal; those are the very limits they are constantly trying to open up to and overcome.

However, the kinds of exclusions Tsing is concerned with are much more subtle than 'the limits of representation'. She isn't interested in the in/ability of the human intellect to grasp nature perfectly or to address every stakeholder. The exclusions she's talking about, and the third kind of limit that intellectuals run up against, are the zones of conceptual intractability that epistemic practices actively make:

> Our categories and discriminations always produce zones of "boredom" and unreadability; powerful projects of categorization, including development and conservation (as well as your scholarly reading practices, whatever they may be), produce persistently uninteresting, invisible, and sometimes illegitimate zones—which I call "gaps." Universal knowledge projects cannot be understood without attention to gaps. (2005: 172)

Gaps are the spaces in which worldly knowledge, because of its historical specificity, does not travel. Gaps do not indicate the discrepancy between nature's expanse and man's finitude. Rather, gaps are the new spaces of engagement for conceptual and material exchange that are carved out by intercultural collaborations and global connections. The epistemic practices that make these connections often have trouble accounting for the corresponding gaps, and these are the kinds of exclusions that "those

in touch with the universal are notoriously bad at seeing." In this vein, Tsing talks about the unsettled spaces inside the gap between 'cultivated' and 'wild' plants and inside the gap between 'farmed' and 'forested' land (2005, chp. 5). Most plants and most land in the Indonesian rainforest fall in between these categories, but these gaps are not intuitively grasped or controlled by the Western practices that nevertheless want to mobilize the natural capital of rural Indonesia. These gaps created by the global market open up a space for a distinct kind of indigenous epistemic authority (a discipline!) that did not exist before for them or for anyone else. Tsing's attention to gaps isn't about bridging conceptual dichotomies (like the gap between 'nature' and 'culture') so much as it is about the ongoing conceptualization of new practices and awkward situations as they appear in real time.[66] Unlike the 'hopeful' interpretation of epistemic limits shared by scientific fundamentalists and neoliberal knowledge projects, Tsing's reading of the universality of knowledge doesn't presume that there is an already unified universe into which universal concepts are simply expanding (perhaps always in vain). By paying attention to gaps as regions of conceptual intractability that are produced by the very movement of universals, she wants to acknowledge the role that knowledge has in creating the unified world it's talking about.

Tsing's conception of universality (as an aspiration that encounters 'friction') encourages a more disciplined self-awareness in epistemic practices.[67] Being self-aware in this way does not mean being aware of the potential limits of your own knowledge.

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[66] See Tsing 2005: 162-170 for a 'collaborative' articulation of what biodiversity in the rainforest means to indigenous Meratus Dayaks. Far from being a study of Meratus ethnobiology, Tsing's account is about the 'pleasures of biodiversity' and the pleasures of accounting for it.

[67] Other scholars have understood this self-awareness in terms of holding ourselves accountable to the 'differences' (i.e., gaps) that our knowledge makes in the world (Haraway 1997, Barad 2007). Cori Hayden explains that, "the very act of trying to [understanding phenomena] often makes us party to their materialization. I found, in many cases, that my own attempts to make [bioprospecting] an explicit object of attention and ground for conversation and shared analysis simultaneously had the effect of extending the webs of people for whom it would be a matter of interest in the first place" (2003: 13).

Rather, it means being sensitive to the potential unboundedness of your knowledge and to the effects it has. This is a self-awareness that is in many ways opposed to the philosophical activity of conceptualizing human finitude. This kind of self-awareness is lacking in those intellectual practices that are so concerned with representing (the interests or nature of) the world that practitioners cannot even notice the differences they themselves are making in the world. And yet, even with such self-awareness, one basic norm of intellectual practice remains largely the same: to figure out what it is that hasn't been properly accounted for. The injunction for intellectuals is to contribute to the scholarly literature in meaningful ways; any self-aware intellectual ought to know that. In that sense, gaps are the conceptual kin of epistemic things. As irresistible zones of unintelligibility, epistemic gaps catch our gaze as things that we haven't yet figured out. They are the gaps that actually show up in practice, the unknowns that we know about.[68] This relationship is not accidental, because epistemic gaps are 'produced' (in Tsing's terms) and 'contained' (in Rheinberger's) by the intellectual apparatuses that notice them. Disciplined self-awareness, then, means being conscious of the gaps produced by gap-filling knowledge projects.

When trying to understand the gaps that knowledge fills and makes, I think it is important to recognize how the disciplinary virtues of openness and self-awareness are in conflict, or rather, how they would be if they were ever enacted at the same time. How can one account properly for existing gaps while *at the same time* being conscious of the gaps being made? It seems like the second task always comes after the first is complete, becoming the challenge for the next generation of research. I believe that this temporal disjunction is enforced by the politics of accountability which push intellectual practices

[68] I use the term 'the unknown' to refer specifically to epistemic gaps, not to indicate the ostensibly large expanse of things we don't know. The unknown, as I will discuss it, is always on the verge of being known.

into a hyperactive mode of knowledge production. How this hyperactivity comes about can be understood by looking again at the basic temporal dynamics of scientific practices (the subject of chapter 1).

At their cutting edge, disciplines hold themselves open and accountable to epistemic gaps. Indeed, it is worth pointing out that the epistemic authority conferred upon indigenous knowledge in global scientific projects is the same kind of normative authority accorded to objects in experimental science.[69] Both conferrals are prompted by a politic of knowledge production that appreciates the cognitive limits of the human and values collaboration. Opening themselves up to the unknown, intellectuals solicit it to speak and perform its own existence. For scientific practices, the self-conscious self-expression of unknown is grasped as a set of possibilities (see chapter 1). Knowledge flows out of the unknown as a disclosure of possibilities. The possibilities being disclosed aren't logical possibilities or possible worlds, but 'real possibilities' as negotiated in the politics of accountability (Rouse 2002: 330-350).[70] I believe that this grip on real possibilities (and their grip on us) is part of what it means for knowledge to be born into the world with universal aspirations (Tsing 2005: 1-11). This is how the politics of accountability regulate the flow of knowledge across the cutting edge of

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[69] This claim takes to an extreme the conceptual link between the unknown and the unrepresented, with at least one difference remaining, namely that indigenous people might be willing to learn English. However, the correspondence I see between people and objects is not a reduction of indigenous knowledge, nor is it a dismissal of ethnoscientific projects. Objects, especially objects of inquiry, can be very lively within experimental systems and behave in unpredictable ways (Rheinberger 1997). In fact, objects don't have the kind of 'culture' that many critical scientific minds object to, which often means that objects are more agential than people. It's not that objects don't express culture; it's that the culture they do express is part of the technoscientific practice in which those objects participate. Insofar as scientific practices include their broader material settings (Rouse 1996, Mol 2002), we can see how a scientific culture would include the expressivity of its objects.

[70] Rouse explains that "part of what distinguishes real possibilities from what is merely not-impossible is that real possibilities *matter*. They express a practical configuration of a situation such that there is something at stake in whether and how these possibilities are to be realized" (2002: 25). We may be able to grasp the logical possibilities presented by research, but it is the real possibilities presented by that research that have a grip on us. This is why real possibilities are the possibilities we actually pursue.

research. But the disciplined openness to 'the outside' doesn't stop here. The birth of knowledge transforms the cutting edge of research, sharpening it and reorienting it to a new set of stakes and potential knowledges. This is the production of epistemic gaps – otherwise perceived as the receding horizon of the unknown and the unaccounted for. The inevitable production of epistemic gaps is a side effect of the irreducible normativity of scientific practices. In the course of figuring out what something is, the practice is transformed, and new possible avenues of research are disclosed with something further at stake. Indeed, as Rouse explains, "the term 'at stake' expresses a normative accountability that outruns any definite formulation of what one is accountable to" (2002: 357). Real possibilities breed more real possibilities, and the unknown and the unrepresented multiply.[71]

The proliferation of real possibilities often has a blinding effect, such that opening up one's practice to the unknown feels like opening it up to an entire world of potential significance. It is important to remember that this proliferation is thoroughly entangled with the politics of accountability and not some unrestrained exercise in conceivability. The unknown is a void teeming with potential that matters. Biodiversity is emblematic of what it means to be full of real possibilities.

> Over time, the greatest value of [biodiversity] may be found in the unknown opportunities it provides humanity for adapting to local and global change. The *unknown potential* of genes, species, and ecosystems represents a never-ending *biological frontier* of inestimable but certainly high value. Genetic diversity will enable breeders to tailor crops to new climatic conditions. Earth's biota—a biochemical laboratory unmatched for size and innovation—hold the still-secret cures for emerging diseases. A diverse array of genes, species, and ecosystems is a resource that can be tapped into as

[71] This multiplication can be understood in terms of the history of complexity. Despite the fact that scientific knowledge is ostensibly becoming more and more sophisticated, natural phenomena have never before shown themselves to be so 'complex'. Indeed, the rise of 'complexity' as a fact is intimately linked with the rise of disciplined techniques of attending to 'complexity' (e.g., computers).

> human needs and demands change. (World Resource Institute et al. 1992, quoted in Hayden 2003: 53, emphasis added)[72]

As scientists and humans and whatever else it is that we are, we are responsible for those things in the world that might make a difference in our lives and the lives of others. This responsibility is perhaps best illustrated by a sentiment shared by Jill Tarter (2009), director of research at the SETI Institute, a California-based non-profit organization leading the search for extra-terrestrial life. "From my perspective," she says, "we live on a fragile island of life [i.e., Earth] in a universe of possibilities." According to her, the existence of extra-terrestrials isn't just a logical or metaphysical possibility. She asks (and note the neoliberal conservationist undertones), "Would the discovery of an older cultural civilization out there inspire us to find ways to survive our increasingly uncertain technological adolescence? Might it be the discovery of a distant civilization in our common cosmic origins that finally drives home the message of the bond among all humans?"[73]

These temporal dynamics turn intellectual practice into a moral discourse on possibilities. In this discourse, there is a tendency to fetishize the future as the future present in a way that deflects responsibilities away from the real present, that is, away from the specific ways in which the future is being realized. This is how the disciplined openness to the 'outside' can alienate us from our own agency. I connect this fetishized

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[72] Also see Maclaurin and Sterelny 2008, chp. 8, on the 'option-value' of biodiversity. Working with a pluralistic conception of biodiversity, they argue that what conservation biology ought to be conserving are the possibilities that we might discover in the natural world. We don't yet know all of the ways that nature can actually be valuable to us, so we should retain the option to use at least some parts that we think might be useful in the future: "The option-value approach to conservation biology depends on our being ignorant, but not too ignorant. Since we lack full knowledge about the future, we are wise to hedge our bets, insuring against unpleasant surprises. But we need to be knowledgeable enough to ignore remote possibilities, and to take serious measures to protect against more likely dangers. One important aspect of the world about which we are ignorant is our own future preferences" (156).

[73] See the genre of science fiction for more on why the real possibility of aliens matters to human. Also see Helmreich 2009 on the trope of 'alien' in the cultures of marine biology and microbiology.

futural-orientation of intellectual practices with the colonial spirit of settler cultures (Rose 2004). The imperial ignorance and erasure of the unknown is recast in postcoloniality as the hyperactive appropriation and production of the unknown. It is in this way that disciplines create and recreate their own 'crises' of knowledge. In a similar line of reasoning, Deborah Bird Rose, drawing on the work of Australian historian and curator Jay Arthur, argues that colonization cannot be understood as simply as the occupation of another space: "the colonist stands in relation to the colonised space as creator and re-creator" (Arthur 2003: 112, quoted in Rose 2004: 64). The connection between the re/creation of epistemic gaps and the re/creation of colonized space can be understood better by looking at crises of knowledge that have a distinctly ontological bent like the ecological degradation of Earth. I don't mean to say that conservationists *created* a planetary ecological crisis, but they are now liable for it in a way that makes them enact extreme measures in order to respond to it. This liability is indicative of what it means for conservationists to belong to a disciplinary domain, and it is through the enactment of such extreme measures that Earth comes to belong to conservationists.

Caring for knowledge and caring for nature

To be clear, I am not rejecting the Western disciplines of conservation.[74] Indeed, I enlist myself in their ranks, insofar as I feel the grip that my planet has on me and am keenly aware of how I treat nature. I also enlist myself in the ranks of the colonized (the disciplined), insofar as I feel the grip that conservation practices have on me and am

[74] Wholesale rejections of disciplinary projects (including rejections of science in general) are often made under the false assumption that such a dismissal, if actually carried out, would remove a specified set of beliefs and procedures that are causing all of the problems. But disciplines are not defined by any such set. Indeed, insofar as disciplines are sites of disagreement, rejecting disciplinary projects means nothing less than rejecting what is at stake in such disagreements. Rejecting conservation is tantamount to rejecting 'nature' (whatever it is) as something worth caring for.

keenly aware of how I participate in those practices. The reason I participate in conservationist discourse is not because I think the revolution is going to happen from 'within', as if the disciplines had fixed borders defining such a space 'within'. Indeed, this sentiment can easily promote the kind of intellectual hyperactivity that I want to avoid. My participation in conservation is motivated by an intention similar to that of the Group of indigenous Papuans caught in the throes of intellectual debate over their land: to encourage among conservationists a more robust disciplinary self-awareness, a sensitivity to the effects of their knowledge. Because of the temporal dynamics enforced by the politics of accountability, this self-awareness is a virtue that is normally enacted retrospectively, oriented toward the errors and exclusions made by our disciplinary ancestors. How can we reenact this self-awareness in the present?

Anna Tsing and others have suggested paying attention to the own gaps our knowledge projects makes, but I think this strategy invokes an abstract point of view in the future where we could look back at our actions as they happen. This is the same point of view invoked by practices of audit that try to predict and control the behavior of epistemic things before realizing what they are. Even paying attention to the *differences* our knowledge makes (Haraway 1997, Barad 2007) is a rather abstract injunction to participate in a potentially hyperactive moral discourse. The lessons that we learn from these disciplinary strategies is that not all research topics are appropriate and to choose our objects of inquiry wisely. But they still fetishize the unknown.

I believe that disciplinary self-awareness is a virtue that should be pursued as a way of slowing down the re/production of epistemic gaps. This means intervening in the hyperactive temporal dynamics of intellectual practice, but it does not mean that we ought to look for what it is that intellectual hyperactivity is continually missing. The

answer is that we've missed nothing that matters more than what we already know. Disciplinary self-awareness is not about making new knowledge, but about caring for knowledge as it grows old so that we don't forget about it. What do I mean by this? I mean that we should move more slowly with the knowledge we've got, not simply by paying attention to the differences it makes in the world, but by attending to it with that same spirit even if it seems like it has stopped making a difference.

When knowledge flows over the cutting edge of research, it is infused with a potential that commands everyone's attention. This potential is what gives an object of inquiry its characteristic livelihood and unpredictability. But once we figure out what that object might be, once its potential is initially realized, the hyperactive dynamics of intellectual practice usually push disciplinary attention away from that object to the new set of gaps that it reveals. *I am not going to talk about those gaps. I want to attend to the knowledge has just been realized*. I believe that most knowledge flowing through today's distributed, hyperactive academies is forgotten relatively quickly. That is not to say that such knowledge doesn't have any effect on the practice that uncovered it; indeed, it conditions its discipline to tackle new problems. But once knowledge flows over the cutting edge of research and out of the spotlight, intellectuals no longer pay attention to it in the same way they did when it was unknown. Again, that is not to say that intellectuals don't have to account for it at all; the history of a discipline is an active site of interpretation and dispute. To forget knowledge is not to lose it forever. But wouldn't you agree that the moons of Jupiter are a little less *lively* than they used to be when Galileo was letting them whip through his telescope into the celestial politics of Earth?

To solicit the unknown to express itself and account for itself is to bring forth the life of world. Self-conscious self-expression is hard, and it's easy for humans to

forget that, but that's what it means to exist in a human's world. The discipline it takes to nurture what we know into existence must be conjoined with the discipline it takes to care for that knowledge as it grows old. Following Rose's Aboriginal logic, I understand disciplined self-awareness to be a relentless confrontation of our origins, not as a fixed albeit debatable point in 'the past', but as 'the source' that is producing the ongoing flow of liveliness called the present. For experimental science, this might entail paying more attention to the ancestral lineages of technical objects in whose tracks follow epistemic things. For conservation, this self-awareness means recognizing that for all of the *potentially harmful* effects that we have had on our planet, we also know what's good for it, even if we've forgotten what that is. Disciplined self-awareness, then, charges conservationists with the task of remembering how it is that humans got along so well with nature that they actually started to care for it, to feel its grip. Caring for knowledge will help us remember how it is that we care for nature.

REFERENCES

Apffel-Marglin, Frédérique. 2005. The Trouble with Natural Relations: Steps to Reintegrating Matter and Spirit. Prepared for the conference Ecological Threats and New Promises of Sustainability for the 21st Century, Queen Elizabeth House 20th anniversary, Oxford.

Argyrou, Vassos. *The Logic of Environmentalism: Anthropology, ecology, and postcoloniality*. Oxford: Berghahn Books.

Arthur, Jay. 2003. *The Default Country: A Lexical Cartography of Twentieth-Century Australia*. Sydney: University of New South Wales Press.

Barad, Karen. 2007. *Meeting the Universe Halfway: Quantum physics and the entanglement of matter and meaning*. Durham: Duke University Press.

Bechtel, William. 1986. Biochemistry: A Cross-disciplinary Endeavor that Discovered a Distinctive Domain. In *Integrating Scientific Disciplines*, ed. W. Bechtel, 77-100. Dordrecht: Martinus Nijhoff.

————. 1993. Integrating sciences by creating disciplines: The case of cell biology. *Biology and Philosophy* 8(3): 277-99.

————. 2006. *Discovering Cell Mechanisms: The creation of modern cell biology*. Cambridge: Cambridge University Press.

Beer, Gillian. 1996. *Open Fields: Science in cultural encounter*. Oxford: Oxford University Press.

Blaser, Mario. 2009. The Threat of the Yrmo: The political ontology of a sustainable hunting program. *American Anthropologist* 111(1): 10-20.

Bowman, Dave. 1998. The impact of Aboriginal landscape burning on the Australian biota. *New Phytologist* 140(3): 385-410.

Cartwright, Nancy. 1994. Fundamentalism vs. the Patchwork of Laws. In *Proceedings of the Aristotelian Society* 94: 279-292.

Cat, Jordi, Nancy Cartwright, and Hasok Chang. 1996. Otto Neurath: Politics and the Unity of Science. In *The Disunity of Science: Boundaries, contexts, and power*, ed. P. Galison and D. Stump. Stanford: Stanford University Press.

de Castro, Eduardo Viveiros. 1998. Cosmological Deixis and Amerindian Perspectivism. *The Journal of the Royal Anthropological Institute* 4(3): 469-488.

Ellen, Roy, ed. *Modern Crises and Traditional Strategies: Local ecological knowledge in island Southeast Asia*. Oxford: Berghahn Books.

Grossinger, Richard. 1970. *Solar Journal: oecological sections*. Los Angeles: black sparrow press.

Guha, Ramachandra. 1989. Radical American Environmentalism and Wilderness Preservation: A Third World Critique. *Environmental Ethics* 11(1): 71-83.

Haugeland, John. 1998. *Having Thought: Essays in the metaphysics of mind*. Cambridge: Harvard University Press.

Haraway, Donna. 1989. Teddy Bear Patriarchy: Taxidermy in the Garden of Eden, New York City, 1908-36. In *Primate Visions: Gender, race, and nature in the world of modern science*. New York: Routledge.

————. 1997. *Modest_Witness@Second_Millenium.FemaleMan©Meets_Onco-Mouse™*. New York: Routledge.

Hayden, Cori. 2003. *When Nature Goes Public: The making and unmaking of bioprospecting in Mexico*. Princeton: Princeton University Press.

Helmreich, Stefan. 2009. *Alien Ocean: Anthropological voyages in microbial seas*. Berkeley: University of California Press.

Ingold, Tim. 2004. A Circumpolar Night's Dream. In *Figured Worlds: Ontological obstacles in intercultural relations*, ed. J. Clammer, S. Poirier and E. Schwimmer. Toronto: University of Toronto Press.

Jacobs, Jerry. 2009. Interdisciplinary Hype. *The Chronicle of Higher Education*, November 22. Online version. http://chronicle.com/article/Interdisciplinary-Hype/49191/.

Keller, Evelyn Fox. 2000. *The Century of the Gene*. Cambridge: Harvard University Press.

Kellert, Stephen, Helen Longino, and C. Kenneth Waters, eds. 2006. *Scientific Pluralism: Minnesota Studies in the Philosophy of Science, Vol. IX*. Minneapolis: University of Minnesota Press.

Klinger, Terrie, Dianna Padilla, and Kevin Britton-Simmons. 2006. Two invaders achieve higher densities in reserves. *Aquatic Conservation* 16(3): 301-311.

Lange, Marc. 2000. *Natural Laws in Scientific Practice*. Oxford: Oxford University Press.

————. 2002. Who's Afraid of *Ceteris-Paribus* Laws? Or: How I Learned to Stop Worrying and Love Them. *Erkenntnis* 57: 407-423.

————. 2008. Laws and Theories. In *A Companion to the Philosophy of Biology*, ed. S. Sarkar and A. Plutynski, 489-505. Malden: Blackwell Publishing.

Latimer, Joanna. 2004. Commanding materials: re-accomplishing authority in the context of multi-disciplinary work. *Sociology* 38(4): 757-775.

Latour, Bruno. 1987. *Science in Action: How to follow scientists and engineers through society*. Cambridge: Harvard University Press.

Lenoir, Timothy. 1997. *Instituting Science: The cultural production of scientific disciplines*. Stanford: Stanford University Press.

MacIntyre, Alasdair. 1980. Epistemological Crises, Dramatic Narratives, and the Philosophy of Science. In *Paradigms and Revolutions: Appraisals and applications of Thomas Kuhn's philosophy of science*, ed. G. Gutting, 54-74. Notre Dame: University of Notre Dame Press.

————. 2007 [1981]. *After Virtue: A study in moral theory, Third Edition*. Notre Dame: University of Notre Dame Press.

Maclaurin, James and Kim Sterelny. 2008. *What is Biodiversity?* Chicago: University of Chicago Press.

Mol, Annemarie. 2002. *The Body Multiple: Ontology in medical practice*. Durham: Duke University Press.

Neurath, Otto. 1983. *Philosophical papers, 1913-1946*, ed. and trans. R. Cohen and M. Neurath. Dordrecht: Reidel.

Norton, Bryan. 2008. Biodiversity: Its Meaning and Value. In *A Companion to the Philosophy of Biology*, ed. S. Sarkar and A. Plutynski, 368-389. Malden: Blackwell Publishing.

Oppenheim, Paul and Hilary Putnam 1958. The Unity of Science as a Working Hypothesis. In *Concepts, Theories, and the Mind-Body Problem: Minnesota Studies in the Philosophy of Science, Vol. II*, ed. H. Feigl, M. Scriven, and G. Maxwell, 3-36. Minneapolis: University of Minnesota Press.

Pickering, Andrew. 1984. *Constructing Quarks: A sociological history of particle physics*. Edinburgh: Edinburgh University Press.

Primack, Richard. 1993. *Essentials of Conservation Biology*. Sunderland: Sinauer Associates, Inc.

Raffles, Hugh. 2002. *In Amozonia: A natural history*. Princeton: Princeton University Press.

Rheinberger, Hans-Jörg. 1995. From Microsomes to Ribosomes: 'Strategies' of 'Representation,' 1935-55. *Journal of the History of Biology* 48: 49-89.

————. 1997. *Toward a History of Epistemic Things: Synthesizing proteins in the test tube*. Stanford: Stanford University Press.

Rose, Deborah Bird. 2004. *Reports from a Wild Country*. Sydney: University of New South Wales Press.

Rouse, Joseph. 1996. *Engaging Science: How to understand its practices philosophically*. Ithaca: Cornell University Press.

————. 2002. *How Scientific Practices Matter: Reclaiming philosophical naturalism*. Chicago: University of Chicago Press.

Sahlins, Marshall. 1976. *The Use and Abuse of Biology: An anthropological critique of sociobiology*. Ann Arbor: University of Michigan Press.

Sanga, Glauco and Gherardo Ortalli. 2003. *Nature Knowledge: Ethnoscience, cognition, and utility*. Oxford: Berghahn Books.

Sarkar, Sahotra. 2002. Defining "Biodiversity"; Assessing Biodiversity. *The Monist* 85(1): 131-155.

Soulé, Michael. 1985. What is Conservation Biology? *BioScience* 35: 727-734.

Soulé, Michael and Bruce Wilcox. 1980. *Conservation Biology: An evolutionary-ecological perspective*. Sunderland: Sinauer Associates, Inc.

Strathern, Marilyn. 2003. *Commons and Borderlands: Working papers on interdisciplinarity, accountability and the flow of knowledge*. Oxford: Sean Kingston Publishing.

Tarter, Jill. 2009. Why the search for alien intelligence matters. Lecture delivered at the TED2009 Conference, February 2009. Online version: http://www.ted.com/index.php/talks/jill_tarter_s_call_to_join_the_seti_search.html.

Tenured Radical. 2009. We're Here, Because We're Here, Because We're Here, Because We're Here! Or, Why Disciplines Rule the University Roost. November 27. http://tenured-radical.blogspot.com/2009/11/were-here-because-were-here-because.html

Thompson, Charis. 2002. When Elephants Stand for Competing Philosophies of Nature: Amboseli National Park, Kenya. In *Complexities: social studies of knowledge practices*, ed. A. Mol and J. Law, 166-190. Durham: Duke University Press.

Traweek, Sharon. 2000. Faultlines. In *Doing Science + Culture*, ed. R. Reid and S. Traweek, 21-48. New York: Routledge.

Tsing. Anna. 2005. *Friction: An ethnography of global connection*. Princeton: Princeton University Press.

Vaughn, Dawn and Richard Strathmann. 2008. Predators induce cloning in echinoderm larvae. *Science* 319(5869): 1503.

Western, David. 1989. Conservation Without Parks: Wildlife in the Rural Landscape. In *Conservation for the Twenty-first Century*, ed. D. Western and M. Pearl, 158-165. Oxford: Oxford University Press.

————. 1994. Ecosystem Conservation and Rural Development: The Case of Amboseli. In *Natural Connections: Perspectives in community-based conservation*, ed. D Western and R. Wright, 15-52. Washington DC: Island Press.

Wilson, E.O. 1984. *Biophilia: The human bond with other species*. Cambridge: Harvard University Press.

World Resource Institute, The World Conservation Union, United Nations Environment Programme, Food and Agriculture Organization of the United Nations, and United Nations Educational Scientific and Cultural Organization. 1992. *Global Biodiversity Strategy: Guidelines for action to save, study, and use Earth's biotic wealth sustainably and equitably*. Washington DC: World Resources Institute.