Landsat in Contexts: Deconstructing and Reconstructing the Data-to-Action Paradigm in Earth Remote Sensing

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ABSTRACT

There is a common theme at play in our talk of data generally, of digital earth data more specifically, and of environmental monitoring most specifically: more data leads to more action and, ultimately, to societal good. This data-to-action framework is troubled. Its taken-for-grantedness prevents us from attending to the processes between data and action. It also dampens our drive to investigate the contexts of that data, that action, and that envisioned societal good. In this dissertation, I deconstruct this data-to-action model in the context of Landsat, the United States' first natural resource management satellite. First, I talk about the ways in which Landsat's data and instrumentation hold conflicting narratives and values within them. Therefore, Landsat data does not automatically or easily yield action toward environmental preservation, or toward any unified societal good. Furthermore, I point out a parallel dynamic in STS, where critique is somewhat analogous to data. We want our critiques to yield action, and to guide us toward a more just technoscience. However, critiques—like data—require intentional, reconstructive interventions toward change. Here is an opportunity for a diffractive intervention: one in which we read STS and remote sensing through each other, to create space for interdisciplinary dialogue around environmental preservation. A focus on this shared goal, I argue, is imperative. At stake are issues of environmental degradation, dwindling resources, and climate change. I conclude with beginnings rather than endings: with suggestions for how we might begin to create infrastructure that attends to that forgotten space between data, critique, action, and change.

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GENERAL AUDIENCE ABSTRACT

I have identified a problem I call the *data-to-action paradigm*. When we scroll around on Facebook and find articles — citing pages and pages of statistics — on our rapidly melting glaciers and increasingly unpredictable weather patterns, we are existing within this paradigm. We have been offered evidence of looming, catastrophic change, but no suggestions on what to do about it. This is not only happening with climatological data and large-scale environmental systems modelling. Rather, this is a general problem across the field of Earth Remote Sensing. The origins of this *data-to-action paradigm*, I argue, can be found in old and new rhetoric about Landsat, the United States' first natural resource management satellite. This rhetoric often says that Landsat — and other natural resource management satellites' — data is a way toward societal good. The more data we have, the more good will proliferate in the world. However, we haven't been specific about what that good might look like, and what kinds of actions we might take toward that good using this data. This is because, I argue, Earth systems science is politically complicated, with many different conceptions of societal good. In order to be more specific about how we might use this data toward some kind of good we must (1) explore the history of environmental data, and figure out where this rhetoric comes from (which I I do in this dissertation), and (2) encourage interdisciplinary collaborations between Earth Remote Sensing scientists, social scientists, and humanists, to more specifically flesh out connections between digital Earth data, its analyses, and subsequent civic action on such data.

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Dedication

For Harvey Fried. You observed the world around you with nuance and thoughtfulness, and you inspire me to strive toward the same.

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Introduction

For the past five years, I have been both a scholar of Remote Sensing, and of Science, Technology, and Society (STS). Remote sensing is a broad field, focusing on data collection from afar, using machinery such as satellites and drones. STS is a broad field, too, largely relating to the history, philosophy, sociology, anthropology, and policy of technoscience. Remote Sensing and STS are both mix cultures of sorts, drawing from a variety of disciplines and using a variety of theories and methods. Both fields are highly data-focused, but in different ways. Remote Sensing collects, archives, refines, and analyzes data; STS observes data collecting, archiving, refining, and analyzing practices, and oftentimes critiques the ways in which we too easily separate the data (and the instrumentation that collected it) from the people, institutions, and policies that brought that data (and instrumentation) to life.

I have spent the most time with Remote Sensing research that is ecological in nature, and often dealing with the conservation of natural resources. I've become fascinated with the ways in which environmental data itself is often treated like a natural resource. There are so many ways to collect it, to use it, to preserve it over time, and to combine it with other data resources. Furthermore, it has become a promise toward a better future. Our earthly resources provide us with sustenance and livelihood. Digital earth data depicts that sustenance and livelihood, and promises to make it more manageable; more attainable; more sustainable, This framing finds its roots even at the very beginning of public earth remote sensing programs. For instance, in 1970, space policy experts John Hannessian Jr. and John Lodgson wrote that

> [t]he top-priority questions of the U.S. during the present decade may not reflect an overriding concern with a global balance-of-power struggle, but rather with finding a way to restore order both at home and in the world [...] concentrating energies on improving the management of environment to ensure quality of life for all the planet's inhabitants. The relevance of space activity to these goals seems to lie in those applications of space technology which produce direct tangible benefits for society in general (60).

In other words, a new kind of natural resource management, marked by the use of earth remote sensing technologies and the data they produce, are part of order-restoring practices in STEM and STEM policy. Earth remote sensing data provides a kind of management of those resources from a different lens and a different angle (literally). This data, from the beginning, was framed as an important resource toward those ends of societal good and restoration. We find similar framings today. For instance, in a decadal survey of environmental remote sensing needs, Simmons et. al. write that

> Much of the land surface is no longer in its natural state, and the chemical composition of the atmosphere and ocean is being changed in ways that change climate and affect life. Present activities may leave their mark on the state of the environment for millenia into the future. It has become imperative to monitor, understand and where possible predict many aspects of environmental variability and change. This requires that observations of sufficient quality, quantity and regularity be made of the key variables of what has come to be called the Earth system. [...] Understanding is also needed to evaluate and interpret the results of the monitoring, forecasting and the projects that are based on prescribed scenarios for uncertain factors causing environmental change, notably future anthropogenic emissions of key gases and aerosols, and future land use (2039; 2016).

These decadal surveys shape environmental research in earth remote sensing, and set field-wide goals toward ideal kinds of research. Here, there is less explicit mention of some social benefit. However, the benefit is implied in the "imperative to monitor, understand and [...] predict." Our environment is changing and, in order for us to regain some control of that change, we must understand it through data. There are many earth remote sensing satellites that provide this imperative data. The focus of my dissertation, however, is Landsat. I chose Landsat as my case study, of sorts, because it was the first earth remote sensing satellite of its kind in the United States, and because it is often considered *the gold standard* (Goward et. al. xviii; 2017, of Moskowitz 2012) for moderate resolution satellite data. In *Landsat's Enduring Legacy* (2017), an internal history written by Goward et. al., the authors note that

> In 2011, the Landsat data archive joined the Gutenberg *Bible*, Tolstoy's personal library, Ireland's *Book of Kells*, and the nearly 300 other entries inscribed on the United Nations Educational, Scientific, and Cultural Organization (UNESCO)'s Memory of the World Register, a record of international documentary collections selected on the basis of world significance and outstanding universal value. Referring to Landsat's archive and why it should be protected for the benefit of all humanity, the nomination submitted to UNESCO for consideration noted, 'Scientists worldwide recognize that there exists only one accurate image record, spanning nearly four decades, of the Earth's land surfaces, coastlines, and reefs at a scale revealing both natural and human-induced change'" (xviii; 2017).

I make note here, particularly, of the connection between Landsat's long-term archive of data and societal benefit. Similar connections are made on Landsat's homepage on the United States Geological Survey (USGS) website: "The long temporal record of data from Landsat satellites has remained remarkably unbroken, proving a unique resource to assist a broad range of specialists in managing the world's food, water, forests, and other natural resources for a growing world population." Lauer et. al. recognize Landsat as a unique and societally-beneficial accomplishment as well, noting that "[t]he United States pioneered land remote sensing from space and has been the unquestioned leader of this unique technology. Americans take pride in having developed the Landsat program and other, more recent, civilian programs" (831; 1997).

These sources, as well as many others, all speak to the canon of Landsat rhetoric, and their authors and sources represent major players in Landsat history. This emphasis on the connections between long-term earth data and societal good are the main concern of this dissertation. Namely, I will argue that Landsat data does not automatically produce action, or a subsequent societal good. Firstly, Landsat itself has a complicated history, and user groups with differing conceptions of societal good (and of action toward it). Secondly, the science of conservation, within which Landsat sits, itself has many conflicting narratives within it, and Landsat is one instance of this. And thirdly, processes by which we might turn data into action are unclear, as is the concept of *societal good* itself.

Furthermore, my dissertation aims to make a similar intervention into STS. STS often aims to critically participate in these sociotechnical systems, and to land critiques that make such systems more equitable to a wider variety of communities (both human and non-human). Some of us attend, especially, to creating sociotechnical systems that are fairer to marginalized communities (both human and non-human). I fear our models of critique, however, are similar to the data-to-action model, except they are *critique-to-action*. I worry, as Bruno Latour did over the last 20 years, that our paradigm has us believing that more — and purer — critique automatically leads to action, and therefore to some societal good. However, we ought to investigate each level of that paradigm as well: how does critique lead to action, what steps would we take to ensure that happens, and what do we feel is some agreed-upon societal good? To do this, I'll draw from frameworks in Engaged STS, Feminist STS, Virtue Ethics, and Philosophy of Technology.

The way forward, I believe, is a kind of diffraction.

I quite like this term, because it suits the nature of this project with respect to the STS and the Remote Sensing: *diffraction* is significant to both communities. In this sense, I am using a visual metaphor — one that deals in patterns of light wavelengths — to explore a technology that visualizes patterns of light wavelengths. So, to be clear to those in remote sensing: from this point forward, I am using the term diffraction metaphorically, and not literally. Metaphorically, I'm diffracting knowledge from those two communities through each other, rather than comparing or contrasting them, or trying to force a way of thinking from one community onto the other. Additionally, this idea of diffraction is directly related to the idea of intervention. When you diffract two kinds of communities through each other, you are making an intervention. However, the intervention isn't a forcible change on one community from another. Instead, both communities learn from each other and are changed in the process.

Diffractive Interventions: A Theoretical Infrastructure

To make these diffractive interventions, I draw from a handful of scholarly traditions in this respect: from theorists who put this question (or questions akin to it) at the forefront of their work. If your primary community is Remote Sensing, this section may not be interesting or necessary for you to read. I invite you to grapple with it, and I have written it in language that I hope is not too opaque. However, if it really does not interest you, feel free to skip it. If you are in STS, though, you ought to read what follows. It is crucial to your understanding of where my dissertation sits and, more broadly, where I tend to sit within our field.

First, there is Bruno Latour. Latour is a continental philosopher, an anthropologist, a sociologist, and a historian of science and technology. He is one of the most influential scholars in the field of STS. He is truly cannon. To his most vehement critics, he represents the old guard of STS. A reasonable critique of Latour says something like this: he does not speak to marginalized communities, at least not explicitly. He also tends to call for us to critique more effectively, while still solely occupying the role of critical theorist in academia (Fortun 2014). *How can we disrupt systems of knowledge/power if we do not call out the empowered and the disempowered in such a system?*, Latour's critics may ask. It is a legitimate critique.

Latour's work is concerned with questions like: Why do we have so much scientific information about issues like climate change, yet we still don't have clear, actionable, democratic ways to address what the data has shown us (*The Politics of Nature*; 1999)? Why is it that we seem unable to address sociopolitical issues in tandem with scientific ones and, still even more vexing, why is it that we STS scholars seem unable to recouple these issues for scientific and public audiences in ways that successfully intervene and create change (1991's *We Have Never Been Modern*, and also 2004's essay, *Has Critique Run Out of Steam?*)? I will argue that, in considering questions about how critiques are made, how knowledge is received, and how change is created through the sociotechnical project, Latour *is* asking questions that deal directly with structures of knowledge/power, and with marginalization of certain kinds of knowledge. In addressing marginalization of knowledge-types — and resistance to hybrid forms of knowledge — I believe we can use Latour's insights to speak more directly to issues of hybrid communities.

In *We Have Never Been Modern*, Latour asserts that "[a]ll of culture and all of nature get churned up again every day."

> Yet no one seems to find this troubling. Headings like Economy, Politics, Science, Books, Culture, Religion, and Local Events remain in place as if there were nothing odd going on. The smallest AIDS virus takes you from sex to the unconscious, then to Africa, tissue cultures, DNA and San Francisco, but the analysts, thinkers, journalists and decision-makers will slice the delicate network traced by the virus for you into tidy compartments where you will find only science, only economy, only social phenomena, only local news, only sentiment, only sex. Press the most innocent aerosol button and you'll be heading for the Antarctic, and from there to the University of California at Irvine, the mountain ranges of Lyon, the chemistry of inert gases, and then maybe to the United Nations, but this fragile thread will be broken into as many segments as there are pure disciplines. By all means, they seem to say, let us not mix up knowledge, interest, justice, and power. Let us not mix up heaven and earth, the global stage and the local scene, the human and the nonhuman. 'But these imbroglios do the mixing,' you'll say, 'they weave our world together!' 'Act as if they didn't exist,' the analysts reply. [...] [O]n the left, they have put knowledge of things; on the right, power and human politics (2).

Here, Latour argues that we as analysts — perhaps, as academics — have a tendency to break the most important issues of our time into tiny, disciplinary parts: into "as many segments as there are pure disciplines" (3). The counterargument, he asserts, is that this is simply the paradigm that we live in. We exist in a world full of disciplines that handle different segments of the same issue, and this way of segmenting is institutionally embedded.

Let's consider, for a moment, Landsat. There are the engineers who design and build the instruments, and who launch and operate the satellite. There are also the geographers and geoscientists, the folks in forestry (who are not necessarily foresters), and the statisticians, who discuss the needs that the satellites must meet, the storage and cataloguing of the data, the analyses that can be done, and the ways in which these analyses help us better understand the environments in which we live. These groups, more and more, are talking to each other. The Remote Sensing Interdisciplinary Graduate Education Program at Virginia Tech, the group of researchers to whom I belong, is an effort to address the necessity that these groups talk to each other. In order to build satellites that can tell us important things about forests, soil, fault lines, bodies of water and water cycles, and so on, there must be communication between the builders of these instruments, and the analysts (and communicators) of the phenomena the instruments study.[1]

However, there are additional, *sociotechnical* aspects of remote sensing that the field, in general, struggles to incorporate, or to take seriously as legitimate research that truly *belongs*. Remote sensing, as a field, has yet to fully address issues such as knowledge-creation, history, ontology, ethics, politics and power, aesthetics, metaphysics, economics, and history. My dissertation cannot address all of those issues, or even a single one to its fullest extent. I will mainly address issues of knowledge-creation, materiality, and history. I address this selection of issues because I am most qualified to address them. However, my set of concerns is by no means the boundary of this project

[1] I should note, too, that physicists and engineers bring an additional kind of understanding of earthly phenomena, at the level of electricity and magnetism — and of the interactions and behaviors of light within that environment.

or this topic. Furthermore, I believe there are ways to address my main question — *How can we deconstruct and reconstruct the data-to-action paradigm in Remote Sensing, and the critique-to-action paradigm in STS?* — that are ontological, ethical, political and dealing with power, aesthetic, metaphysical, and economic. I offer some suggestions for these equally important projects in my conclusory chapter, but they are by no means an exhaustive list. I am, if nothing else, a *modest witness* (Haraway 1997).

Our reluctance to link together modest witnesses of different stripes is an important facet of my dissertation. Latour addresses this in *We Have Never Been* Modern. He argues that our lack of connection, for instance, between the history of satellite development and satellite development itself (in other words, our inability to connect issues of history, knowledge-creation, and ontologies across disciplinary boundaries), are a function of modernism. The crux of Latour's essay is that modernism consists of two practices: (1) the practice of *translation*, which is the creation of hybrid topics in our world (operationally, this could be land remote sensing: its history with respect to aerial reconnaissance and DoD-funded science, its impact on our understanding of weather and climate change, the hefty tasks of storing and sharing remotely-sensed data — all interdisciplinary, hybridized topics); (2) the process of purification, or the sorting of these hybrid topics into "two entirely distinct ontological zones: that of human beings on the one hand; that of nonhuman on the other" (10). He argues:

> Without [translation], the practices of purification would be fruitless or pointless. Without [purification], the work of translation would be slowed down, limited, or even ruled out. [Translation], for example, would link in one continuous chain the chemistry of the upper atmosphere, scientific and industrial strategies, the preoccupations of heads of state, the anxieties of ecologists; the second would establish a partition between a natural world that has always been there, a society with predictable and stable interests and stakes, and a discourse that is independent of both reference and society. So long as we consider

> these two practices of translation and purification separately, we are truly modern — that is, we willingly subscribe to the critical project, even though that project is developed only through the proliferation of hybrids down below. As soon as we direct our attention simultaneously to the work of purification and the work of hybridization, we immediately stop being wholly modern, and our future begins to change. At the same time we stop having been modern, because we become retrospectively aware that the two sets of practices have always already been at work in the historical period that is ending. Our past begins to change (11).

Here, Latour is saying that we arbitrarily separate the processes of translation and purification, and this is what makes us *modern*. Were we to "direct our attention simultaneously to the work of purification and the work of hybridization," as he says, we would stop being modern. And as we would have awareness about these arbitrary separations between translation and purification, the way we consider the present, past, and future would change. Latour argues here that even history itself, in some sense, would change, as the way we write it would take on a new flavor. Furthermore, I interpret a subtle (or, perhaps, not-so-subtle) criticism of the critic who favors the humanist perspective over the scientific one: that scholar, too, subscribes to the world of modernism, and proliferates the dynamic of purification. This speaks to my diffractive intervention into STS, and my analogy between the data-to-action paradigm and the critique-to-action paradigm. They both ascribe to the modernist project and, hence, facilitate further purification.

Latour is not the only scholar thinking along these lines. Political theorist William Connolly makes a similar assertion in his 1995 book *The Ethos of Pluralization*.

"This correlation between pluralization and fundamentalization is not accidental," he says,

> for each conditions the other: each drive to pluralization is countered by a fundamentalism that claims to be authorized by a god or by nature. Moreover, any drive to pluralization can itself become fundamentalized. These two drives participate, therefore, in the same political matrix (xii).

Here, Connolly uses pluralization to describe a push for diversity of thought and of people, while fundamentalization is the struggle against diversity, and a "demand [for a] reinstatement of a unified faith, race, reason, gender duality, normal sexuality, nation and/or territory that was never secure" (ibid).

This bold hypothesis coordinates well with Latour's, particularly given his assertion that "[…] [purification] has made translation possible: the more we forbid ourselves to conceive of hybrids, the more possible their interbreeding becomes […] " (12; 1991). I (and Latour) extend this argument as follows, given Connolly's words: we often see our struggle against the act of purification as one that will bring about liberation. However, it can serve to demarcate the scientific project from the humanist one in such a way as to be a further act of purification. In other words, as we in STS fight against the data-to-action paradigm with a critique-to-action paradigm, the critique has the potential to speak past those whom we'd like to have hear it. This only serves, I fear, to deepen the separation between our field and others, while imbuing these fields with complexities that become more difficult to parse out.

What is at stake? And how can theoretical frameworks in STS — and beyond — guide us?

If we do not address the proliferation of hybrids within Remote Sensing *and* STS, I fear that two things will happen: (1) I fear that all of this long-term data collection in

Remote Sensing won't be actionable in large, meaningful ways toward a societal good, as we won't have addressed the elephants of action *or* societal good, or the ways in which the two are connected; (2) STS will continue to make beautifully thought-out critiques that won't do what we'd like to see them do in the world, as we also haven't defined how critique becomes action, and how action becomes some sort of equitable good. STS and Remote Sensing have a major commonality here, in that we tend to create more of what we already have, in hopes that it will become more compelling. The work of both fields involves so much interdisciplinary knowledge already, however, that I think it becomes overwhelming to envision even further disciplinary connections. However, I think we can use these commonalities to our advantage: to work together toward more developed conceptions of data, critique, action, and the ever-ephemeral societal good.

To do this, we must recognize the hybrid knowledges when we see them, and fully acknowledge their entanglements. I see this dissertation as a beginning attempt at tracing entanglements of hybrids in Remote Sensing, and within the Landsat satellite program specifically. I see it, simultaneously, as a way of tracing hybrid knowledges in our engagement with critique, with scientific communities, and with institutional power structures.

There is other important STS work about creating these places where hybrid knowledges can be exchanged, namely the body of work on *trading zones* and *boundary objects*. The concept of a *trading zone* as STS-ers use it originated with Peter Galison, and was carried further by Harry Collins, Robert Evans, and Mike Gorman. In *Image and Logic: A Material Culture of Microphysics*, Galison calls the trading zone "an intermediate domain in which procedures could be coordinated locally even where broader meanings clashed”:

> Within a certain cultural arena, two dissimilar groups can find common ground. They can exchange fish for baskets, enforcing subtle equations of correspondence between quantity, quality, and type, and yet utterly disagree on the broader (global) significance of the items exchanged. Similarly, between scientific subcultures of theory and experiment, or even between different traditions of instrument making or different subcultures of theorizing, there can be exchanges (coordinations), worked out in exquisite local detail, without global agreement (46; 1997).

In these trading zones, there are things that Star and Griesemer have termed *boundary objects*. Boundary objects are “objects of scientific inquiry [that] inhabit multiple social words, since all science requires intersectional work” (392; 1989). Collins, Evans, and Gorman extend Galison, along with Star and Griesemer, as they identify different kinds of trading zones and trading zone configurations. Specifically, Collins, Evans, and Gorman develop the notion of the *inter-language trading zone*, which springs up around “the development of technologies such as radar and high-energy physics particle detectors which involve/d communication between physicists and engineers who [are] culturally dissimilar […]. […] [T]he resolution of communications via jargons, pidgins, and creoles” (658; 2007) happen within an *inter-language trading zone*. This is what I hope to create between Remote Sensing and STS, by way of certain theoretical frameworks and styles of reasoning, including Latour, *diffraction*, postphenomenology, and Habermassian dialogue.

The question is: How to begin a paradigm in which translation and purification — and in which data-collection, critique, and action toward some agreed-upon progress toward good — are simultaneous processes *through* an inter-language trading zone? It seems as though rhetoric around technologies like Landsat makes the separation possible, or at the very least heavily supports the separation. In order to translate and purify simultaneously, we will have to change the way we think, talk, and engage about topics like Landsat. Scholars William Connolly and Jürgen Habermas, both political theorists, offer models for how this work could be possible. Much like Foucault, Connolly's abstract language leaves a great deal open to the interpretation of the reader. Connolly's orientation to something we in STS call *critical participation* is a *critical responsiveness*. "[…] [T]he recurrent disjunction between injuries suffered by particular constituencies," writes Connolly, "and the barriers to their rectification posed by cultural *codes* of morality and normality requires medication by an *ethos* of critical responsiveness never entirely reducible to a code" (xvi; 1995).

Let's pretend, for a moment, that we are STS-ers who feel that scholars in certain STEM fields, say Remote Sensing, are unaware of the power structures that they occupy, the privilege of their funding and instrumentation, and the harm that the military inclinations of their technologies, for instance, can do in the creation of knowledge and research. Let's also pretend that STS-ers have made a critique that scholars in the Remote Sensing community have heard, and that some scientists and engineers in that community want to begin to make moves to rectify the situation. The problem, according to Connolly, is that members of each community live and act in such different ways that it is difficult to find a way for them to communicate with each other. In other words, even if the critique lands, and all groups are on board, finding a way to make change together is difficult, given everyone's different backgrounds and different disciplinary languages. We must acknowledge these incommensurabilities[2] (Kuhn 1962) in order to work through them. This involves a simultaneous commitment to criticality and justice, as well as an openness to shifting identities across *both* communities. Says Connolly:

> The key challenges to a pluralist culture are first to cultivate critical responsiveness to new movements of pluralization and then to negotiate modified relations of coexistence as new identities cross the magic threshold of enactment. […] [T]o *become* something new is to move the self-recognition and relational standards of judgment endorsed by other constituencies to whom you are connected. Identities are always collective and relational: to be white, female, homosexual, Canadian, atheist, and a taxpayer is to participate in a diverse set of collective identifications and to be situated in relation to a series of alter identifications. Hegemonic identities depend on existing definitions of difference to be. To alter your recognition of difference, therefore, is to revise your own terms of self-recognition as well. Critical responsiveness thus moves on two registers: to redefine its relation to others[,] a constituency must also modify the shape of its own identity. In that sense critical responsiveness is always political. It is a political response to the politics of identify/difference that already precedes its intervention (ibid).

The central thesis of this argument is commensurable with that of Latour's from previous pages. On one hand, political intervention requires us to acknowledge the identities and separation between groups (an act of purification). On the other hand, political intervention requires us to acknowledge the hybridization that will happen as we communicate across boundaries and form new alliances (a proliferations of hybrids). The intervention, there, is not only in the community to whom we in STS target our critique, but also takes place within our own community. These ideas from *critical responsiveness* are also commensurable with *diffractive* theories by Karen Barad and and Donna Haraway, which I will briefly explain later on in this introduction, and also in Chapter 3.

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[2] Incommensurability is a commonly-used word in STS, and comes from Philosopher of Science Thomas Kuhn's 1962 book *The Structure of Scientific Revolutions*. In this book, Kuhn uses to describe the discord between scholars who work within different paradigms of the same field (namely physics). For instance, if my expertise is in Electromagnetic Theory, and yours' is in Quantum Mechanics, we may talk past each other. Our ideas about light may be *incommensurable*. This applies to disciplines beyond physics and is often a problem between disciplines too. For instance, if I am not careful to define *diffraction* for both fields, there may be some incommensurability around my operational use of diffraction in this dissertation.

There are connections, too, between Latour, Connolly, and Habermas. Much like Connolly, Habermas's *communicative rationality* begins reflexively, with a critique of the philosophical project (and specifically, Immanuel Kant's philosophical project) as "setting up a domain between itself and the sciences, arrogating authority to itself":

> It wants to clarify the foundations of the sciences once and for all, defining the limits of what can and cannot be experienced, This is tantamount to an act of showing the sciences their proper place. I think philosophy cannot and should not try to play the role of usher (Habermas 2; 1981).

This attitude — that, to begin to make an intervention, the humanists (philosophers, STS-ers, both, or neither) must begin reflexively and collaboratively — is compatible with both Latour and Connolly. In order to make an intervention into one's own community as well as another, one must take the role of reflexive critic. Habermas scholar Thomas McCarthy describes this as "a reciprocal vulnerability that calls for guarantees of mutual consideration to preserve both the integrity of individuals and the web of interpersonal relations in which they form and maintain their identities" (x; 1981). This is similar to Donna Haraway's *modest witness*. "The modest witness," writes Haraway, "is a figure in the stories of science studies as well as of science. S/he is about telling the truth, giving reliable testimony, guaranteeing important things, providing good enough grounding — while eschewing the addictive narcotic of transcendental foundations — to enable compelling belief and collective action" (n.a.; 1997).

Furthemore, the modest witness "works to *refigure* the subjects, objects, and communicative commerce of technoscience into different kinds of knots" (ibid). Plainly, the modest witness is situated in both science and the critical study of it; does attempt to give some best account, but not in a way that "arrogates authority," in the words of McCarthy; and reconfigures the boundaries between the researcher and the research, and the ways in which that research is communicated.

But I digress. I have been harsh on STS and the humanities, while simultaneously invoking their theories. I should be clear, then, that I see STS (and the humanities more broadly) as bringing important, *nonscientific* talents to the table, that those in technoscience ought to listen to. We offer expertise in dialogue, to begin to deconstruct and reconstruct those paradigms of which we are critical. Habermas argues that clear, clarified communication is at the core of the humanist project. And furthermore, he theorizes that communication is dialogic. That is, it is based in a back-and-forth exchange between two or more communities with differing ideological lenses.

Not only does this dialogue facilitate understanding between communities but, according to philosopher Rick Roderick, this sort of dialogic exchange allows us to "[…] become ourselves in our interactions with ourselves" (Roderick 1993). This implies that we begin with a kind of communication that is less true to our identity. This implication is intentional: Habermas posits that all communication is distorted and, indeed, through continual dialogic exchange, we move toward a kind of clarity and, therefore, *rationality*. This is at the heart of Habermas's theories of communicative rationality: communication becomes distorted through the lens of different beliefs, values, cultures, and power structures. We can never fully liberate ourselves from these distortions, but we can shed light on them through continued, measured, reflexive dialogue (ibid).

Postphenomenology and Multistabilities; Phenomenology and Originary Givenness

Finally, the subfields of phenomenology and postphenomenology have been hugely influential to my theoretical framework. If phenomenology is the philosophy of our experience of life, postphenomenology asks us to consider the ways in which our experience of life is always mediated:[3] by our eyes and/or ears, or our own corporeal sense more broadly; by the microscopes, telescopes, and other lenses through which we study the world; by digital imaging technologies such as MRIs or earth remote sensing satellites. Digital imagery, specifically, is a topic most favored by the field of postphenomenology.

"A pragmatic wedded phenomenology," writes postphenomenologist (and founder of the postphenomenological field) Don Ihde, "is also in a deep sense empirical":

> That is, it is a phenomenology which deals not so much with academic disputes and a literary-critical style of work, as with the examination of 'the things themselves.' Academic philosophers rarely do this, although I detect a contemporary trend which does point in such directions. Interestingly, it is a trajectory more often taken by those engaged with philosophy of technology than other subfields (8; 2003).

Relevant context behind Ihde's argument, here, is that he talks of technology as an extension of self; as an embodiment. The facet that most "plays a very distinctive role in postphenomenology as I am defining it," argues Ihde in another volume, is "[…] the sophisticated theory of perception and embodiment which post-Husserlian[4] philosophy developed" (3; 1993). Therein lies a connection between us and our technologies — "the

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[3] Philosopher of Technology Dylan Wittkower explained the relationship between phenomenology and postphenomenology like this to me at a conference one time, when I was newer to the field. I love this definition, and often use it to explain the relationship to friends whose backgrounds are not in philosophy.

[4] Husserl is a well-known and influential 19th-Century phenomenologist.

things themselves[5]," so to speak. Technologies extend our senses. They tell us something about our own experiences: our beliefs, values, and so on. To study a *thing itself*, an extension of *us*, is to glean some pearl of wisdom about *us* (or at least to try). This philosophy guides my research for this dissertation — particularly for Chapter 2, in which I provide an extensive history of Landsat's instrumentation. Like postphenomenologists (and some phenomenologists too, probably), I see Landsat's instrumentation as an extension of our values and, more specifically, of certain kinds of institutional and political values. By doing this, I am also acknowledging the hybrid nature of Landsat: it is at once political, instrumental, technoscientific, institutional, local, global, etc.

Another important idea from postphenomenology, which I do not add to or amend in any theoretical sense, but which I take up *pragmatically*, is that of *multistability*. Multistability, a term coined by the aforementioned Ihde, is the idea that Landsat, as a technology, for example, does not have one correct interpretation or use. Rather, the Landsat "hold[s] different meanings for different users" (Rosenberger 37; 2014). "The term is useful," says Rosenberger,

> because it emphasizes two things at once: (1) the 'multi' part of the term reflects the idea that technologies are always capable of fitting into multiple use-contexts; (2) the 'stability' part of the term reflects the simultaneous conception of technology as something always limited in its potential meanings and uses by the concrete materiality of the user and the device — that is, a technology is limited to a restricted number of concrete 'stabilities.' On this view, a technology can be used for many different things, can mean many things to many users, and can be made to fit into many contexts, but at the same time it cannot do simply anything, cannot mean simply anything, and cannot be made to fit into just any context (ibid).

[5] This is actually a reference to a Husserlian phrase: "Let's go back to the things themselves," or *Wir wollen auf den Sachen selbst zuruckgehen* (168; 2001).

Here, Rosenberger argues that the term *multistability* importantly reflects a finite number of usages for a given technology, while simultaneously emphasizing that technologies have multiple, and not singular, usages. This relates directly to Landsat's instrumentation: it can't do just anything, or be used or accessed by just anyone. It performs certain tasks, and can only be understood and manipulated (in terms of instrumentation and data) by those who have a certain amount of expertise. However, there are distinct kinds of communities who developed the program — and who use the data. National security, identification of oil and minerals, soil and vegetation health, and climatology — to name just a few — are examples of Landsat's multiple uses and, therefore, multiple stabilities.

Peter-Paul Verbeek, also a well-known postphenomenologist, writes that "[t]he multistability of artifacts implies not only that artifacts can have different meanings in different contexts, but also that specific goals can be technologically realized in different ways by a range of artifacts" (136; 2005). In *What is Multistability? A Theory of the Keystone Concept of Postphenomenological Research*, Kyle Powys White names the thing that allows multistabilities to vary across artifacts — yet still connect — the *pivot point.* "By pivot," he writes, "I mean *that* which allows the variation to make sense as a variation" (75; 2015). Along those lines, in *Digital Images and Multistability in Design Practice*, Fernando Secomandi argues that "digital images, in addition to harboring divergent interpretations on the part of designers, can ultimately converge on the final designed object" (141; 2017). This brief literature review only scratches the surface of important, related writings in postphenomenology. The following references are here if you'd like to read further on the topic: Wellner 2015, Tripathi 2018, Hasse 2015, and even Borgmann 2006.

But I digress. The notion of the *pivot* across stabilities — or convergence of diverse or divergent stabilities — is important to this work. In Chapters 1 and 2, I will highlight the ways in which Landsat is not only multistable, but also the ways in which these stabilities are deeply entangled with each other. For instance, Landsat has extractive, preservative, and military stabilities. However, the histories of these various uses — and the values from which they come — are intertwined. Therefore, it is to open up one stability without opening others. Additionally, each stability — extractive, preservative, and military — tends to have different ideas about what a societal good might look like, and thusly how that good might be achieved through action. Not only is it difficult, then, to naturally achieve some singular, unified kind of good with Landsat data, but it is also difficult to separate action toward ecological preservation, for instance, with action toward extraction. As these stabilities are multiple *and* entangled, action toward some societal good using Landsat data isn't easy *or* natural. We must be specific about the processes between data, action, and values. Being specific in this way — and opening up this space to acknowledge these entangled stabilities that share a strong pivot point — will involve figuring out theoretically/practically how to engage one stability without the others.

Virtues and values are not my area of expertise. However, phenomenology and postphenomenology find connections toward value-based discussion through Virtue Ethicist Shannon Vallor. In her article "Beyond Originary Givenness?

Postphenomenology, Digital Imaging, and Evidentiary Responsibility," Vallor further interrogates the notion of multiple stabilities. More specifically, she revisits a Husserlian phenomenological idea: that there may be one *most correct* (or most ethical) interpretation of a given perception, image, or technology: evidentiary responsibility within *originary givenness*. Originary givenness is the notion that, even if I observe a situation, my observation may be in some way distorted or flawed. "Imagine, for example," writes Vallor, "a person who is too distracted by a TV crime show to recognize that her injured dog at the door is not just 'making a bunch of noise,' as she believes, but is actually crying in great pain" (22; 2015). In this case, there could be multiple interpretations of the dog's cries. However, the ethically salient interpretation is that the dog is in pain. In a case like this, there is a kind of ethical reading, and a subsequent normative stance. In other words, our best interpretation of a technosocial circumstance — perhaps the data produced by Landsat — may compel us to interpret it in a certain way, and to act accordingly. Vallor makes this point clear:

> [T]he kind of normatively implied in the phenomenological motto is not, at least immediately, of a political kind. For the normative implication of [the things themselves] suggests a different sense of responsibility, one not to human subjects but to things. It is the responsibility to confront them as given, to meet the phenomena exactly where and how they are, rather than where and how my theoretical constructs and schemas expect them to be in advance (20; ibid).

She points out here that, if the focus is on "the things themselves," then we have a certain commitment to recognize the most ethical possible interpretation of those things. Perhaps, to recognize that there are multiple possible interpretations is not enough: perhaps it is also our job as scholars to work toward an interpretation that is equitable.

From Vallor's article, it is clear that there is no set code for a most ethical or a most equitable interpretation in a given situation. Rather, the charge is to wrestle with the notion that this is something to strive for. Vallor's 2016 book *Technology and the Virtues* offers a more in-depth description about the kind of intuitive orientation one must have to making ethical decisions with respect to technology, and to interpreting technological quandaries in equitable ways. Although virtue ethics are not fully within the purview of this dissertation, I will briefly explore Vallor's particular brand of virtue ethics in Chapter 3.

The Military-Industrial Complex and Interdisciplinarity

My project is also situated among literatures on the military-industrial complex in technoscience. In Chapter 2, I will invoke Edmund Russell and Lily Kay, who wrote about the ways in which pesticides and genetic science respectively were deeply interconnected with military research (Russell 2001; Kay 2000). There is also a massive literature on nuclear research and the military-industrial complex. It is not quite within the realm of my project to engage with this research in a meaningful way. However, I would be remiss if I did not note the broader influence of MacKenzie 1993, and Gusterson 1996 and 2004. However, I do not invoke these literatures explicitly, as they are quite contextually situated within nuclear issues. Of course, there are important parallels to draw between nuclear research and Earth Remote Sensing (particularly by way of MacKenzie's work on accuracy). However, this dissertation already draws from an unusually broad swath of literature, and I want to speak more directly to issues in environmental science as I am addressing my Remote Sensing audience.

Additionally, there is a vast literature on interdisciplinarity, much of which I am not aware. Again, it is not (yet) my project to engage with interdisciplinarity from a formal pedagogical perspective. However, I hope to collaborate with scholars in this realm in the future — and I hope that they pick up the loose threads of this work.

Literature, In Summary

In summary, my dissertation sits relative to Latour's calls to reflexively refute modernism by simultaneously translating and purifying; to Connolly's critical responsiveness, and Habermas's communicative rationality, and communicative action through dialogic change; to broader theories of stability, interpretation, and lens in both phenomenology and postphenomenology, attempting to hold in mind both multiple realities, while always keeping in mind that some realities are more just and ethical. You will find more theoretical explorations in all three chapters. However, as this dissertation is written for audiences in Remote Sensing as well as STS, you'll find that the theories often watch over the project from the background. I occasionally call them forward to make a point. For the most part, though, they quietly and intentionally support the infrastructure of the project (Dr. Lee Vinsel, a professor here in the STS Department at Virginia Tech, might say that they are the invisible *Maintainers*).

Diffractive Interventions: A Roadmap of Chapters

Beyond critical theory, this dissertation is foregrounded by archival explorations of Landsat's history, as well as more grounded theoretical explorations pertinent to conservation and security. Chapter 1 begins with *the thing itself*: Landsat. A few histories of Landsat have already been written, including one about transitions in its ownership and operation, between public and private entities (Gabrynowicz 1993; Gabrynowicz 2005); one about its beginnings in the Department of the Interior, and within NASA (Mack 1990); an internal history by the American Society of Photogrammetry and Remote Sensing (Goward et. al. 2017). I offer an additional history of Landsat: the history of its multispectral scanner, because I feel it points to Landsat's military origins in ways that other histories do not. The multispectral scanner is the *pièce de résistance* of Landsat: the instrument that makes Landsat what it is, and gives it the ability to "see" in many wavelengths of light. Many other earth remote sensing satellites since Landsat have used the multispectral scanner as well. However, Landsat was the first *civilian* satellite to use this technology.

The multispectral scanner was developed at a large, Department of Defense-funded laboratory at the University of Michigan called Willow Run. Willow Run contained multiple laboratories within it, including the one that developed the multispectral scanner. The Willow Run Laboratories (WRL) were first established in 1946, and existed under the umbrella of the University of Michigan up until the early 1970s. Furthermore, the majority of the research conducted at Willow Run was classified research. This means that it was funded by the Department of Defense (DoD), and that researchers at WRL often could not openly publish on their findings. Furthermore, much of the research conducted at Willow Run was known as *countermeasures* research. Countermeasures, in the military context, involves a type of defensive surveillance system, usually airborne. This technology will watch an enemy force from above, zero in on their location, and subsequently deploy weaponry. I will delve into a history of Willow Run research activities, and will show you how guided missiles for *countermeasures* ultimately birthed the multispectral scanner.

This classified, countermeasures research had a couple of important implications for the University of Michigan: with respect to university governance, activism, and the place of classified, defense-funded research at the public research university. As a result of debates surrounding these issues, Willow Run closed, and re-established itself as the Environmental Research Institute at Michigan, or ERIM, a non-profit organization that did research. ERIM held annual symposia on earth remote sensing, and internal histories of Landsat often credit these symposia as crucial to the development of the Landsat satellite program. I do not deny their importance. However, because the remote sensing community is already aware of the symposia, I do not focus on them in my retelling of this history.

Because there are no archival documents that contain in-depth discussions about the renaming, we can only conjecture is no coincidence that the laboratory was renamed as *Environmental* non-profit organization: indeed, environmental conjures certain kinds of political values that might please scholars and activists opposed to countermeasures research, and would be a very strategic move. No doubt, associations with an *Environmental Research Institute* have, over time, done work to help the general public

— along with the remote sensing community itself — associate Landsat with environmental matters.

Chapter 2 contextualizes the entanglement of these military and environmental connections. In this chapter, I explore the interrelatedness of national security, resource preservation, and resource extraction. Landsat has ties to all of these narratives, and its users (and early promoters and adopters) hail from all of them. However, history of entanglements often becomes buried in rhetoric about action toward societal good. In order to have a conversation about how data becomes action, and in turn how action becomes societal good, we must reckon with the conflicting narratives that shape our conceptions (or lack thereof) of what constitutes action. Furthermore, we must acknowledge that, as military, preservation, and extraction are entangled by way of Landsat (and by way of earth Remote Sensing in general), so too are their notions of societal good.

Consider a 2016 report from the Union of Concerned Scientists, who argue that "[b]urning coal, oil, and natural gas has serious and long-standing negative impacts on public health, local communities and ecosystems, and the global climate. [...] We don't pay for the cost of cancer, or the loss of fragile wetlands, when we pay our electricity bill — but the costs are real" (n.a. 2016). This framing positions extractive and environmental perspectives as incompatible. And, indeed, I would stand by the thesis that extraction of oil, natural gas, coal, and certain kinds of minerals is not only destroying the environment, but individual lives near sites of extraction (see Wylie et. al. in Chapter 3). However, the relationship between preservation and extraction is not as opposed as we often believe — indeed, they are both tied up in the larger field of *conservation*. These entanglements are bigger than Landsat, and bigger than earth Remote Sensing more generally. Rather, they date back to the Theodore Roosevelt administration: to efforts to bring irrigation to the arid Western United States.

We observe similar connections between environmentalism and national security: connections that are just as conceptual as they are historical. Environmental research has often, for instance, been framed as a call to identify a threat to our safety, and to collect information that will teach us how to work against this threat. This type of framing finds its roots in the history of the concept of *national security*, and the ways in which we use this concept to denote threats to our country, our sense of nationalism and, indeed, our bodily well-being. Connections between environmental and military thinking, too, are bigger than the Landsat satellite system. However, Landsat provides a great case study for the ways in which these connections manifest within a large-scale technological system.

Chapter 3 intervenes in the critique-to-action paradigm in STS. The best way to intervene in this paradigm, I believe, is to engage with scholarship in a subfield called *Engaged STS*. Engaged STS is a kind of public philosophy for the field of Science and Technology Studies. Along with the *making and doing* movement pioneered by Gary Downey and Teun Zuiderent-Jerak, Engaged STS offers frameworks that facilitate STS interventions, and more deliberate STS engagement with technoscientific communities. I explore these theories as outlined by Downey and Zuiderent-Jerak, and then I put them into conversation with theories from other scholars that I believe we ought to talk about when we invoke the name of Engaged STS. These scholars include: feminist epistemologists Donna Haraway and Karen Barad, along with virtue ethicist and philosopher of technology Shannon Vallor.

With those frameworks in mind, I offer a couple of examples of how I think we can *diffractively* intervene in the data-to-action and critique-to-action paradigms in Remote Sensing and STS respectively. Specifically, I argue that action-oriented thinking should be built into research projects in Remote Sensing, just as it should be built into critique in STS. I cannot offer a complete map for how to accomplish this. As a modest witness, I offer beginnings rather than endings.

Before We Begin: Why *Landsat in Contexts?*

The title of my dissertation is a nod to an article by Kass Green, entitled *Landsat in Context: The Land Remote Sensing Business Model*. Green, who is brilliant and fascinating, writes a history of Landsat as it relates to public and private funding, and as it relates to institutional affiliations. The article was one of many inspirations for this dissertation, both for what it contains, what what I wish to add to its canon. For instance, Green makes mention of the military history of Landsat, but doesn't delve into it. So my title aims to say: that was one context. I'm providing further contexts. There are multiple contexts here. The title is a *Yes, and*.

Chapter 1:
Landsat in the Context(s) of Willow Run, ERIM, and the Public University

In the introduction to this dissertation, I invoked a quotation by philospher and phenomenologist Edmund Husserl: translated from German, "Let's go back to the things themselves." By this, Husserl invokes a central tenet of phenomenology: to describe *things as they appear* (Kant 1781).[6] My intention is to go back to Landsat itself: to its instrumentation, as it was developed in a university laboratory; to the university, state, and national politics surrounding that laboratory; to the laboratory's reestablishment. I will do my best, as I put on my historian hat, to describe things as they appeared (as well as the things themselves).

However, *going back to the things themselves* has an additional meaning within the context of this dissertation. Philosophers of technology often believe that *the things* — namely, instrumentation — extend our senses and, along with them, what we know, and how we know it. In *Thing Knowledge — Function and Truth*, Philosopher of Technology Davis Baird calls this *thing knowledge*. Thing knowledge, he writes,

> Is an epistemology where the things we make bear our knowledge of the world, on a par with the words we speak. It is an epistemology opposed to the notion that the things we make are only instrumental to the articulation and justification of knowledge expressed in words or equations. Our things do this, but they do more. They bear knowledge themselves, and frequently enough the words we speak serve instrumentally in the articulation and justification of knowledge borne by things (13; 2002).

[6] It's worth noting that Kant believed that *things as they appear* and *things themselves* were distinct from each other. However, phenomenologists blurred this boundary.

Landsat itself bears knowledge in the form of data, in addition to the analyses, interpretations, and uses of its data (of course, these bear knowledge to). Its particular instrumentation — moderate resolution, with an array of spectral bands intended to help us understand a variety of earthly dynamics — has a certain orientation to knowledge-creation. If Landsat's instruments create data/knowledge, and if we hope that this knowledge will result in action toward societal good, we must ask: where do those instruments come from? Under what circumstances were they developed, and for what purposes? We must understand the context of this *thing knowledge* before we can fully interrogate how it might lead to action.

Landsat's Enduring Legacy: Pioneering Global Land Observations from Space, Landsat's internal history written by those who have worked on and with the satellite system throughout their careers, pinpoints the origins of Landsat's instrumentation at the University of Michigan:

> By 1963, one of the most significant optical sensor innovations developed at U. Michigan was the original aircraft optical-mechanical multispectral scanner. This aircraft system served as inspiration for both the multispectral scanner system (MSS) flown on Landsats 1-5 and the various thematic mapper sensors aboard Landsats 4-7. [...] Research managers in military labs became keenly aware that developing remote sensing technologies for Earth-orbiting satellites might have more important contributions to civilian science and applications than within the military domain. With the support of the [U.S.] military and the National Research Council (NRC), workshops and conferences were organized to advance the nonmilitary use of remote sensing (Goward et. al. 9-10; 2017).

Goward et. al. attribute Landsat's multispectral scanner design to that of the multispectral scanner developed at University of Michigan. The original multispectral scanner was developed for military surveillance, and Goward et. al. specifically point to defense agencies like the Office of Naval Research, or the ONR (9; ibid). However, the military and the National Research Council (NRC) put forward money for workshops and symposia — workshops and symposia through which this technology could be repurposed for civilian uses. These workshops and symposia "were referred to as the ERIM symposia" (11; ibid). *ERIM* stands for the Environmental Research Institute at Michigan. A couple of previously-mentioned sources credit the initial idea for Landsat to these ERIM symposia (Lauer et. al. 832; 1997; Mack 37; 1990).

Goward et. al. mention ERIM alongside the Willow Run Laboratories at the University of Michigan:

> During World War II[,] the Willow Run Airfield east of Ypsilanti, Michigan was a major location for the production of combat aircraft. Post-WW II [sic], the University of Michigan Institute for Science and Technology, with its Infrared Research Laboratory, became the Willow Run Laboratories in 1946 when they took over the Willow Run facilities. Guided by the University, Willow Run Labs became a center of remote sensing technology development primarily funded by defense agencies [...]. In 1972, the University cut formal ties with Willow Run, which was then established as the private not-for-profit Environmental Research Institute of Michigan (ERIM). (9; ibid).

In other words, Willow Run Laboratories — where important strides in Remote Sensing research were made and where the multispectral scanner was potentially developed — eventually became the Environmental Research Institute of Michigan (ERIM). The modern-day Landsat community recognizes that important Landsat-originating symposia were held at ERIM. But how and why did Willow Run Laboratories become ERIM? What, specifically, was the nature of the research programs at these laboratories?

To answer these questions, I conducted extensive archival research at the Bentley Historical Library in March of 2018, in an attempt to better clarify these connections. In the following pages, I'll provide a brief history of the multispectral scanner at Willow Run Laboratories, within the context of the institution and broader political events. Then, I'll provide some context for WRL's transition to ERIM. Specifically, I'll talk about a controversy that happened at University of Michigan's campus in the late 1960s and early 1970s — one that left WRL with no other option but to re-establish itself as an institution separate from the University. This will involve a brief dive into university governance, which shaped this debate, and its relationship with broader political issues and policymakers. Finally, I'll discuss the famous symposia at ERIM, and their implications for the modern-day Landsat community. Ultimately, I will tease out two themes within this chapter: (1) that the distinction between civilian and military Remote Sensing is blurry and, therefore (2) action toward a militarily-inclined societal good, and action toward an ecologically inclined societal good, are blurred as well. Ultimately, I hope that recognition of the blurriness between military and civilian remote sensing will lead us to more effective, more thoughtful dialogue about Landsat and other earth remote sensing satellites.

A History of the Environmental Research Institute of Michigan (ERIM), Willow Run Laboratories (WRL), and the Michigan Aeronautical Research Center (MARC)

In the introduction to this chapter, I spoke of how various Landsat publications have recognized the Environmental Research Institute at Michigan (ERIM) and/or the Willow Run Laboratories (WRL) as places where Landsat originated. Willow Run, too, recognizes Landsat as a project that it birthed. "I feel that we need to freely demonstrate our pride in ERIM's history," writes William G. Dow. Dow was a prominent electrical engineer at University of Michigan: a researcher at Willow Run laboratories, and an emeritus trustee at ERIM.[7]

> For example, Landsat and other earth observing satellites can trace their origins to WRL because their multispectral scanning ‘eyes’ were the direct result of the work of Willow Run researchers Michael Holter, Gwynn Suits, Donald Lowe, and Richard Legault. [...] But is the world aware of how far-reaching the results have been? We must, therefore, be quietly proud of these and other achievements and always be ready to provide the needed information. This leads me to state my feeling that our name, Environmental Research Institute of Michigan, serves us very well indeed, for it implies that our charter for public service permits and encourages us to carry on research in relation to the entire human environment. This includes attention to the national defense to ensure a peaceful environment for freedom of human endeavor, as well as research involving physical, biological, geophysical, solar system, and galactic phenomena (13-14; 1994).

Dow directly connects Landsat — and other earth-observing satellites — to the multi-spectral scanner (or Landsat’s “eyes,”[8] as Dow calls them) developed at Willow Run. Dow then continues on to say that the connection between the military research laboratory and “far-reaching [...] results” like Landsat is evidence of a “charter for public service.” Furthermore, he argues that this charter includes “attention to national defense.”

The following section will explore a brief history of Willow Run Laboratories and the Environmental Research Institute of Michigan. I will heavily invoke these internalist histories of ERIM and WRL as archival materials — written by both Dow and Irvin Sattinger, another WRL researcher — along with supporting sources: both secondary and archival. Both are no longer living. However, their voices are very much alive in this

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[7] Dow lived to be 104 years old. His obituary reads: “At his 100th birthday, Dow described himself as ‘a first-class engineer in the field of physical electronics, a pretty good teacher, and a damn find promoter.’ His longevity came from exercise and ‘choosing the right grandparents,’ he said. ‘When I was 85, I lost one of the major arteries in my heart. But other arteries will do the job — if you ask them everyday’” (Michigan News 2007).

[8] Steve Koppman, writer at the Michigan Daily, would report that the University of Michigan has earned itself the title “Eyes of the Army.” “[This is] a title conferred by the Department of Defense (DOD) for the large amount of research done here by faculty members on contracts with the DOD in the fields of remote sensing, surveillance, and countermeasures,” reports Koppman (1971, September 9. *Classified research debate: Soul-searching for the ‘U’).*

internalist history; it reads like a personal interview. I was lucky to have found it at the Bentley Historical Archives at the University of Michigan.

"The story begins with a conversation I had sometime either in December 1945 or early 1946" (1; 1994), writes Dow. Dow and Emerson W. Conlon (or "Connie") had both returned from "war-related research":

> Conlon was a broadly based aeronautical engineer with special expertise in the design of aircraft structures. At the time, he had major responsibilities in the U.S. Navy's Bureau of Naval Aeronautics, located in or near Washington D.C. Whether Conlon served as an officer in the Navy or as a civilian, I cannot recollect. I had spent two and a half years at Harvard University's Radio Research Laboratory in Cambridge, Massachusetts. At Harvard I was primarily responsible for guiding a group of some 35 to 50 staff members — engineers, physical scientists, and support personnel — engaged in research involving the jamming of enemy radar. [...] I, therefore, had become familiar with the then very new microwave arts (ibid).

To summarize: Conlon's work was in aerospace engineering, and Dow's was in remote sensing with microwaves. Conlon did research for the Navy, and Dow completed Department of Defense-funded research at Harvard University. Both men returned to the University of Michigan after the Second World War, and worked together to reimagine what postwar — and, ultimately, what Cold War — research might look like. Both were aware of this this postwar research was a lucrative pursuit; one that the DoD was willing to fund. And finally, both men knew that this research would likely be cutting edge. "As a result of our within-the-system wartime experiences, Conlon and I were both vividly aware of the oncoming Cold War and the threats it carried," writes Dow.

> We were equally aware that Uncle Sam's [DoD] fully realized the urgent need to keep within the front rank of applied science, lest we might indeed lose a next war. We also knew that the upper echelons of government had a high regard for the research capabilities of our universities. This meant that the DoD would be likely to finance military-oriented research at the [University of Michigan] fairly handsomely. This included funding to obtain both the needed 'high technology' [...] equipment and the resources for employing the full-time staff and graduate students required to provide scientific leadership (1-2; ibid.).

Dow and Conlon put their heads together and came up with a “large-scale research project to design and prove the feasibility of what would today be called an antiballistic missile [...] system — a term that had not yet been invented” (2; ibid). Dow said the plan was to “design and build guided missiles that could intercept and destroy attacking enemy space vehicles [...]” (ibid). The closest technology to this sort of guided missile, at the time, was the German V-2 rocket. BBC reporter Richard Hollingham says the V-2 was “a new type of weapon, crashing and exploding without warning in target cities [...]” (2014). Hollingham reports that there were only five short minutes between the time a V-2 rocket was launched, and the time it made impact on a target city. “V,” says Hollingham, “stood for *vergeltungswaffen*,’ or ‘retaliatory weapon,’” and these retaliatory weapons were indeed a powerful and last-minute attempt to turn the war in the Germans’ favor (ibid).

Dow pitched his idea, to construct a V-2 at the University of Michigan, to a willing Air Force. With this Air Force contract, he and John Strand, M.S.E. — an engineer with expertise in radar from Bell Telephone Laboratories — embarked upon the WIZARD contract. “[I]t was the WIZARD contract that gave birth to [ERIM’s] first predecessor organization, [the Michigan Aeronautical Research Center], which was immediately established at what became the Willow Run Airport” (Dow 3; 1994).

WIZARD, also referred to as Project Wizard, “[studied] the feasibility of a supersonic missile capable of reaching 500,000 feet,” and was a “[...] sophisticated weapon, [...] intended for use against a 4,000 mph target at an altitude anywhere between

60,000 and 500,000 feet" (Leonard 106; 1956). In short, WIZARD was an extremely powerful piece of weaponry. It's also worth noting that the University of Michigan received $1,000,000 per year to conduct research for WIZARD. A well-funded and cutting edge project, WIZARD ultimately won out over a parallel project called Thumper. Thumper was a nearly-identical project, also a supersonic missile intended to target and and hit other missiles in flight. It was also funded by the U.S. Air Force, but developed at General Electric (Leonard 105-113; ibid).

Dow notes that it was convenient to establish WIZARD at the Willow Run Airport. The University of Michigan's campus was already strapped for laboratory space and, because of the limits and specifications of the contract, the project had to begin immediately. So, what was available? "[An] airfield and the large hangars[9] used in flight-testing the World War II B-24 bombers built at the Willow Run Bomber Plant by the Ford Motor Company, which was no longer needed by the Department of Defense" (5; ibid). The Willow Run Airport was located in Ypsilanti, Michigan. The distance between Ypsilanti and Ann Arbor, where University of Michigan is located, is similar to the distance between Blacksburg, VA and Christiansburg, VA (for reference). The Willow Run Bomber Plant became the Willow Run Airport when the University leased part of the space to commercial airlines (ibid).

[9] Dow digresses into a fun story about this purchase: "Robert P. Briggs, the forward looking and agressive UM vice president for finance," Dow elaborates, "[...] consummated a multifaceted deal whereby the university, for a payment of one dollar, purchased the Willow Run airfield, hangars, and contiguous land and buildings from the DoD" (5; 1994). Ten years later, UM's president, Alexander Ruthven, noted that UM never completed the one dollar transaction. "Ruthven had obtained a silver dollar to make a ceremonial payment but had failed to find anyone [at the DoD] who would accept the payment, even after several years of trying" (ibid). Dow notes that Ruthven finally commemorated the missed payment by donating a silver dollar to each of his two grandchildren.

In 1950, at the dawn of the Korean War, the Air Force engaged MARC in another opportunity. This project was a collaboration with Boeing — and co-funded by the Army and the Air Force — to develop more powerful supersonic missile that could intercept and explode bomber aircraft (15; ibid).[10] Within this collaboration, Boeing acted as the primary contractor, as well as the primary interlocutor for designing and building the hardware. MARC, on the other hand, worked on the technologies that controlled the missiles. This included "ground-based radar, tracking, and [a] command guidance system" (ibid 17). "MARC's contribution to the BOMARC system was to define and design the ground-based tracking and guidance system, construct a prototype system, and test the system at Patrick Air Force Base in Florida," says Dow (ibid 18). Note that this control system made it possible for the missiles to fly *unmanned*. Supersonic missiles like BOMARC are called *ground-to-air pilotless aircraft*: they are the earliest versions of UAVs, or *unmanned aerial vehicles*. And much like the UAVs we know today, BOMARC was guided to its targets by way of a radar system (ibid 18).

In the same year, MARC changed its name. Dow felt that the name change was necessary, as these projects demanded a broader, more fitting descriptor than *aeronautics*. These new projects were not simple aircraft: they were autonomous, radar-guided weaponry. Therefore, shortly after embarking on BOMARC, the Michigan Aeronautical Research Center became Willow Run Laboratories (ibid 21). Following that, in 1953, Willow Run embarked upon one of its largest projects: Project MICHIGAN. MICHIGAN

[10] Note also that, when I say *intercept*, I say it with awareness of the term's murky meaning. Pinch and Collins deconstruct this in their book *The Golem at Large: What You Should Know About Technology.* "Interception," they write, "says nothing about destruction, damage, or diversion" (23; 1993).

was "a large-scale tri-service program in battlefield surveillance" (ibid 31). In other words, MICHIGAN's goal was to develop a digital surveillance system that could watch a battlefield at all times, and in all kinds of weather. Dow writes that

> [s]pecific Project MICHIGAN tasks were concerned with the fundamental development of high-resolution radar using optical processing of radar signals, moving target indication [...] radar, airborne infrared and multispectral scanners, acoustic and seismic systems for locating enemy activities, systems for locating sources of electromagnetic radiation, night-vision optical systems, [...] infrared detectors, navigation and guidance, and information and data processing and display for such purposes as system simulation and design, sensor interpretation, and operational automation (ibid 36).

MICHIGAN operated by "picking up" signals from different target materials, as they're often called in Remote Sensing. These materials could be trees, people, tanks, cars, etc., and would help researchers learn about the structure and makeup of a given environment, like a battlefield. The sensors that picked up these signals would be powerful and accurate enough that conditions like weather and time of day would not interfere. These night-vision, seismic, infrared, radar, navigational, and acoustic sensors were early examples of *passive* remote sensing. *Passive* remote sensing, as opposed to *active* remote sensing, is when a sensor collects and interprets signals from a target material, as opposed to emitting those signals itself. It is worth noting that the Landsat satellites, too, are examples of *passive* remote sensing.

Dow notes that MICHIGAN was as much about software and data interpretation as it was about hardware, much like the earth Remote Sensing we see today. For these earlier technologies, this meant "assessing the effectiveness of the complete battlefield surveillance system that incorporated new sensor and data processing equipment" (ibid). And, perhaps most paramount, MICHIGAN was not only about surveillance. Unlike its predecessors, MICHIGAN was a form of *target acquisition.* Dow defines this "as the part of combat intelligence that involves ‘the detection, identification, and location of a target in sufficient detail to permit the effective employment of weapons” (ibid). This means that, while WIZARD, Thumper, and BOMARC *were* autonomously-guided missiles that surveilled and launched at enemy weaponry or aircraft respectively, MICHIGAN surveilled battlefields and autonomously launched the missiles itself. MICHIGAN was about automating warfighting processes in one device: from surveillance, to battlefield interpretation and, finally, to release of weaponry.

Dow credits MICHIGAN for both the financial and reputational success of Willow Run and ERIM (ibid 37). By the end of the 1950s, MICHIGAN “was operating at full throttle and providing about three-quarters of the total funding of Willow Run Laboratories” (ibid 47). Dow is cryptic about institutional relations around this time. However, he does make mention of two institutional shifts that coincide with Project MICHIGAN’s success. The first shift occurred in 1955, when the UM Board of Regents “amended the [UM] bylaws to eliminate all reference to the Willow Run Research Center, thus officially making its activities an integral part of the Engineering Research Institute” (ibid). The second shift happened in 1959, when the Board of Regents established Willow Run Laboratories as its own research unit, outside of the Engineering Research Institute. The following year, Willow Run joined a newly-established group of research laboratories at UM known as the Institute of Science and Technology, or IST (ibid 50). Of this change, Dow notes that “there was little enthusiasm for burying the well-known Willow Run Laboratories name, and it continued to be used prominently on reports, along with that of IST” (51). And although those at Willow Run cooperated within the larger institutional structure of IST, remarks Dow, "WRL maintained a separate and distinct identity within the scientific research community" (ibid).

Dow does not go into great detail about why these changes were made by the Board of Regents. Additional archival materials offer little in the way of explanation as well. However, I would conjecture that the University of Michigan was strategizing: trying to find ways to keep the growing success of Willow Run Laboratories within its purview, without appearing to have a close connection with its warfighting research. From an administrative perspective, this positioning makes sense. On one hand, the school wants to keep the success of Willow Run's projects within its grasp, but to maintain its reputation as a liberal campus. But I digress.

I've talked somewhat extensively about WIZARD, BOMARC, and Project MICHIGAN because (1) they are relevant to the *things-as-they-appear* history of Willow Run Laboratories. However, (2) they are also directly related to the development of the spectral scanner and, hence, (3) to Landsat itself. Indeed, they are the multispectral scanner's lineage and, therefore, the technoscientific lineage of Landsat itself. Direct development of the multispectral scanner began at Willow Run Laboratories in the early 1960s, following the three aforementioned projects. "The scanner concept grew out of a search for practical methods of merging images from various sensors (cameras, infrared scanners, radar, and so forth) to provide a combined image that could be analyzed to identify natural and human-made materials" (ibid 61), writes Irvin Sattinger, another Willow Run researcher and author of the WRL and ERIM internalist history found at the Bentley Historical Library. MICHIGAN was already using a combination of scanners. In this way, the multispectral scanner and Michigan are quite alike. The difference between them is that WRL's official multispectral scanner is a single instrument that measures a combination of images from different *spectral signatures*, rather than a conglomeration of instruments on a single piece of machinery.

I will explain the concept of *spectral signatures* for those of you who are not familiar with the electromagnetic spectrum (likely some — but not all — in the STS community). If you are in the Remote Sensing community, please feel free to skip this section. The electromagnetic spectrum encompasses every kind of light there is — from light that's invisible to our eyes, to light that is visible, and back to light that's invisible again. Those of you who are familiar with quantum mechanics know that light acts as both a particle and a wave. However, electromagnetic theory is based within the paradigm that light is a wave. Despite the potential for incommensurability, Remote Sensing draws from both paradigms to help us understand how light — and therefore how spectral signals — work. The light on the right-most side of the spectrum is invisible to our eyes because its *wavelengths* are too long for us to see (this includes some infrared light, as well as microwaves and radio waves). The light on the left-most side of the spectrum is invisible to our eyes because its wavelengths are too small for us to see (this includes ultraviolet [UV] rays, gamma rays, and X rays). Our eyes can only take in wavelengths at the very middle of the spectrum. However, the *multispectral scanner* allows us to see more (but not all) of these types of light. Multispectral scanners are built when scientists and engineers choose *spectral bands* — or particular points on the electromagnetic spectrum whose wavelengths they'd like to "pick up" — for the scanner to measure. The spectrum is, of course, a continuum. Therefore, not all multispectral scanners will measure wavelengths at the exact same places on the spectrum. For instance, each reincarnation of the Landsat satellite system measures a slightly different set of spectral bands.

Furthermore, the multispectral scanner allows us to choose multiple different spectral bands at which we'd like to view an image. This allows us to *extend our senses*; to observe what we cannot see with our eyes. Multispectral scanners do allow us to view *true color approximations*, using a combination of spectral bands that produce an image roughly like one we could observe direction. They also allow us to see *false color composite* images, which are similar to infrared photography and photographic negatives. However, the combinations of spectral signatures through which we can view an image are potentially endless. And each combination of spectral bands that produce an image allow us to "[make] some things visible while hiding other things" (Downey 5; 1998).

Dow explains that the combination of imagery at different spectral signatures was a large obstacle to surmount. This was because computers in the early 1960s were not as powerful as they are today, and they "did not have the speed to perform the massive computations needed to register pixels" (ibid), pixels being the base unit of any discrete, digital image. WRL's solution to slow processor speed was a weaving-together of various already-existent scanners from Project MICHIGAN. Additionally, WRL managed to borrow some already-built infrared scanners from the U.S. Army (ibid).

> In 1965 two of the scanners were used to build two separate systems, each with a different set of spectral bands, for a total of eighteen bands ranging from the ultraviolet through the thermal infrared. The third scanner provided a source of spare parts. The two scanners were mounted in two L-20 aircraft. The completed systems were then flown in a

> structured exercise over a period of time to record and analyze spectral data for various seasons and over complete diurnal cycles (ibid).

In essence, WRL built two entirely different spectral scanners out of all of the aforementioned parts. Each of the two scanners could sense a different combination of spectral bands and, therefore, each scanner produced entirely different-looking imagery. These two scanners were placed on the L-20 model aircraft. The L-20, also known as the Beaver, "is a rugged, short take-off and landing utility aircraft [...]" (Bae Systems n.d.). It is a highly customizable aircraft, and is favored by the US Air Force. Often used for search and rescue missions, it has other uses too: particularly, for "aerial crop-dressing over vast areas" (ibid). The L-20 was the perfect plane to strap some scanning equipment to, and to use for quick, experimental test missions over broad swaths of land.

The initial tests went well, and the project ultimately attracted NASA's attention. NASA would use one of WRL's spectral scanners aboard its first Earth Resources Technology Satellite (ERTS-1) mission in 1972.

> Based on the preliminary assessment of the usefulness of the multispectral scanner, NASA decided to include this sensor as one of the imaging systems that would be carried aboard a satellite to demonstrate the potential value of earth observation from space. [...] In mid-1972 the first Earth Resources Technology Satellite (ERTS-1) was placed in orbit, and, in short order, images were beamed down to earth and rushed to all scientific investigators. NASA made the imagery provided by these early scanners available to many investigators, including WRL, to enable them to explore its value in applications involving environmental monitoring and earth resources management. As the practicality of earth sensing from satellites became accepted, additional earth observation satellites, now referred to as Landsats, were sent up by NASA (Sattinger 62; 1994).

Note that the ERTS-1 mission ultimately came to be known as the Landsat-1 mission. As a thank-you gift for the development of the ERTS-1/Landsat-1 instrument, NASA would provide WRL — or, by 1972, ERIM — with the early data from the first Landsat mission.

Here, I aim to be very explicit about the connections between NASA, Landsat, WRL, ERIM, and Project MICHIGAN. Oftentimes, Landsat's origins are attributed vaguely to the Environmental Research Institute at Michigan, or ERIM. Sometimes, they are even more vaguely associated with Willow Run. I've provided an alternate narrative from University of MIchigan's archival materials. Landsat's birth can be *directly* attributed to the multispectral scanner project at Willow Run Laboratories, which grew out of the DoD-funded Project MICHIGAN and some spare Air Force-owned infrared scanners. Project MICHIGAN grew out of the success of the BOMARC and WIZARD projects, both DoD-funded, and both intended for battlefield surveillance. And WIZARD grew out of a desire to have weaponry as powerful and stealthy as the German V-2 missiles. When framed this way, it's clear that Landsat has strong ties to both military and ecological pursuits. And, as *instruments* like Landsat create knowledge about our world, we ought to reflect on the kinds of institutions and values that brought that knowledge to bear — and, in turn, that the knowledge brings to bear on the world. Most of all, though, I want us to reflect on the ways in which military and ecological narratives are deeply intertwined with one another. Landsat represents one moment of this intertwining. In the following chapter, I will discuss more generally the interconnections between military and ecological narratives.

For now, however, I'll focus in on a particularly complex chapter in the histories of Willow Run and ERIM: the transition between them. Willow Run Laboratories' transition from University of Michigan, DoD-funded laboratory to independent, ecologically-branded non-profit is not only situated within the context of blurry civilian/

military boundaries in scientific research, or within the complexities of conservation narratives (although I would argue that the transition is pertinent to those narratives as well). Additionally, the transition had everything to do with debates about the meaning of university research and its role in society; with relationships between research labs and private contractors; with university distinctions between teaching and research faculty; with university politics and the *research infrastructures* (Ribes 2014; 574) that they create.

How the Willow Run Laboratories (WRL) became the Environmental Research Institute at Michigan: A Story of University Politics and Wartime Strife

Broadly, the demise of Willow Run — and its rebirth as the Environmental Research Institute at Michigan — arose out of a university-wide debate on classified research. The debate was provoked by a charge to examine the relationship between research centers at the University of Michigan, and their relationships with the broader University of Michigan academic community. Our first evidence of this type of discussion comes from University Senate minutes: from a meeting that took place in December of 1963. The Research Policy Subcommittee reports that "The committee is [...] trying to understand the effect of the large amount of sponsored research carried on by the University both on those units heavily involved and on those hardly involved at all."[11] Some early findings, it says, included the immense burden that teaching and research faculty endure when they must also be administrators of such research centers,

[11] University Senate Meeting on Monday, December 9, 1963, at 4:15pm. University of Michigan Senate Advisory Committee on University Affairs 1954-1967. Call no. Flmu B31a.2. Vol. 11, No. 1. Bentley Historical Library, Ann Arbor, MI.

and the multiple ways in which these administrative burdens take teaching and research faculty away from a desired participation in the university community.[12] Based on later meeting minutes, archival materials, and the Board of Regents' aforementioned erasure of Willow Run from the University of Michigan bylaws, I conjecture that there was anxiety, too, about the reception of DoD-funded research at the University.

Note that these discussions are situated within a specific political context. These discussions about countermeasures research and its place at the university did not take place during the 1940s or the 1950s — during the earlier days of the Cold War, or during the Korean War. Problematization of wartime research as an issue of morality was a new idea, brought about by activism during the Vietnam War (whose timeline began in the mid-1950s, and ended in the mid-1970s). It is also worth noting, then, that when the University of Michigan's Senate began contemplating the issue of morality and wartime research, they were right in the thick of the Vietnam wartime era. Bill Zimmerman of the New York Times says it best: "The Vietnam antiwar movement, famous for its sound and fury [...] [was] the first mass movement against a war in American history and one of its great moral crusades [...]" (2017). Other kinds of discussions about Willow Run Laboratories and classified research — particularly those having to do with its secrecy, and what secrecy meant for the modern university — were set against a backdrop of this particular brand of discussion about research and morality.

In December of 1964, not too long after initial concerns about classified research were raised, a prominent professor and physicist at the University of Michigan requested

[12] ibid.

to meet with the Research Policy Subcommittee.[13] This physics professor, Kip Siegel, was quite well-known for his research on thermonuclear fusion.[14] Having many public-private partnerships himself, Siegel expressed frustration with University of Michigan's Institute for Science and Technology (IST), the broader university laboratory under which Willow Run operated. "[H]e alleged," said S.W. Churchill, member of the Policy Subcommittee *and* of the University of Michigan IST's (the larger University research center of which Willow Run was a part, if you recall) executive board, "that the IST Laboratories were competing unfairly[,]"

> and against the State's interest with private companies [...] for contracts which were not properly the business of a university. Furthermore, he alleged that the behavior of IST employees was frequently unethical with respect to seeking contracts and with respect to proprietary information. He also questioned the University's policy on publication rights.

In other words, Siegel felt that the IST was involved (and, therefore, was involving the University) with conflicts of interests, particularly with respect to relationships between the state and private companies. I hypothesize that IST's contracts were potentially disadvantageous to Siegel and his research as well. It is also possible that there was bad blood between Siegel and researchers at Willow Run. Siegel was involved with radar research at Willow Run right after he graduated from Rensselaer Polytechnic Institute (RPI) in the early 1950s. "Siegel [implemented] [...] an excellent

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[13] March 9 1965 Memorandum to James T. Wilson from S.W. Churchill about Request for IST Materials. Institute of Science and Technology University of Michigan Records 1950-1989. Call no. 90132 Bimu C147 2. Box 10, Folder "SACUA - Research Policy Cmte Report to Subcmte on WRL." Bentley Historical Library, Ann Arbor, MI.

[14] While not the focus of this dissertation, Siegel's story is an interesting one. A very well-known high-energy physicist at the time, the Ann Arbor News says he "invented the field of optics."
He founded Conductron Corporation, KMS Industries and KMS fusion. It's also worth noting that "KMS Fusion was the first and only private sector company to pursue controlled thermonuclear fusion research through use of laser technology. On May 1, 1974, KMS Fusion carried out the world's first successful laser-induced fusion in a deuterium-tritium pellet [...]" (Wyatt 2009). Not loved by the DoD and public agencies, Siegel died of a heart attack in 1975 while testifying before Congress, defending an independent researcher's right to carry out powerful experimentation.

program that including testing to measure the radar cross sections of scale model V-2s launched from aircraft" (23), writes WRL research alumnus Irving J. Sattinger in his portion of the Willow Run and ERIM internal histories. Further specifics on the nature of Siegel's relationship with researchers at Willow Run, or the nature of WRL's potential conflicts of interest, are not clear from the archival materials available.

Nevertheless, it was Siegel's allegations that sparked particular interest in IST. And because of these allegations, the Research Policy Subcommittee asked IST for organizational charts, current projects and costs, proposal titles and corresponding amounts of funding, recent publications, personnel, numbers of research and teaching faculty, and budgets to investigate.[15][16] Furthermore, the subcommittee mentioned particular interest in having the aforementioned information on Willow Run Laboratories.

University scrutiny held continued focus on Willow Run through the mid-late 1960s. In 1966, minutes from an April Committee on Research Policy Meeting mention the "Future of the Willow Run Laboratories." "The Subcommittee [on Classified Research]," says the minutes, "has devoted considerable time to the question of how the Willow Run operation can be integrated into the teaching functions of the University. The problem is still under study."[17] Some of my archival sources refer to a debacle between

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[15] March 24 1965 Memorandum to S.W. Churchill from James T. Wilson with Materials on IST and WRL. Institute of Science and Technology University of Michigan Records 1950-1989. Call no. 90132 Bimu C147 2. Box 10, Folder "SACUA - Research Policy Cmte Report to Subcmte on WRL." Bentley Historical Library, Ann Arbor, MI.

[16] March 9 1965 Memorandum to James T. Wilson from S.W. Churchill about Request for IST Materials. Institute of Science and Technology University of Michigan Records 1950-1989. Call no. 90132 Bimu C147 2. Box 10, Folder "SACUA - Research Policy Cmte Report to Subcmte on WRL." Bentley Historical Library, Ann Arbor, MI.

[17] Report of the Subcommittee on Research Policy, April 1966. University of Michigan Senate Advisory Committee on University Affairs 1954-1967. Call no. Flmu B31a.2. Vol. 12, No. 4. Bentley Historical Library, Ann Arbor, MI.

the University's Subcommittee on Classified Research and Willow Run Laboratories coming to a head in 1968. The Michigan Daily, for instance, notes a "a student referendum asking a ban on classified research in 1968" (Koppman 1971), which was ultimately defeated.

In February of 1971, however, the Subcommittee's contemplation about Willow Run Laboratories — and the future of classified research at the University of Michigan — came to a head. Michael D. Knox, a PhD student in the Department of Psychology, was one of three students sitting on the Subcommittee on Classified Research. He published an editorial in the Michigan Daily, University of Michigan's student-run newspaper. *Research panel member hits 'U' military work*, the heading reads. "Michael Knox," says the front page of the Michigan Daily, "[...] says the committee has approved projects which are 'making a significant contribution to the war technology which is currently being used to destroy and kill in Southeast Asia.'"

"University researchers," divulges Knox in a report by Dave Chudwin, "especially at Willow Run Laboratories in Ypsilanti,"

> are heavily involved in military projects in a variety of fields dealing with surveillance, target-acquisition and countermeasures. Remote sensing with infrared or heat-measuring instruments was largely developed here, and Willow Run researchers are using the devices to measure the heat characteristics (signatures) of missile plumes, vehicles and other military targets. The Defense Department is using infrared devices similar to those developed here in Vietnam to pinpoint troops and supplies at night and in rainy weather. They can pick up even small cooking fires (8; 1971).

Here, Knox alleges that the satellite-related research conducted at Willow Run, namely anything infrared — or anything labeled "Remote Sensing. Countermeasures Techniques. Target Signatures. Acoustic Surveillance. Infrared Surveillance and

Countermeasures" (ibid) — was for the sole purpose of targeting and killing enemy forces in other countries. "All of these seemingly innocuous research areas are," implicates Knox in a complementary editorial, "in reality, sophisticated weaponry enhancing destructive capabilities in Vietnam, Cambodia, and Laos" (Knox 4; 1971). The editorial is flanked by articles about peace treaties and the invasion of Laos.

Sattinger has a different take on this idea of *countermeasures*. He writes that the Committee on Classified Research "rejected WRL proposals as well," mostly because they misunderstood "the basic purpose of countermeasures systems" (84; 1994):

> Willow Run work in the area of countermeasures, which had been a strong program up to that time, was especially hard hit. [...] The committee considered countermeasures work to be outside the limits of acceptable research even though countermeasures are for the most part defensive techniques aimed at protecting ships, tanks, aircraft, and the people inside them (ibid).

In other words, Sattinger frames countermeasures not as technology that is meant to hurt or destroy, but as one that is meant to save and protect. The ways in which the anti-war parties and the researchers whose work pertains to warfare talk past each other are most visible in this kind framing.

Knox's editorial had a couple of notable aftershocks: (1) an entry into a debate about remote sensing, and whether technologies developed for defense could escape their warfighting essences if repurposed peacefully; (2) the ultimate dissolution of the Willow Run Laboratories, and its reestablishment as the Environmental Research Institute at Michigan (ERIM). In the following pages, I will explain each of these after-effects in some detail, and expound upon their relevance to Landsat — and earth remote sensing research more generally — now.

Earth Remote Sensing and *Original Sin*

The issue is best summarized by the Michigan Daily.

"The issues of classified and military research arouse strong emotions, on the parts of both faculty and students," writes Steve Koppman. "This is to be expected, for they raise fundamental questions involving the University's relationship with the government, the meaning of academic freedom, [...] and secrecy in an academic community" (1; 1971).

"It is sometimes argued," wrote Knox in his editorial, "that the results of this military research can be applied to peaceful purposes":

> Research funded by military agencies is used by the military for its own purposes; when data and reports are classified they are not generally available for peaceful and humane applications. Moreover, the nature and mission of the agencies sponsoring this type of research mitigates against the non-military utilization of the knowledge obtained (4; 1971).

I'll translate a lighter version of Knox's argument: if the original funders of the research are military in nature, the research will always contribute some benefit to defense causes. Furthermore, when any project has been classified, we should be aware that it may not be peaceable in nature.

At this point, however, I want to draw your attention to the issue of *original sin*[18] in this debate: a contemplation about whether this research has some kind of net-positive or -negative (or -neutral) value. Ultimately, I believe this is not the right way to conceptualize this kind of debate. However, it is worth exploring the ways in which this *original sin* argument — and its counterarguments — manifest in the controversy over Willow Run. *Original sin* here begins with a well-intentioned research policy adopted by the Classified Research Committee. The Committee's Research Policy I reads as follows: "The University will not enter into any contract supporting research the specific purpose of which is to destroy human life or incapacitate human beings."[19] It is unclear from the archival materials exactly when Policy I (and its sister policies) was developed. However, the archival materials show that these policies were established by the Classified Research Committee. Additionally, the Subcommittees reports show that Policy I (alongside Policy II, which bans non-disclosure agreements in research contracts), was "formulated to exclude certain types of projects which the Committee views as clearly

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[18] By invoking the term *original sin*, I do not mean to imply that Earth Remote Sensing — or Landsat — is sinful. Rather, I refer to a kind of critique often made in STS. To be sure, there are many technoscientific histories that have done harm to marginalized groups. There are many examples of this across the history of science, technology, and medicine. One case that cuts across all three of this, for instance, is MaryCatherine McDonald's work on the history of the MRI, and the ways in which it was used to minimize the trauma of war veterans (2016). It is necessary for us to understand the cost of the discoveries that make our world the way it is. I have noticed that critiques in STS tend to sit there. We uncover the history of a technoscientific artifact, and the ways in which the development of that artifact has done a lot of harm. What we have not been trained to do, however, is to *do something* about that history. How do we think it is manifesting today? And what could we change to make the technoscience more equitable? I call this halfway-finished critique (exemplar of the critique-to-action paradigm) a critique of original sin. It seems as though it is only there to point out an unsavory (sometimes, unsavory at best) history, and then leave its audience in shock. I call this kind of critique one of *original sin* because I aim to put a name to it, and then to move beyond it.

[19] Annual Report of the Senate Assembly Classified Research Committee, March 1, 1971, pg. 3. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987). Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

unacceptable under University auspices," and promises to reject projects that do not conform to the policy.[20]

In a March 1971 breakdown of discrepancies between his own views and the views of the Committee on Classified Research, Knox implicates the committee for a blatant ignorance of Policy I. "Although the meaning of Policy I is clear to me," he says, "it is apparently ambiguous enough that other Committee members are able to justify approving research to develop weapons systems and subsystems."[21] Additionally, there is a Policy V, which states that "[t]he University will not enter into any classified research contract funded by a Department of Defense agency or any other sponsor which kills or injures other human beings."[22] To be sure, Knox notes that the Classified Research Committee rejected only two proposals in the span of two and a half years.[23] It is clear to me that Willow Run's proposals were more than likely in violation of these two policies, at least.

This debate continues to play out between James T. Wilson, then-Director of the Institute of Science and Technology, and Knox in a February 19th, 1971 issue of Science Magazine. "In Wilson's opinion [...]," reads the article, "Knox over emphasizes the

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[20] Annual Report of the Senate Assembly Classified Research Committee, March 1, 1971, pg. 3. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987). Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

[21] A Report in Response to the Annual Report of the Senate Assembly Classified Research Committee, March 1, 1971, pg. 4. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987).. Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

[22] A Report in Response to the Annual Report of the Senate Assembly Classified Research Committee, March 1, 1971, pg. 2. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987).. Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

[23] ibid.

military applications. ‘He starts at the Viet Nam end and works back to the basic research [...]’”:

> ‘Obviously,’ Wilson continued, ‘the military wouldn’t support the work if there weren’t military applications.’ But he added that ‘the Willow Run Laboratories pioneered remote-sensing devices even before the military applications were foreseen. And ‘a little more than one-third of Willow Run’s budget now comes from non-Defense Department sources.’ According to Wilson, only about 10 percent of the staff at Willow Run are regular university faculty members. Most, he said, are older graduate students who might work on the classified aspects of military contracts, but who always publish their theses in nonclassified areas. [Furthermore,] Wilson emphasized that the Willow Run facility builds no prototypes of military hardware, only ‘breadboard models to collect data’ (656; 1971).

Wilson’s arguments are as follows: Willow Run has expanded its non-defense budget to a little over one-third of its research. Despite the fact that many Willow Run research faculty don’t teach, the research still benefits the University because older graduate students gain important experience while working on it. And, furthermore, those older graduate students publish their work in nonclassified journals. Finally, Wilson points out that labs like Willow Run don’t do exact prototyping for the military. Rather, they do a more approximate prototyping with breadboard models and data collection (note that the Wikipedia definition of breadboard is “a construction base for prototyping of electronics”). Science Magazine points out that “Michigan receives about $10.4 million per year from the Defense Department in research contracts, half of which have classified portions. Most of [this research] is performed under the direction of the [IST] at the Willow Run Laboratories [...]” (ibid).

This is not the only place in which this *multistable* view is expressed. In an interview with the Detroit Sunday News, William Brown, the Director of Willow Run Laboratories (who would go on to become the director of the Environmental Research Institute at Michigan) offers a similar position.

> Remote sensing techniques developed here have been used to spot and control crop diseases, produce better radar systems for the nation's airports, chart weather flow and detect air and water pollution long before they can be monitored by conventional means. [...] Eventually, sensory technology may be able to predict earthquakes, tidal waves and volcanic activity, perhaps saving countless lives and millions in property damage. [...] The Earth Resources Technology Satellite [Landsat-1] due to be launched early next year will contain remote sensory equipment developed here (Peterson 11).

Brown points to ways in which remote sensing research can not only have applications we categorize as *peaceful* or *humane*, but that can potentially save communities from natural disasters, damage, and death. Note that Brown makes mention of the first Landsat satellite as exemplar of this technology, repurposed. This seems in-line with Irvin Sattinger's characterization of environmental remote sensing research: that "environmental research [is] assumed to include helping to defend the free political environment" (92; 1994). What a free political environment might entail, however, is not clear. Nor is it clear how we'd get there. This framing is not unlike the data-to-action paradigm, in which action and a subsequent societal good (or political freedom) manifest automatically and naturally, and are brought about by these technologies. More earth remote sensing technologies, of course, lead to more data. More data leads to some action of defense which will, in turn, lead to some ideal *free political environment*. In the following chapter, I will further explore this kind of language as it relates to more broadly to both military and environmental research.

We see this kind of language manifest in our language about Landsat to this day. For instance, on Landsat's current website, there are scrolling quotations of endorsement. I will provide a few examples here. Matt Hansen, Co-Director of the Global Land Analysis & Discovery (GLAD) unit at the University of Maryland (and a Remote Sensing scientist who specializes in mapping changes in land use) is quoted as saying that

"Landsat represents a public good, Earth-observation infrastructure that allows everyone to study their respective land resources and their change over time."[24] And Tom Loveland, Co-Director of the USGS Geospatial Sciences Center of Excellence — who is also Director of the USGS Land Cover Institute and a geographer at the USGS Earth Resources Observation and Science (EROS) Center — "[summarizes] Landsat 8's science impacts in three ways: More data, better data, and improved, expanded applications."[25] Larisa Serbina, a corporate strategist for the Climate Corporation, and Holly Miller, a lab manager at the University of Colorado Boulder's Institute for Arctic and Alpine Research, say that,

> [i]n the world of water resource management, Landsat has played a key role in providing objective and continuous data for the United States, particularly in the arid west. Water-related benefits of Landsat imagery are also reaped far beyond the United States' borders in countries such as Chile, Australia, Morocco, Sunda, and Venezuela, which are using Landsat data to make informed decisions regarding natural resource allocation and use.[26]

These are just a couple of examples of quotations from Landsat's NASA homepage. Additionally, just off of this homepage, there is a *Societal Benefits* section for visitors to read about. "To feed and shelter the planet's growing population," it reads, "extensive and intensive land use has been required, but the environmental degradation caused by these requisite activities is diminishing the planet's capacity to sustain needed food and fiber production and fresh water supply [...]":

> Land imaging from moderate-resolution Earth-observing satellites, such as Landsat, offer the critical and irreplaceable capability to observe land use and land use change across those scales. Landsat's space-based land imaging is essential because it provides repetitive and synoptic observations of the Earth otherwise unavailable to researchers and

[24] From https://landsat.gsfc.nasa.gov/.

[25] ibid.

[26] ibid.

> managers who work across wide geographical areas and applications. Landsat data informs good decisions in many disciplines, especially: human health, agriculture, climate, energy, fire, natural disasters, urban growth, water management, ecosystems and biodiversity, and forest management.[27]

I want to put forth the idea that this rhetoric finds a great deal in common with that of Wilson or Brown, defending DoD-funded activities in Remote Sensing to a skeptical, anti-war public. However, we often don't think about this kind of environmental research as having military inclinations — at least not directly. I hypothesize that the *original sin* argument about military technologies — like Earth remote sensing ones, and as expressed by Knox — is really the origin of this kind of language. It assumes a kind of automatic action toward greater good. When we examine Landsat's history, we see that this rhetoric originated as a kind of defensive response to the implication that these technologies will only ever be harmful. However, I don't imagine this kind of rhetoric is an actively defensive response today. Rather, this rhetoric has become the norm in our framing of Remote Sensing technologies. We don't question it, or wonder where that positioning comes from. Furthermore, this rhetoric cuts off opportunities for us to expand upon what Remote Sensing actually does for society, and to engage with it a different scales. I will revisit this hypothesis at the end of this chapter, and tease out more details from the lessons of Willow Run's history. There is still more to learn, however, from the controversy over Willow Run Laboratories, and its re-establishment at the Environmental Research Institute at Michigan (ERIM).

Endings and Beginnings

[27] https://landsat.gsfc.nasa.gov/how_landsat_helps/

"Though we do not wish to prejudge specific policies," reads a Summary of Senate Assembly Actions from March 22, 1971,

> The Assembly instructs the committees to work out means of barring classified military research whose clearly foreseeable purpose is to destroy human life or to incapacitate human beings. By this instruction we show our desire to extend the criterion well beyond the 'specific purposes' wording [...].

In other words, the University Senate took seriously the idea that perhaps they should draw the line somewhere — that perhaps certain kinds of research were more likely to extinguish human life than others, and perhaps weapons research was not something the school should be known for. On November 22, 1971, multiple members of the Willow Run Laboratories came before the Senate Assembly to present counter-arguments to these decisions, and to potential changes in research policy that might follow.

Dr. George Zissis, a research scientist at Willow Run Laboratories, whose obituary describes him as someone who was a "pioneer in the field of remote sensing,"[28] implored the Senate Assembly to consider their decision more carefully. "A resolution has been made to alter research policies on a long-term basis," he said, "without a clearly exhibited documented rationale or foundation." He continued on to argue that the rationale presented "two difficulties": that there was no practical solution to be put into place by administrators ("the case for the resolution must be purely surmised by the Administrative officers who are to act upon it"), and that the issue was a case of

[28] Source: https://www.osa.org/en-us/about_osa/newsroom/obituaries/earlier/georgezissis/

semantics ("the case or foundation serves as a contextual delimitation of the meaning of the words and phrases of the resolution").[29]

For further context, Dr. Zissis was the head of the Infrared Physics Laboratory at the University of Michigan. That lab was responsible for two Willow Run projects under scrutiny: the ARPA Multiband Photographic and Infrared Reconnaissance Tests (AMPIRT) and the Joint Military Research Development Center U.S. Aerial Reconnaissance Laboratory. Together, these projects — along with two additional ones from the Geophysical Laboratory — were referred to as the "Thailand Projects." Scrutiny of these projects began in 1968 (Sattinger 83; 1994).

Dr. Wilfred Kaplan, a mathematics professor at University of Michigan, came to Zissis' and Willow Run's defense. "To me," he said, "it is an expression of a political attitude, a political evaluation of a foreign policy of our government.

> Given different circumstances and a different policy, the supporters of the action might easily reverse their attitude and enthusiastically support secret military research on campus, since they see that their security is at stake. I believe it is wholly improper for the University to take such a political action. Furthermore, I find a strange inconsistency in our continually appealing to the state and federal governments for support, while regarding the University as a sacrosanct institution which must not be sullied by working on some of the harsh real problems these governments face.[30]

Here, Dr. Kaplan's concerns speak directly to the culture surrounding the Vietnam War. Countermeasures research had been going on for quite some time, but was never before framed as an issue of morality. *Why begin now, during this war?*, is essentially what Kaplan is asking. And why hold the university to a kind of pure, untouchable

[29] University Senate Meeting, Monday, November 22, 1971, pg. 8. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987).. Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

[30] University Senate Meeting, Monday, November 22, 1971, pg. 9. Senate (University of Michigan) records, 1880-1995 (bulk 1906-1987). Call no. 87309 Bimu B31a 2. Bentley Historical Library, Ann Arbor, MI.

cultural standard that is not reflective of broader narratives surrounding the war? We may not agree with Kaplan, but his arguments poke holes in the general sentiment, from student and faculty activists, that *original sin* is the best point of entry into these discussions.

Willow Run's Transition to ERIM: A Closer Look at Legislation and Funding

The aforementioned debate did lead to the dissolution of Willow Run Laboratories, and to its subsequent reincarnation as the Environmental Research Institute of Michigan (ERIM). The archival materials show — and Sattinger's internal history confirms — that Willow Run was experiencing some financial trouble around the time of its transition.[31] Additionally, Willow Run's closure was a sticky issue for the University of Michigan. On one hand, its continued existence offered publicity issues for the school, and for the laboratory itself. On the other hand, WRL housed world-class researchers, cutting edge research, and brought in important research dollars and jobs for both the institution and the state. This precarious set of concerns ultimately led to Willow Run's re-establishment as the Environmental Research Institute at Michigan, or ERIM. ERIM, a non-profit organization, could still collaborate with the university. However, its status as a non-profit allowed it to maintain some separation as well. Several sources confirm that ERIM borrowed state funds to do this. Michigan Daily reporter David Stoll writes that ERIM made an appeal to "Washtenaw County for $3 million in tax exempt industrial

[31] April 8 1971 Memorandum to Robert W. Fleming from James T. Wilson. Institute of Science and Technology University of Michigan Records 1950-1989. Call no. 90132 Bimu C147 2. Box 7, Folder "Willow Run to ERIM transition docs." Bentley Historical Library, Ann Arbor, MI.

revenue bonds" (1974). Sattinger's account is slightly different. "The university administration and the state legislature agreed with WRL's decision to leave," he writes.

> Bill Brown, director of WRL, was primarily responsible for conducting the negotiations with U-M and the Michigan Legislature that led to the eventual separation of WRL. [...] Eventually, the necessary steps for the separation of Willow Run Laboratories from U-M were agreed upon. This included a special act of the legislature that would make available a loan of up to $2 million as working capital for the operation of the separate organization. [...] On June 27, 1972, the Environmental Research Institute of Michigan filed articles of incorporation. [...] [O]n June 30, after several day of anxious waiting at WRL, the Michigan Legislature passed Amendment 124 to the Higher Education Bill, thereby extending to ERIM an interest-bearing repayable loan of $2 million as working capital (88-89; 1994).

Dr. William Brown, the director of Willow Run Laboratories and subsequently of ERIM, lobbied the state legislature for money necessary for the separation. Sattinger argues that this was necessary because "[t]he availability of a source of working capital was vital to the success of the separation. Among other things, it allowed the new organization to pay its employees and vendors while waiting to collect money from its sponsors on a more leisurely time scale" (ibid). And so, on June 30th — just 3 days after ERIM officially established itself as an incorporated entity — Sattinger says that the Michigan Senate passed a legislative amendment to the Higher Education Bill that allowed for a $2,000,000.00 loan to be paid out.

I took a closer look at the legal documents pertaining to this legislation. There's record of the transaction in the Michigan Senate's Public Acts of 1972 documentation. Act No. 260, which is an appropriations act "for the state institutions of higher education and certain other purposes relating to education for [FY 1972-1973] [...] and to provide for the disposition of fees and other income received by various state agencies" (695; 1972). Essentially, this is the state's official documentation of its yearly provisions for state colleges and universities, and for other educational pursuits that it deems worthy of public funding. There are also restrictions within this documentation: restrictions on issues like lobbying (one isn't permitted to lobby with state funds, says Section 6) and firearms (one isn't permitted to use appropriations for the purchase of firearms, at least "without being a peace officer employed by an institution of higher education, [or] any firearm not registered with the institution or other dangerous weapon in any university [...] including all the buildings and grounds under their jurisdiction" [714; ibid], says Section 12).

Section 18, the final Section within Act 260, deals with ERIM. It is titled *Environmental research institute; loans; board of directors; interest*. It reads:

> In order to provide for the orderly total separation of the Willow Run research facilities of the university of Michigan from the university without the diminishing of research capacity and to avoid the potential for economic and job loss in the state, the state treasurer is authorized to invest not to exceed $2,000,000.00 of the funds of the retirement systems in loans to the environmental research institute of Michigan, a Michigan nonprofit corporation established for the purpose of research at the Willow Run laboratories presently under the control of the university of Michigan. The loans shall be secured and the state treasurer, the director of the department of commerce and the secretary of state shall be members of the board of directors of the institute until the loans are completely repaid. The rate of interest of the loans shall be 2% in excess of the prime rate as determined by the state treasurer and adjusted quarterly, based upon the current prime rate in the market place. This act is ordered take immediate effect. Approved August 9, 1972 (716; ibid).

Section 18 of the appropriations act authorizes the state of Michigan to loan ERIM $2,000,000, the interest rate of which will be an amount that the state treasurer decides upon plus 2%. Additionally, it appears that the Section was actually approved on August 9th, 1972. In order to follow up on these two threads, I attempted to find more legislative resources on the act, and on amendment 124. A law librarian for the Michigan legislature instructed me to search for these documents using other terms, and not using the term “amendment 124,” as the appropriations acts and information pertaining to them doesn’t quite fall under the category *amendment*. Additionally, the Virginia Tech library does not have access to the kinds of law-related materials that would make it possible to find further information about the appropriations bill, and negotiations leading up to it. Therefore, I am leaving these threads loose. I hope other scholars will take them up.

However, I was able to find out more about the broader issue of state funding for ERIM. In short, state support for ERIM had been growing for a while. The state legislature indicated early approval of the concept of ERIM. On July 22nd, 1971, for instance, Michigan Daily reporter Chris Parks wrote that, a week prior, “the state senate gave encouragement to the plan [to re-establish Willow Run within a separate research institute] by passing a resolution introduced by Sen. Bursley [...].” However, Parks continues on to add that this vote “has no direct power,” and that the decision ultimately rests on the shoulders of the University’s Board of Regents.

Gilbert Bursley, a Republican state senator (1964-1978, and a state representative from 1960-1964), was outspoken on the ERIM matter on multiple accounts.[32] A September 1971 edition of the Michigan Daily has Bursley speaking in defense of Willow Run, and the important revenue it brings to the state of Michigan. “The climate on college campuses, particularly Michigan’s, is not favorable to classified research [...]. The ongoing hostility has lowered the morale of Willow Run’s staff considerably,” argued Bursley. Representative Raymond Smit, also a Republican and a supporter of state

[32] It’s also worth noting that Bursley was outspoken on other educational issues, and was a sponsor for a bill allowing sex education in public schools in the state of Michigan. In a 1975 edition of the Michigan Daily, he’s quoted, in an effort to assuage worries, as saying that “All [the bill] does is allow a sex education curriculum to include reference to contraception and venereal disease. [...] It’s incredible the number of teenagers who don’t know how babies are made” (10; 1975).

assistance for a re-established Willow Run, chimed in: “The state can’t afford to lose Willow Run’s payroll and prestige.” In short, state legislators like Bursley had incentive to assist ERIM in its founding, given that it was an important source of jobs and revenue for the area.

It’s also worth noting that the Representative Marvin Stempien (Democrat, and representative to Michigan’s 35th district from 1965-1966, and 1969-1972) is responsible ERIM’s name. “[...] Stempien, the majority whip of the state house of representatives and a key participant in the legislative effort that separated the laboratories from the university, strongly recommended [the new name] [...]” (Sattinger 92; 1994). Stempien felt that the name, in this particular arrangement, “recognized the organization’s contributions to earth resource management and encouraged further participation in this type of work” (ibid). Sattinger mentions that this framing helps us conceptualize “environmental research” as an occupation that “[helps us] [...] defend the free political environment in which the world would later increasingly operate” (ibid). In the next chapter, I’ll explore the deep connections between ecological research and research on national security in greater depth.

What’s behind the term *environmental*, and what would have given politicians at the time incentive to use it? Environmental historian Samuel P. Hays offers up a brief history of the term *environmental*, and the ways in which it evolved over time. *Environmental*, writes Hays, first starting popping up in post-World War II America. Early proponents of environmentalism questioned the whole concept of *resource management*. Hays sees this questioning as an outgrowth of certain pre-war values:

namely, a love of "outdoor recreation" (16; 1982). "But there were other strands even less rooted in the past," he writes:

> The most extensive was the concern for environmental pollution, or 'environmental protection' as it came to be called in technical and managerial circles. While smoldering in varied and diverse ways in this or that setting from many years before, this concern burst forth to national prominence in the mid-1960s and especially in air and water pollution. [...] One of the most striking differences between these post-War environmental activities, in contrast with the earlier conservation affairs, was their social roots. Earlier one can find little in the way of broad popular support for the substantive objectives of conservation, little 'movement' organization, and the scanty evidence of broadly shared conservation values. The drive came from the top down, from technical and managerial leaders. [...] [I]n sharp contrast, the Environmental era displayed demands from the grass-roots, demands that are well charted by the innumerable citizen organizations and studies of public attitudes (16-17; ibid).

In the post-World War II era — and particularly in the 1960s — we begin to see narratives of environmental protection with respect to pollution. And while conservation-based, resource management narratives were, as Hays says, *top down*, environmentalism was a grassroots movement. Hays examines surveys in which Americans expressed environmental values. He finds that environmental consciousness "[reflected] a desire for a better 'quality of life'" (20; ibid), and notes that these desires "tend to be stronger with younger people and increasing levels of education" (ibid).

While we don't have access to the conversations that Michigan state politicians were having in the early 1970s, we can begin to make inferences from these kinds of historical analyses. If the goal was, indeed, to assuage worries about the connections between the DoD and earth remote sensing, titling this newly established non-profit as an *environmental* one was a smart move. It spoke to the needs and desires of grass-roots motivated young people; of, say, college activists. And it implies, in essence, that this top-down organization might really be bottom-up. The re-labeling and re-establishment of WRL as ERIM after a controversial run-in with activists was a strategic move with respect to the organization's public relations.

Although the controversy surrounding classified research at University of Michigan had subsided after 1972, some students and faculty were still unhappy with this new development. Stoll writes: "ERIM President Dr. William Brown says that in 1974 'more than half' its budget still came from classified military contracts; the proportion has remained steady in recent years, and according to ERIM literature, its present work builds on its past" (ibid). In other words, ERIM is Willow Run, but under a different name.

Archival materials confirm that ERIM was a different name for the same thing. A founding document provides some information on the manner in which ERIM would be staffed. "As part of the negotiations leading to the formation of ERIM," it reads

> An agreement was reached between the University administration and the leadership of the Willow Run Laboratories to the effect that ERIM would tender offers of employment to substantially the entire technical staff of the Willow Run Laboratories. Concurrently, the University agreed to transfer the ongoing Willow Run Laboratories research activities to ERIM in total rather than on a piecemeal basis. The net effect of these agreements is that the support for essentially all current WRL technical staff members has been moved to ERIM, and formal offers of employment have already been made by ERIM to all staff members actively employed by Willow Run Laboratories. [...] At the present time, all key personnel have committed themselves to remaining with ERIM; and 95% of the total staff has done the same.[33]

This report confirms that the great majority of Willow Run's personnel remained on board for the ERIM transition. Therefore, ERIM was, from a Human Resources perspective, the same organization.

[33] Agreement to Recognize Successor in Interest as required by ASPR 26-402, pg. 2. Institute of Science and Technology University of Michigan Records 1950-1989. Call no. 90132 Bimu C147 2. Box 7, Folder "Novation Agreement." Bentley Historical Library, Ann Arbor, MI.

One difference is clear, though: it is difficult, as an archival researcher, to find information on ERIM's activities. I expect that this relates to ERIM's status as a private non-profit. As it's not a public institution, it doesn't have to be accountable in the same way that, for instance, a laboratory at the University of Michigan does. When I finally procured the dusty cardboard boxes containing ERIM files from the Bentley Historical Library at the University of Michigan, I was disappointed to find that this box, unlike the ones pertaining to Willow Run, weren't as filled with memos and documentation and letters on official letterhead. Rather, they were mostly filled with brochure-like papers, advertising ERIM's remote sensing symposia. There was little to no report of the internal operations of the organization. As reporter John E. Peterson of the Detroit Sunday News points out, "Ironically, this victory for the activists is likely to mean that more Defense Department research will be done at the laboratories, not less" (11; 1971).

It's also worth noting that, had the education appropriations bill not been favorable to Willow Run/ERIM, Willow Run administrators were ready with contingency plans to ask the University of Michigan for start-up money. In a May 23, 1972 memo to University President Fleming, assorted University Vice Presidents, and Dr. Wilson of IST, Willow Run Vice President A.G. Normal wrote:

> Dr. Brown and his immediate colleagues are extremely reluctant to accept the conclusion that help is probably not forthcoming from Lansing. [...] The fall-back position apparently taken by some of Brown's colleagues — and to some extent by Brown — is that the 'only decent thing' for the University to do is to provide sufficient money or backing so that the Laboratories could go it alone. This could take several forms, they suggest: (1) Operate WRL as a 'fully-owned subsidiary' and a separate cost center until such time as it had all the internal capability and resources to stand alone. [...] (2) Provide a long-term 'loan' of at least $250K either in combination with some action by the State or with local banks [...].

Essentially, Brown and other WRL administrators, when decisions by state legislators and the UM Board of Regents regarding funding weren't looking as optimistic, were prepared to ask the school for financing or a loan.

Conclusion

I provided you with a history of Landsat's military roots, not to belabor this issue of *original sin*. As we can see from the examples, *original sin* arguments about Earth Remote Sensing get us nowhere. We remain in a *these technologies kill* and a *but they have humanitarian purposes* gridlock in which we talk past each other. Nothing gets accomplished and, as we saw, classified research can become even more shrouded in secrecy when we attempt to put a halt to it with the weaponry of *original sin*.

Rather, I've shown you Landsat's military roots because I believe they matter to our understanding of the data-to-action paradigm. There are *two* main ways in which these roots influence this paradigm. The first is that a kind of taken-for-granted rhetoric surrounding technologies like Landsat is a *direct reaction* to the kinds of *original sin* arguments we saw in this chapter. This rhetoric originated from the defensive responses put forth my remote sensing researchers during the Vietnam War. These researchers attempted to invoke a kind of *data toward action and societal good* rhetoric to shift attention away from the *countermeasures* applications of these technologies. This rhetoric is still alive and well in the Landsat community. However, this rhetoric not a defensive reaction to accusations that Landsat kills. Rather, I hypothesize, the rhetoric is a relic of the kinds of debates over classified research shown in this chapter. This kind of defensive rhetoric helped to establish — and hold in place — this notion that processes between data and action (and some imagined, singular good) happen automatically. Even though Remote Sensing has invisibilized military roots, these roots are still shaping our taken-for-grantedness of data's affects.

These roots are linear: they don't grapple with complex sociotechnical systems. Data doesn't just *arrive* in these complex systems and work it's magic: it is not as simple as assuaging worries. To bring about change with respect to our environment — real societal benefits — we have to make that data work. Making that data work is highly undervalued — and necessarily interdisciplinary — research. When we acknowledge that we must *put data to work* with intentionality, we also acknowledge that we have never been modern. What we have now, though, is a Latourian purification. Additionally, we have a military *stability* that is entangled with an ecological *stability*.

There is a second way in which Landsat's military history brings us to a greater understanding of the data-to-action paradigm. It is less clear, and I believe it has to do with automated battlefield technologies like Project MICHIGAN. To be sure, I don't have expertise on what modern-day battlefield surveillance and assessment looks like. However, Landsat's instrumental history, in its exploration of automated sensing technologies like Project Michigan, tells us this: data collection, assessment, and subsequent action is all an automated process. Therefore, more data seems to yield more action.

I conjecture that this, too, helped to contribute to — and solidify — the data-to-action paradigm for Remote Sensing technologies. And, while this paradigm may work for a strictly defense-oriented remote sensing, it doesn't work for environmental monitoring. This is a case in which Landsat's military *stability* is deeply intertwined with its ecological *stability*, making it impossible for us to bring forth one without bringing forth the other. The processes between data and action need to be imagined more clearly for such a complex problem, that spans years and not days, and that doesn't require a single action or a few actions, but many, many emergent processes. Understanding this history — more specifically, the ways in which military and ecological stabilities are connected through a very strong *pivot point* (Whyte 75; 2015) — allows us to see why and how we're lacking in this arena. My intervention into Remote Sensing is to make this history clear, along with this multistable relationship. Furthermore, this intervention ought to guide further interdisciplinary reflection. This reflection should involve a conversation about the ways in which can build the processes between data and action into environmental remote sensing with more intentionality.

Ultimately, writing this chapter has left me with more questions than answers. What I have, rather than a prescription, is an entryway into a dialogue. The dialogue will take place, I imagine, after people in both Remote Sensing and STS fields (and in related fields and subfields) read this chapter, or listen to a presentation or a talk that I give based on this chapter. I hope that what will follow is an earnest conversation that transcends one about *original sin*, and that contemplates what we should carry forward from this history. Some branches of political philosophy — namely, the Frankfurt School — tell us that this is a realistic approach to such a complex problem. The notion that change is *dialogic*, or that change is the result of effective dialogue, comes from German philosopher Jürgen

Habermas. Habermas's formulations around *communicative rationality* provide us with a framework for such a complicated, interdisciplinary dialogue as the one that could happen around this data-to-action paradigm and its military roots.

Philosopher Rick Roderick notes that "Habermas distinguishes between 'action oriented to success' and 'action oriented to understanding'" (207; 1985). "Strategic action" and "instrumental action" make up Habermas's notion of *action oriented to success*, says Roderick. Instrumental action, according to Habermas, is "action [that] can be understood as following technical rules and can be evaluated in terms of efficiency in dealing with the physical world" (ibid).[34] Strategic action, meanwhile, amounts to "following rules of rational choice and can be evaluated in terms of efficiency in influencing the decisions of other social actors viewed as potential opponents" (ibid). In essence, *action oriented to success* is about things that are measurable, and resides in our data-to-action paradigm: we know something is successful if it can be measured. However, the data-to-action paradigm shows us that just because something is measurable, does not mean it is actionable, or productive in the ways that we want it to be. According to Habermas, *instrumentality* describes this notion that we can evaluate and benchmark this measurable success.

Habermas, however, argues for more emphasis on *action oriented to understanding*, or *communicative action*. "Communicative action," writes Roderick,

> is co-ordinated not through the egocentric calculations of the success of the actor as an individual, but through the mutual and co-operative achievement of understanding among participants. It is directed toward achieving a genuine agreement based on the

[34] Note that this is slightly different than the *instrumental* vocabulary and meaning invoked by political philosopher Yaron Ezrahi (*Descent of Icarus*, 1990). However, exploring and clarifying this distinction is not the goal of this dissertation.

> intersubjective recognition of 'validity claims' (i.e. the claims raised in speech acts such as truth, rightness, sincerity, comprehensibility). It thus has a 'rational basis' which Habermas approaches through a consideration of communicative rationality (207-208; ibid).

Communicative action seeks to leave the paradigm of measurable success altogether and, therefore, the data-to-action paradigm. It redefines a new kind of success, marked by cooperation and understanding between actors, and agreement that is based in back-and-forth dialogue. We typically define validity as a kind of truth, for instance, between a remotely-sensed data point and an on-the-ground measurement. Validity for Habermas, however, looks toward different marker: that of agreement between actors, exploration of what it means for something to be true (or, for instance, what it means to have reached a *societal good*). For instance, Habermas might advocate that, rather than accepting that remote sensing leads to some kind of action and subsequent societal good, we ought to begin a dialogue about this taken-for-granted rhetoric. What is this action we hope to achieve, how does it lead to some kind of good, and how do we collective define that good? How do we know when we've reached it?

Before we get to this discussion, however, there are other terms that we ought not take for granted: terms like *conservation* and *security*; like *preservation* and *extraction*. We must understand these terms in their historical context, before we can move forward in our reconstruction of the data-to-action paradigm. In Chapter 2, I will explore the ways in which our concept of environmental conservation is shaped by a confluence of multiple, conflicting ideologies.

Chapter 2:
Landsat in the Context(s) of Security and Conservation

In his book *Expanding Hermeneutics*[35], philosopher Don Ihde argues, among other things, that we can and should interpret technoscientific practice as we might a work of literature or a religious text. Ihde writes: “that science is culturally embedded and that science as [an] institution functions according to more or less well-understood social structuring would be denied only by an Enlightenment ‘reactionary’ today,” says Ihde (4; 1998).

> I argue that much of science praxis is functionally hermeneutic, but only understandably so if certain modifications are made [...] to the understanding of science as an embodied *technoscience*, the instrument-embodied science of the contemporary world. I try to isolate the features of visual hermeneutics which operates as the ‘proof’ and the demonstration which supports production and construction of scientific knowledge (ibid).

For instance, the Landsat satellite system is an extension of our own senses: a way of gathering information about the world that we can’t do with our bodies. Therefore, we create devices like Landsat to gather that knowledge for us. Landsat *embodies*, to use Ihde’s word, our data-gathering senses and sensibilities. Contextually, Ihde might argue that it can appear as though Landsat’s imagery is the “‘proof’ and the demonstration which supports production and construction of scientific knowledge” (4; 1998). However, that is the trick of the imagery. That we sometimes take Landsat’s imagery for granted as proof of its own context — or somehow explanatory of its use and usefulness — is a

[35] A quick and dirty definition of *hermeneutics*: the philosophy of meaning. My committee member, Dr. Robert Rosenberger, says hermeneutics is not for those situations in which things seem to have an easy, agreed-upon meaning. Rather, hermeneutics is for those situations in which we must interpret and clarify some contested or unclear meaning.

function of the data-to-action paradigm in which Landsat exists. However, to fully understand where Landsat's imagery comes from — and what it does in the world we must explore the narratives that brought its imagery and instrumentation into being. And the narratives behind the Landsat program lie within a larger history of conservation and national security in the United States. Therefore, in this chapter, I will explore, hermeneutically, the ways in which Landsat is *grounded* in these various communities environmental research.

As the United States' first natural resource management satellite, Landsat's primary purpose is to monitor earth systems and resources, and to give actionable, quantifiable information on those systems and resources. Therefore, we can say that Landsat is situated, more broadly, within the tradition of natural resource management. I will provide a *brief* history of natural resource management in the United States, and then I will situate Landsat within that context. Satellites more generally — and Landsat specifically — are also situated within the tradition of *aerial reconnaissance* and narratives of natural security. I showed you a specific instance of this in the previous chapter. However, it is not just Landsat that finds itself embedded the context of aerial reconnaissance. All Earth remote sensing technologies owe their roots to military instrumentation. For this reason, I will also delve further into national security narratives, and the ideologies behind them.

I will end Chapter 2 by exploring, in philosophical terms, the nature of Landsat as a multi-use instrument and dataset: one that spans fields whose values can diverge, and one situated within divergent narratives more generally. Theorists like Bruno Latour and

Vandana Shiva have written extensively on technoscience that does — or does not — hold within it multiple, conflicting values. As the Landsat program is utilized toward a variety of different political ends, I believe that Latour and Shiva offer important lenses through which to discuss it.

Landsat and Natural Resource Management

Conservation narratives comprise values of both preservation and extraction. This becomes clear as we review a general history of conservation practices in the United States. As with Chapter 1, the goal is not to provide a complete history of conservation, nor the kind of fine-grained and nuanced history that a true historian of ecology could provide. Rather, I aim to shed some *further* light on the ways in which conservation narratives in the United States comprise conflicting values, and the ways in which Landsat is situated within those conflicting values. The late Samuel P. Hays, a famous environmental historian, had this to say about the divergent values within conservation narratives:

> Conservation, so its partisans believed, was a new way of looking at all public problems. The American people, convinced of this, would demand with a single voice that its principles be adopted. Yet, conservation in practice meant vastly different things to different people. The movement's unity, as exhibited by [...] intense emotional fervor [...], proved to be false, a religious enthusiasm directed against certain federal policies and officials rather than a common support for agreed-upon positive measures. As concrete issues became clarified, diverse interests revealed this superficial unity and shattered the unified crusade into particularistic groups (Hays xx; 1959).

Hays' words are a call to think more critically about natural resource management, and thusly of conservation, that work through the technologies of remote sensing. Aims of natural resource management were never clear or unentangled to begin with, and contain within them deep political conflicts. These conflicts find their roots within the history of conservation and natural resource management.

The concept of *conservation* in the United States didn't always exist. It was brought about by drought in the Western portion of the country. More specifically, natural resource management within the U.S. came about to address water shortages in the United States' most arid climates. Hays says that resource management and conservation finds its roots in a quest "to construct reservoirs to conserve spring flood waters for use later in the dry season" (5; 1959). This is where the term conservation comes from and, for that reason, it is intimately connected with the concept of natural resource management. Initially, irrigation was under the purview of private companies. However, after these companies found that irrigation in the Western United States wasn't an exciting and profitable enterprise, this region had no choice but to turn to the government for assistance. Initially, this was not an idea that the U.S. government was keen on supporting. However, after Theodore Roosevelt took office, that changed. Roosevelt was not an expert on the more wonkish aspects of land and resource management. However, these issues became his pet project, and they were quite compatible with his open adoration for an idealized American *great outdoors*.

Once Roosevelt's administration took up issues of conservation and resource management, however, they revealed a truth about the nature of such issues: that questions about resource management prompt more questions than answers. For instance, the system in place for water management and use, at the time, was as follows: a person would collect a certain amount of water from a stream. Then, that person would file a claim with the county in which they lived, saying, *Yes, I've collected this this amount of water*. And in theory, this amount was theirs' for days, weeks, and years to come. Hays points out that this was not a sustainable system. If I'm allowed to claim x gallons of water — and I realize that this claim will also affect any future claims I might make — my instinct will be to claim as much water as possible. For this reason, there wasn't enough water for the volume of claims within this system. And people in the Western regions of the country would fight over this limited resource, sometimes even suing each other over these county-filed claims.

On top of this, there was another issue too: that of watersheds and streams, and the counties — or even states — they belong to. Bodies of water aren't neatly or evenly distributed across county boundaries. Therefore, in addition to an unsustainable slew of legal disputes, there were also issues of measurement and standardization at play (or lack thereof). Consider this: I might live on the border of California and Arizona. The closest stream is in Arizona, but the system of county claims forces me to continue to draw water from California. Suppose also that it would be more efficient for the water supply itself if I collected my water from Arizona. In these instances, says Hays, "the transfer could rarely be accomplished" (18; 1959). Therefore, formal categorization of water supplies by state entities commenced.

You may wonder how the United States made its transition from cataloguing water to cataloguing land and forest. Hays says that we can attribute the "fusion of land and water policies" (23) to the Roosevelt administration:

> Western irrigators pioneered in the theory that watershed vegetation directly affected their water supply. Forests, they argued, absorbed rainfall, retarded stream runoff, and

> increased the level of ground water; forests retarded snow melting in the early months of the year, reduced spring floods, and waved water for summer use when supplies ran low; forests retarded soil erosion and silting in irrigation ditches and reservoirs. Private power and water supply corporations, as well as municipal water departments, joined with irrigators in presenting these arguments. They opposed commercial use of the watersheds; they fought to prevent lumbering in the forests and grazing on the mountain ranges. They centered their fire especially on sheep, which cropped vegetation close to the ground and, they argued, vastly accelerated erosion. [...] These views prevailed especially in California where steep slopes and torrential rainfall created acute flood and erosion problems. Urbanites concerned with water supplies and irrigators led in demanding public action to protect forest cover (23-24; 1959).

In other words, the water supply issue wasn't just a water supply issue: it was deeply entangled with land-based issues and with forest-based issues, and the ways in which terrain and forest held and preserved ground water after rainfall and snowfall. If you are in the Remote Sensing field, you might compare the entanglements of earth systems within the history of conservation to those widely recognized as objects of study for earth remote sensing satellites via the Bretherton diagram.[36]

The Bretherton diagram represents all of the different kinds of earth systems that earth remote sensing satellites — like Landsat — can measure. Furthermore, it maps the *entanglements* of these systems, however imperfectly. Just as conservation (and mapping and cataloguing) of water supply is deeply entangled with conservation (and mapping and cataloguing) of forest and terrain, so too are the measurements of ocean and atmospheric

[36] For the full Bretherton Diagram, visit: https://serc.carleton.edu/details/images/459.html.

dynamics (and so too is water entangled with the measurement of other important cycles and biogeochemical processes).[37]

Hays implicitly draws connections between the physical terrain of earth, and the human and animal use and treatment of that terrain. For instance, conservation advocates who cared about the preservation of water supply and the keeping-around of groundwater were opposed to lumber industries and logging in areas of water scarcity. They did not necessarily oppose these pursuits out of a specific obsession with trees — rather, they wished to preserve trees that preserved water (27; 1959). Additionally, there were further entanglements within the topic of land erosion and grazing. Water supply activists were frustrated that free-grazing sheep exacerbated erosion processes. However, grazing was also an important issue for the livestock industry. At this point in time, large plots of public land were available for grazing. These popular lands, free to any cattleman, had a limited supply of grazing material. And so, competition between cattlemen to have their cattle graze upon the public lands was fierce, and occasionally murderous — with both person and cattle casualties (50; 1959). This fight was about more than a limited public natural resource: it was about livelihood for farmers in the beef industry.

There was also the issue of *maintenance* of these public grazing lands. As they were so popular, weeds would often spring up in place of grazeable grass. “Under such

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[37] Note that the Bretherton diagram was also an important way to map relationality between the different disciplines and subdisciplines that studied these entangled biogeochemical cycles. In the latter parts of this dissertation, I will create a Bretherton diagram of my own, but one that expands on the relationalities of *sociotechnical* processes, and one that sees STEM and humanistic disciplines as entangled within these processes. To read more, see:

Center for International Earth Science Information Network. (2013, March 4). A Road Map Toward Better Understanding of Global Environmental Change. Retrieved June 20, 2018, from http://blogs.ei.columbia.edu/2013/03/04/a-road-map-towards-better-understanding-of-global-environmental-change/.

conditions," says Hays, "the quality and quantity of available forage rapidly decreased; vigorous perennials gave way to annuals and annuals to weeds" (51; 1959). The issue of water supply is deeply connected to the issues of forest maintenance and use, grazing land maintenance and use, and entanglements between public and private use of — and responsibility for — natural resources. Livelihood, too, is entangled with all of these issues. All of these entanglements are best summed up by Fikret Berkes, a distinguished scholar in the field of community-based resource management. "The social systems involved in conservation are multilevel," says Berkes,

> with institutions at various levels of organization from local to international. Processes at these levels require different but overlapping sets of concepts and principles [...]. Because each level of a scale is different, the perspective from each level is likely also different. [...] This difference does not mean one perspective is right and the other wrong; it means they can both be correct from different points of view. Pluralism in perspectives is mirrored [by] pluralism in knowledge. In conservation disputes, local knowledge may often appear at odds with science. But in many cases, the differences in knowledge and understanding of a resource system have to do with differences in the level at which information is obtained (15188; 2007).

In other words, pluralism in perspectives is part and parcel of conservation and resource management. Furthermore, this pluralism — caused by so many systemic entanglements and relationalities — is what often gives way to conflicting values, and the systems of knowledge that encompass them ("Pluralism in perspectives is mirrored [by] pluralism in knowledge"). This is a useful framework when thinking about Landsat, and all of the seemingly-conflicting political end goals that it encompasses.

Consider geology. "Satellite imagery," say Zall and Michael, "has contributed significantly to many programs requiring regional geological information. The initial use of satellite imagery in the first phase of a program has, in many cases, cut costs by decreasing the time and effort needed to focus and define subsequent more detailed phases" (136; 1980). They go on to list the ways in which Landsat data helps geologists: cost-effective, better than "mosaicking" many aerial photos together, more accurate and updated than many topographic maps, and better at helping us understand hydrologic networks. "Landsat imagery portrays this information in a geometrically correct image format, and it often can be relied upon to correct misleading information shown on these maps" (ibid).

Specifically, Hall and Michael refer to a mapping project in Houston, TX. The purpose of this project was to map lineaments: to study "[s]tructures situated on the surface traces of [...] faults," which could potentially "suffer foundation damage, and water and sewer [line disruption]" (143-144). Lineaments are lines on terrain that are difficult to spot from the ground, but that can be seen easily with the help of aerial photographs or satellite imagery. These lines are often expressions of important geologic features such as fault lines. For this reason, studying lineaments with Landsat imagery — or other Earth remote sensing data — is an easy and inexpensive way to see, for instance, if a bridge or a building, potential or current, could be sitting on a fault line.

Consider, also, a 2009 article by Akhir and Abdullah. They are using the Landsat Thematic Mapper, an enhanced version of Landsat data, to map lineaments as well. Studying lineaments this way, they say, "may be of economic importance to the area" (1; 2009). "Lineament mapping and analyses from satellite data [...] can provide a new, rapid and stimulating overview for regional structural (lineament) study for mineral exploration, locating of new mineral deposits and planning the development of the selected area" (ibid). In other words, fault lines can be mapped using Landsat data for the purposes of disaster prevention, and for exploration of natural resources like minerals, oil, and gas. It's also worth noting that, although there is not scientific consensus on the issue, there appear to be clear links between drilling activities and earthquakes, both direct and indirect (Hornbach et. al. 2015; Rubinstein and Mahani 2015; Ellsworth 2013).[38]

Famed geologist and petroleum engineer — and wildcatter[39] — Michel T. Halbouty[40] wrote multiple articles about the importance of Landsat data to geologic exploration. "Explorationists have long recognized that certain geological and ecological features peripheral to many ore deposits or oil and gas fields are sometimes detectable on normal aerial photographs," begins Halbouty in his article, *The Impact of Landsat Imagery on Scientific and Technical Orientation*.

> When Landsat images became available explorationists immediately recognized that, by adopting remote sensing methods to the images, such features were more pronounced - the conclusion being that, the higher the image is taken, the more visible the features - or *'the higher we go, the deeper we see.'* It then became obvious that such features as subtle discoloration of vegetation (botanical color anomalies), a reddish discoloration of the rocks surrounding large copper deposits, and intersections of large faults or structures in the earth's surface are often indicative that certain types of natural resource deposits exist in the subject area of study. *From these observations it became obvious that properly interpreted data from Landsat images could save corporations and countries hundreds of millions of dollars in unnecessary exploration and development efforts and, at the same time, provide important geologic clues which could lead to the discovery of substantial reserves.* Furthermore, the more the Landsat data are implemented, the more innovations

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[38] There is notable discord between the USGS page on earthquakes (which references the Rubinstein and Mahani article) and the Ellsworth and Hornbach et. al. pieces. However, it is not the task of this dissertation to pick apart these nuances.

[39] A *wildcatter* is a rogue petroleum geologist of sorts: someone who drills oil wells, but in places not known to be oil fields.

[40] A New York Times obituary calls Halbouty "legendary." "[K]nown in oil patches and boardrooms as Mike," the obituary reads, "[he] seemed an oilman from central casting with his silver mustache and suave speech. Drilling for oil, he said, was 'a heartache business,' and his hundreds of dry holes and at least two personal bankruptcies proved it. But he struck black gold often enough to be a millionaire many times over, he bought and sold banks, and he advised presidents. He headed Ronald Reagan's energy task force in his first campaign and was chairman of George H.W. Bush's presidential library" (Martin 2004).

> and expertise in their use will be established which, in turn, will generate greater accuracy and rewards (22-23, italics are my own).

This quotation mirrors the ideas expressed by Hall and Michael: Landsat allows us to view features from above in ways that are indispensable to geologists. Of course, geologists don't only attend to natural disasters or preservation of underground water supplies; they also sometimes work for major oil companies. And to oil companies, Landsat data is also indispensable. Note also, from Halbouty's quotation, that vegetation-focused analyses of Landsat data can be just as useful to those in search of oil as those interested in the ecological upkeep of healthy plants and trees.

Halbouty enumerates further ways in which Landsat data is useful to exploratory geology. They include: "possible detection of very subtle tonal anomalies that may represent alteration of the soils resulting from miniseeps of gas from hydrocarbon reserves"; "potential for detecting natural marine oil seeps with consequent improvement in efficiency of offshore exploration"; [d]etection of outcrops of important minerals and metals, especially in hostile environments"; "the monitoring of oil-field development and transport facilities, such as the Alaska pipeline, and an assessment of this development on the environment"; "the potential for improved communication and decision-making with petroleum companies," among others (23; 1978). The benefit of earth remote sensing data to petroleum geologists cannot be overstated: "It is estimated," says Halbouty, "that potential rewards for the use of Landsat data are in the billions of dollars annual. In some instances, the use of Landsat data has cut costs to 1/10th of those previous methods" (29; ibid). In another article, *Application of Landsat Imagery to Petroleum and Mineral*

Exploration, Halbouty argues that, “without remote-sensing data, the cost of a man-conducted ground survey to establish a resource target would be very high”:

> [...] each regular standard spacecraft image covers an area of 13,225 sq mi (34,385 sq km) — 115 by 115 mi (185 by 185 kim) — and the total cost for this image is only $3. Conceivably this $3-image might be responsible for finding hundreds of millions of dollars of energy or mineral resources (746; 1967).

We often assume that *conservation* is directly in conflict with these kinds of statements. However, as this history tells us, conservation encompasses both the preservation and extraction of natural resources.

National Security, Conservation, Multistabilities, and the *Data-to-Action* Paradigm

In the previous section, I outlined the ways in which Landsat is embedded in conservation narratives. Conservation narratives aren’t united but, rather, carry within them a plurality of conflicting values (and are *multistable*). In this section, I’ll outline the ways in which Landsat has been — and will continue to be — a tool of national security as well. In the process of doing so, I hope to show you that Landsat both (1) perpetuates already-existing narratives of national security and militarization within technoscientific practices and (2) blurs the boundaries between military and civilian technoscience. Ultimately, I also hope to convince you that land and resource management itself comprises within it interconnectedness between narratives that we assume are conflicting. As with the previous section, this one is not meant to be an exhaustive review of national security narratives within technoscience, or of the connections between national security and land and resource management. Rather, it is meant to bring overviews of these topics into conversation with each other — and into conservation with the world of land remote sensing and Landsat — because I feel these cross-conversations are currently non-existent.

Furthermore, I hope to show you that Landsat — and other Earth Remote Sensing technologies — hold entangled multistabilities within them. That is, ecological stabilities, military stabilities, and extraction stabilities are possible through Landsat, and through Earth Remote Sensing technologies. However, when we make one stability prominent (i.e. using Landsat data to study forest degradation in the name of the ecological stability), we are also bringing forward possibilities for military and extraction stabilities. I hope to continue to show you the nature of these entanglements in this chapter.

But first, I must clarify what I mean when I invoke the concept of *national security*. Just as it isn't always clear what we mean when we say *natural resources management* or *conservation*, security, too can have an ambiguous definition. Furthermore, I want to be specific about the kinds of activities involved in *security* or *national security*, so that I can make clear connections between those activities and the activities of the Landsat satellite system. To be clear, I do not draw equivalence between the concept of *security* and the concept of *conservation*. I don't think it's appropriate to define *security's* historical context, for instance, because that context is too broad to explore in a single project (let alone a single chapter). Therefore, as I attempt to answer the question *What is security*?, I will do so philosophically.

Political theorist Jef Huysmans, in his 1998 article *Security! What Do you Mean? From Concept to Thick Signifier*, parses a definition of security within the context of international relations. Thinking through security in terms of its status as a thick signifier, he argues, can help us understand it to a fuller extent. *Signifier* is a term from semiology, or the study of signs and symbols. A signifier is a medium through which we understand a specific semantic meaning of a word — a word, in other words, which may have different meanings in different contexts.

Let's briefly consider the sentence *There's a cat in the box*. When I say, "I just bought a few books from Amazon, and now there's a cat sitting in the box that they arrived in," this brings to mind a very different image than, "There's a cat in the box, and it's dead." The words surrounding the basic phrase "There's a cat in the box" tell you whether to think of a cuddly animal in a hurriedly-opened cardboard box, or to be mournful. These are very different kinds of situations in which we might imagine cats in boxes, the cat in the box being the signified and the words around that phrase — indicating its context — being the signifiers. For Huysmans, the term *security* itself is not a singular kind of signifier. Rather, it does different things in different contexts, in addition to imbuing contexts with its own unique array of meanings that only the term *security* can invoke. This is the *thickness* of security as a signifier (229; 1998). "In principle," says Huysmans, "there is no limit to definitions of security":

> Since its primary function is to identify what one is going to analyse, in principle it is an autonomous act on the part of scholars to fix the topic of their research. Nevertheless, in practice the definition of security is to a considerable degree determined by the community within which they work. If they want to be heard, the utterance should be recognized as security talk by the research community (the peer group), and the definition of security obviously plays an important role in this game of recognition. For example, the community of security experts in International Relations does not generally recognize security questions which are defined in terms of an inner condition of being at peace with oneself (230; 1998).

Here, Huysmans quips that those who are talking the talk of national security will likely not be speaking of "being at peace with oneself." In this quip, there is a serious realization: while *security* may be ambiguous, we often do not use the word when we are speaking of peaceful relations between communities, states, countries, etc. Security, therefore, is not totally ambiguous; it does conjure a specific set of meanings, which are important for us to be explicit about when we approach it. Furthermore, to Huysmans, the "common denominator" (231; 1998) of security talk is that it centers around a fear of our international neighbors, and their ability to do harm to us. "The fear of death in security stories is thus a double fear," says Huysmans:

> First of all, it is a fear of other people who have the power to kill. So it is a fear to die biologically by the hand of other people. But it also connotes a fear of uncertainty, of an undetermined condition. More generally this is a fear of the unknown which is constituted by the limits of reflexivity. Thus, the fear of death is also an epistemological fear — a fear of not knowing (235; 1998).

In other words, security is about surveilling our environment, gathering knowledge about potential threats to our lives and, therefore, potential causes of death. Consider our geologist, mapping earthquake threats by viewing lineaments from above with Landsat data. This geologist is participating in an activity of security, in that they are gathering knowledge about potential threats through a type of surveillance.

Huysmans also contemplates another way in which *security* signifies: by mediating "between the self and the concretized danger — [hereby performing] a double function": this "double function" is at once a "strategy [...] [in which we develop] [...] counter-measures to danger in the hope of postponing death" (237; 1998) which, in turn, creates a "balance of power system" within international relations (238; 1998).

This opens up space for "threat construction" and "threat management," as termed by Huysmans, to perpetuate each other. In other words, our identities within the national security system rely on our continued identification of potential threats outside of us, and our ability to manage them. However, our political identity is also based upon the identification and management of outside political identities. This “[articulates] a paradox in security policy.” says Huysmans. “[O]ur political identity relies on the threatening force of the other; nevertheless security policy aims ideally at eliminating this threat; [however,] if the threat were really eliminated, the political identity would be damaged and, depending on how strongly it relies on the threat, it may very well collapse” (239; 1998). For this reason, our job within national security is self-perpetuating; never complete.

I would like to draw a similarity, here, between national security and conservation narratives. Both involve a tenuous relationship between preservation of a type of order. In the case of national security, this means an order (perhaps a hierarchy) of sovereign nations with varying degrees of power, and varying incentives to maintain (or disavow) the order of that structure. In the case of conservation, this means identifying a stasis at which we can preserve and extract resources sustainably, or at a consistent level in which the most human and non-human actors can attain the most benefit (or so we’ve calculated). Unless, of course, those human or non-human actors become threats to that stasis (in the case of invasive species that eat crops, for instance). In both cases, there is an order that must be maintained, and threats to that order that must be identified and quashed.

Consider, also, the climate scientist, whose tenuous and uncertain order involves threats from an angry and unbalanced earth, surveilling rising sea levels to manage potential threat to the lives of human and non-human animals. Consider the petroleum geologist, whose threat is a world without fuel, in which transportation becomes stagnant and there are few to no sources of energy. A lack of certain kinds of resources, in all of these cases, presents a threat to both our survival *and* to a certain order of nations — a threat to our stasis of political power, too.[41] Conservation stabilities (which contain within them ecological and extractive stabilities) and national defense stabilities are — and were always — entangled. Any attempts to separate them without dealing with this nuance, rhetorically or otherwise, are examples of Latourian purifications (see pgs. 9-10 in the Introduction for more specific definitions). Additionally, I'll put forth the idea that the political identity of the researcher — the environmentalist who cares about preservation or the petroleum geologist who cares about energy — relies on the construction of a certain kind of threat. However, that thread has always been entangled with other identities, and the construction of other threats. It is only our acts of purification — our attempts to negate those hybrids — that create the illusion the the construction of these threats is separate, or comes from separate frameworks. To that end, the purified ways in which these various threats are constructed continues to define — and separate — the continued activities of the research at hand.

We can even see these languages meeting again, coalescing, in a January 2019 report by the Department of Defense. On January 18th, 2019, the Military Times released an article summarizing this report, entitled "DoD: Majority of mission-critical bases face climate change threats." The report, released by the Pentagon days before the Military

[41] A point brought forward by Dr. Randolph Wynne, another committee member.

Times report, explores the ways in which climate change-related natural events are affecting military bases across the country — and the world. The Pentagon selected 79 of its hundreds of locations, "which included Army, Air Force and Navy installations — and notably no Marine Corps bases" (Copp 2018). The study noted that "53 of the 79 [installations] faced current threats from flooding; 43 of the 79 face current threats from drought and 36 of the 79 faced current threats from wildfires" (ibid). The DoD concluded that climate change is an issues of national security, "with potential impacts to Department of Defense missions, operational plans, and installations" (ibid).

The most surprising part about this report, I'd argue, is that it's not surprising at all. The language of security threat is a highly usual rhetoric when we discuss matters pertaining to climate change. Consider the Guardian article *Your brain on climate change: why the threat produces apathy, not action* (Harman 2014); the report by the Pew Research Center entitled *Climate Change Seen as Top Global Threat* (Carle 2015); the very recent article in the Economist about *Climate change and the threat to companies* (n.a. 2019); the *THREATS: Effects of Climate Change* section of the World Wildlife Foundation (all the way across the page, unfortunately, from the *ADOPT A POLAR BEAR* section), which urges us that "Climate change poses a fundamental threat to the places, species, and [...] livelihoods [...]. To adequately address this crisis we must urgently reduce carbon pollution and prepare for the consequences of global warming, which we are already experiencing."[42]

[42] Source: https://www.worldwildlife.org/threats/effects-of-climate-change

Not to mention, there is the United Nations Forum on Climate Change (UNFCC) report, *Climate Change Poses Increasing Risks to Global Stability*, which warns that "[a]s the clearly visible impacts of climate change accelerate around the globe, including rapidly receding Arctic sea ice, drought in the Middle East and a major heatwave in Australia, climate change is emerging as one of the biggest security threats, if not the biggest" (UNFCC 2017). Here, even a cursory glance over titles and brief articles shows us the deeply intertwined natures of security narratives and conservation; defense and climate change. This rhetoric of *threat*, just like the rhetoric of *original sin*, serves to keep us in this data-to-action paradigm.

Landsat and National Security

In the last chapter, I argued that Landsat's military history plays into the *data-to-action* paradigm in which it sits. I reasoned that this was because the battlefield surveillance technologies upon which Landsat was built (literally, with those technologies' spare materials!) extend a data-to-action paradigm, in which threats are assessed, and an automatic response of weaponry is immediately enacted. This automatic response, a response that appears to collapse the space between data and action, has become taken-for-granted within Earth Remote Sensing. Therefore, we must revisit the particular military history of Landsat to become reacquainted with this data-to-action paradigm, and with how and why it manifests.

I have also shown you that this *data-to-action* paradigm exists not only within the history of Landsat's instrumentation, but within the ethos of the national security narratives and stabilities. These stabilities deeply interconnected with narratives in conservation and in environmental change. To be sure, once we make specific the nature of this security stability, we can more easily spot it in coming forward when we address issues of conservation, and even of climate change. As Earth Remote Sensing is a way by which we *extend our senses* to explore issues of conservation (and sometimes of climate change).

My argument is not entirely ground-breaking, but it does somewhat contradict other histories of Landsat. Historian of Science and Technology (and STS-er) Pamela Mack has written one of the few *external* histories of Landsat to-date.[43] And in this history, she notes that

> Although none of [the] early military reconnaissance technology was directly used in the Landsat project, it is harder to determine whether more recent military technology was transferred [...] What evidence is available suggests that the Pentagon had only a minor interest in the two kinds of sensors launched on Landsat in 1971: scanners and television cameras. Infrared scanning and multispectral imaging were introduced onto reconnaissance satellites in 1966, but these were systems designed only for coarse-resolution surveys of large areas. [...] In general, the Department of Defense had only limited interest in those types of sensor technology used in Landsat because they provided only coarse resolution (35-36; 1990).

Furthermore, Mack continues on to say that the DoD actually feared a more general adoption of Earth Remote Sensing for civilian purposes, as a more openly-available surveillance of Earth is potentially advantageous to those that the U.S. defines as enemies (37). While I don't disagree with the history itself, I surmise that taking these points at face-value is an act of purification. I also believe they are an act of purification, of sorts. First of all, I will acknoweldge that the differences between Landsat and other

[43] Joanne Gabrynowicz, a historian of space policy, has also written histories of Landsat. However, these histories focus on its transitions between public and private ownership and management. They are incredibly interesting, but not the focus of this dissertation.

finer-resolution, DoD-specific satellites (which produce imagery referred to as Very High Resolution, or VHR, imagery) are real.

I question the legitimacy, though, of an argument that takes only this distinction at face-value as well. The technologies themselves, regardless of resolution, still use a combination of spectral bands to view imagery in certain ways, opening up space for us to observe things we usually can't.[44] There are some other, more specific technological similarities too. For instance, Landsat, like many militarily-focused satellites, orbits the Earth in a near-polar manner. This kind of orbit is incredibly desirable from a surveillance perspective, as it allows provides a satellite with "the potential to observe any point on the planet's surface" (Belward et. al. 117; 2015).

There is a wealth of articles, beyond what I have time to meaningfully address in this dissertation, that outline the ways in which Landsat data (and Earth Remote Sensing data more generally) is useful to activities of national security and surveillance.[45] Many in the Remote Sensing community will not be at all surprised to read this, as Landsat's military usefulness is not lost on the broader Remote Sensing community. Alan Belward and Jon Skøien, two environmental researchers who use Landsat data, note this in their 2015 article *Who launched what, when, and why; trends in global land-cover observation*

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[44] In many ways, this is not that different from some of the boundary work we do within STS, for instance. It brings to mind instances in which we say that analytic and continental philosophy are different, or that an ethnography of a drone community is not at all like a philosophical exploration of the ways in which drones extend certain sets of values. Are they stylistically different? Yes, of course. But do they both engage with sets of literatures that seek to clarify terms or patterns or relationships, and that value a similar kind of knowledge creation? I would argue yes. I hope you follow my analogy.

[45] To find more sources, simply search for the keywords "Landsat" and "warfighting" using Google Scholar. The word *warfighting* is key. I didn't know this until Nick Copeland, Assistant Professor of Sociology at Virginia Tech, tipped me off to this keyword.

capacity from civilian earth observation satellites. "[D]ata from military systems are effectively inaccessible for civilian use," they say. "However, whilst military systems are not available to the civilian[,] the opposite does not apply, and some civilian/commercial systems are used by the military" (116; 2015). In their 1997 article *The Landsat program: its origins, evolution, and impacts*, Donald Lauer, Stanley Morain, and Vincent Salomonson count national security as one of the five major reasons for the development of the Landsat satellite system (832; 1997).[46] They note that

> [t]he United States Government maintains national security, which includes using data from civilian satellites to protect and defend the Nation against aggressors. It is no secret that defense/intelligence satellites are assets for maintaining national security. It is not as widely known, however, that the defense/intelligence community has always employed data from civilian satellite systems to carry out its security mission (ibid, of National Security Council 1989).

In other words, while there are some earth remote sensing satellites that are expressly for military use, civilian earth remote sensing data is often used for military reasons as well. Kass Green, a prominent member of the remote sensing community, and of the Landsat community particularly, notes that the type of moderate-resolution data that Landsat provides has, increasingly, been of particular use to the military since Desert Storm in the late 1990s. "Over the last 15 years," she says,

> the military use of Landsat has [...] increased. [...] Instead of focusing on specific sites in one or two large countries (USSR and China), reconnaissance must now be dispersed around the world, and new technologies required. Since Desert Storm, the military has recognized the usefulness of moderate resolution data to provide a wide synoptic view. In 1998[,] NASA contracted with Earthsat to create a world wide mosaic of Landsat imagery [...]. [...] As a result, the military has become one of the biggest users of Landsat imagery" (1150; 2006).

[46] The other four issues are: "the need for better information about Earth features"; "commercial opportunities"; "international cooperation"; "International law" (832; 1997). International cooperation and international law, however, seem like they could be quite tied up with issues of international security as well.

Desert Storm was the first instance in which we can draw clear bibliographic maps of the research that has been done with respect to Landsat and the military, aside from the instrumentation connections I have drawn in the previous chapter. Take Robert O. Work's Naval Postgraduate School thesis, *Toward a national space warfighting architecture: forging a framework for debate about space-based operational and tactical combat support*. Work articulates that

> Depending on the bands used, [Landsat] images can highlight important features of a region and provide a variety of information with commercial applications: soil water content, crop production, drought effects, etc. [...]. However, the images also have widespread military applications. According to one Deputy CINC of the US Space Command, LandSat [sic] has [...] paved the way for a revolution in amphibious and strike warfare. By identifying terrain characteristics [...] and camouflage techniques, the probability of success of special force and amphibious assault force missions or land warfare can be greatly increased (39; 1990).

In other words, the same kinds of instruments that make Landsat great for agriculture and resource management *also* make Landsat great for distinguishing land and vegetation from potential enemies.

I took up a criticism of one aspect of Mack's *Viewing the Earth: The Social Construction*[47] *of the Landsat Satellite System* a few pages ago. However, Mack draws this connection — that Earth Remote Sensing tools used for agricultural management and ecological preservation are often tools for war fighting and defense — in this history as it relates to NASA. In 1964, NASA geologist Peter Badgley initiated remote sensing projects (ultimately ones that would develop into Landsat instrumentation) across various universities. More specifically, he was interested in seeing if multispectral sensors

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[47] Back in 1990, it was a little less taboo in the STS discipline to say, without a large twinge of self-criticality, that technologies like Landsat were socially-constructed.

typically used for lunar observation would also be useful in crop identification and harvest-size prediction. This research was conducted at both the University of Michigan (this was the flight of the multispectral scanner on the zippy L-24 plane from Chapter 1) and at Purdue University, and was a continuation of research that had previously been conducted by the Department of Agriculture, but with aerial photography (47; 1990).

Badgley also funded similar studies for oil deposits, at University of Kansas, University of Michigan, and at the Ohio State University (48; ibid). This is another instance of the deep connections between Remote Sensing for defense and for conservation, and it relates to extraction of natural resources. And finally, NASA eventually contracted with the Environmental Research Institute at Michigan, or ERIM, known for its ecological *and* its military research (48-49; ibid). Mack explicitly traces these projects to Landsat lineage (51; ibid). I elaborated on this lineage quite extensively in the previous chapter.

Again, Work confirms Landsat's multi-functionality, or *multistabilities*. "In some cases," he says, "different tasks may be accomplished by a single space system that lends itself to several different functional applications":

> For example, the multi-spectral capability of LandSat [sic] lends itself to camouflage detection, terrain analysis, and near-shore obstacle identification. Moreover, while its [moderate] resolution makes it unsuitable for precise identification of targets, it is still useful as a surveillance tool to tip-off reconnaissance efforts when change is detected in a given area [...]. By listing all the tasks that a given payload can accomplish and associating it with the appropriate command action process function, warfighting commanders can better see the broad range of capabilities and cost effectiveness of space systems (165; 1990).

In addition to being cost-effective and having flexible usage, Work also discusses the ways in which Landsat is useful for reconnaissance in times of uncertainty. "At lower levels of war, where disorder and chaos are at their maximum, critical vulnerabilities are not always evident and uncertainty is a fact of life. Commanders are often forced to pursue lesser vulnerabilities until a path to the critical weakness is discovered" (169; ibid), he says. To effectively collect data in uncertain situations, the most effective tactic is to combine "satellite digital imagery [...] with LandSat [sic]" so that the "resulting composite 'gives a detailed portrait of any battlefield or beach anywhere on the globe' [Buenneke 38; 1990]. This type of seemingly mundane information offers considerable combat advantages. As Napoleon said, one should always avoid a field of battle reconnoitered and studied by the enemy [...]" (170; ibid).

Note that this kind of rhetoric is similar to what we've read about the ways in which Earth Remote Sensing aids in the extraction of natural resources, as explained by Halbouty: it's easier, cheaper, more efficient to view terrain from above. There are also similarities between the language of uncertainty invoked in Work's piece ("At lower levels of war, where disorder and chaos are at their maximum, critical vulnerabilities are not always evident and uncertainty is a fact of life"), and the language of uncertainty used by the Earth Remote Sensing community when addressing research goals. For instance, in Simmons et. al.'s *Observation and integrated Earth-system science: A roadmap for 2016-2025* — an extensive overview of imperatives for tracking earth systems over a decade — the authors fervently insist on a similar need to track evolving unknowns in uncertain times. "Much of the land surface is no longer in its natural state," say Simmons et. al., "and the chemical composition of the atmosphere and ocean is being changed in ways that change climate and affect life":

> [...] It has become imperative to monitor, understand[,] and where possible[,] predict many aspects of environmental variability and change. This requires that observations of sufficient quality, quantity[,] and regularity be made of the key variables of what has come to be called the Earth system. It also requires a capability to model with sufficient realism how these key Earth-system variables are likely to change in the future [...] (2039; 2016).

Earth Remote Sensing technologies fulfill a similar purpose for both groups: a source of data for uncertainties that could eventually become dire; a need to track and monitor for potential disaster, before it becomes actual disaster. We can connect this rhetoric back to Huysmans' clarification of security narratives, and the ways in which our ideas about security (and hence, technologies of security) sit within these crafted narratives of security threats to Earth, and to living creatures on it.

These narratives are not limited to multispectral scanning either. In *Hyperspectral Imagery: Warfighting Through a Different Set of Eyes*, Lieutenant Colonel Paul J. Pabich discusses the utility of *hyperspectral* imagery to both the Navy and the Airforce. To be clear, hyperspectral imagery is slightly different than multispectral imagery, and is becoming more and more popular for researchers who use remotely sensed imagery. Think of hyperspectral imagery as the discrete information to multispectral imagery's analog: multispectral bands can be anywhere on the sliding scale of the spectrum. Hyperspectral imagery, however, involves more discrete values: each material has something called a *spectral fingerprint*: it gives off a specific spectral signature when a satellite's signal interacts with it. Just like multispectral imagery, it's great at distinguishing, healthy vegetation from unhealthy vegetation, and also potential invaders from vegetation.

Pabich finds hyperspectral imagery useful in tracking underwater disasters, which include “currents, oil slicks, bottom type,” and “atmospheric visibility,” among others (6; 2002). “The utility to the Navy and the Marine Corps,” says Pabich, “comes from the ability to predict the littoral [underwater] environment with much better confidence[,] and to better prepare for amphibious assault” (ibid). The Air Force has some different uses for hyperspectral imagery: “the service is looking for [hyperspectral] systems that can provide the warfighter with near-real-time information to enable attacks on targets such as tanks hiding under trees”:

> The Air Force is also interested in knowing what *not* to attack, as in the case of decoys. Destroying decoys can be a costly proposition. At $21,000 per copy for the tail kits alone, Joint Direct Attack Munition precision-guided weapons should be zealously guarded from unwittingly attacking decoys. Since decoys will not have the same material composition as real targets, [hyperspectral imagery] should be able to differentiate between the two. By using HSI to discriminate between real and false targets[,] the U.S. can save money by avoiding bad targets and at the same time wisely expend precision munitions on true targets. Defeating camouflage is another particularly likely mission candidate for [hyperspectral imagery] because even though the camouflage may show up as the same color as the surrounding terrain in the visual spectrum, its material makeup will cause it to reflect very differently at other wavelengths (6-7; ibid).

Here, we can see similar narratives to the ones previous mentioned regarding Landsat data. Hyperspectral imagery, generally, allows researchers to distinguish between target materials, allows entities such as (but not limited to) the Navy, the Marine Corps, and the Air Force to save money by distinguishing between threats and non-threats from above. To this point, it is also not so unusual for military researchers to cross-reference hyperspectral or Very High Resolution (VHR) datasets with moderate resolution ones from satellites like Lansat. We can identify instances of this dynamic that are *specific* to Landsat. For instance, in *Battle Command Advanced Warfighting Experiments: Summary of February and March 1994 Experiments*, Steven B. Schorr notes that the Army Space

Command (ARSPACE) cross-references Landsat data with their own proprietary datasets to better understand terrain elevation (5; 1994). And in *Canopy-cover thematic-map generation for Military Map products using remote sensing data in inaccessible areas*, Chang et. al. "[compare] Landsat classification results with Military Map [a very high resolution, or VHR, and proprietary product], [so that] the coordinates of accurately classified pixels are acquired" (264; 2011).

Chang et. al. are using Landsat data to gather more information about canopy cover, or the extent to which the ground is covered by leafy trees. Ecologists use this kind of analysis to learn more about healthy vegetation. However, for Pabich and Chang et. al., hyperspectral and multispectral data, respectively, are useful for forest reconnaissance. I belabor this point about these connections between Landsat, hyperspectral, and VHR data because it is easy to dismiss Landsat as a coarser-resolution product that the DoD may have no interest in. The aforementioned examples show us that this dismissal isn't well-founded.

Belaboring the Point with a Specific Example

I will belabor the point some more, by noting that Landsat is no longer a strictly coarser-resolution product. There have been additional capabilities, like the Enhanced Thematic Mapper (ETM+), which has been around since Landsat 7's launch in 1999, and the Operational Land Imager and the Thermal Infrared Sensor, which have been in service since Landsat 8's launch in 2012. These instruments augment Landsat's moderate resolution data product. *Calibrated Landsat ETM+ Nonthermal-Band Image Mosaics of*

Afghanistan, by Philip A. Davis, explores features specific to Landsat 7 (which, by the way, is still in orbit).[48] ETM+ captures infrared light at wavelengths that we cannot see with our eyes — and a range of infrared light that Landsat satellites usually do not capture.

Infrared Remote Sensing is highly useful to researchers across many fields, and instruments that allow us to view infrared wavelengths have applications across national security (i.e. is there a person hiding in those trees?), agricultural (i.e. are these crops healthy?), ecological (i.e. is this vegetation healthy, and how has its health changed over time?), and extractive (i.e. how can we more clearly distinguish between minerals and vegetation from a bird's eye view?) realms. ETM+ also captures higher-resolution imagery than the standard Landsat instrument. Add-ons ilke ETM+ — and like the Landsat 8 Operational Land Imager (OLI) and Thermal Infrared Sensor (TIRS), described in footnote 45 — add power to research questions across conservation and defense needs.

Davis explores the ways in which this ETM+ instrument is useful for a variety of the aforementioned kinds of applications in Afghanistan. "In 2005," he says, " the U.S. Agency for International Development and the U.S. Trade and Development Agency contracted with the U.S. Geological Survey to perform assessments of the natural resources within Afghanistan."

> The assessments concentrate on the resources that are related to the economic development of that country. Therefore, assessments were initiated in oil and gas, coal, mineral resources, water resources, and earthquake hazards. All of these assessments

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[48] Landsat 8, the most up-to-date Landsat satellite, has the Operational Land Imager (OLI), as well as the Thermal Infrared Sensor (TIRS). Both instruments build upon the capabilities of the ETM+ instrument by adding additional spectral bands, and by enhancing the quality of the data overall.

> require geologic, structural, and topographic information through the country at a finer scale and better accuracy than that provided by existing maps, which were published in the 1970's by the Russians and Germans. The very rugged terrain in Afghanistan, the large scale of these assessments, and the terrorist threat in Afghanistan indicated that the best approach to provide the preliminary assessments was to use remotely sensed, satellite image data [...] (4; 2006).

There is some important political context behind this 2005 partnership between USAID, USTDA, and USGS to study terrain in Afghanistan. Terrorist attacks on September 11th, 2001, made Afghanistan and its then-Taliban-lead government of utmost political interest to the United States. The U.S. Department of State website notes that the United States has been militarily "engaged in Afghanistan since 2001." However, the relationship between the United States and Afghanistan had been more complicated than a simply combative one. This mirrors Huysmans' observations about the paradox of national security, as a simply combative orientation would not preserve the delicate symbiosis required to establish a U.S. political identity on the international stage relative to other countries' political identities, and vice versa. Therefore, it should follow that, after overthrowing the Taliban, the U.S. participated in vast funding and rebuilding efforts in Afghanistan. Here, we see ecological and military *stabilities* coming forward in another kind of entanglement.

The rebuilding efforts described in the Davis article — utilizing Landsat technologies — are direct manifestations of a May 23rd, 2005 treatise called the Joint Declaration of the United States-Afghanistan Strategic Partnership. This document specifies economic, political, and security-related collaborations between the two countries, all as part of the aforementioned rebuilding efforts. These are efforts that STS scholars Benjamin Sovacool and Saul Halfon refer to as *reconstructive* efforts. And although their specific case study focuses on Iraq, and not Afghanistan, Sovacool and Halfon describe a dynamic analogous to the one perpetuated by the Davis article (and one, I should add, that is also situated within the same Bush-era national security policies). "[T]he intervention in Iraq," they say, "is not simply a military conquest, but also remains a significant *development* project":

> And, as a development project, intervention in Iraq relies on a convergence of economic, social, and political justifications that expand beyond the confines of conventional notions of security. Condoleezza Rice almost admitted as much when she [...] stated that 'it is impossible to draw neat, clear lines between our security interests, our development efforts and our democratic ideals' (224; 2007).

The above described blurring between national security and international development further supports Huysmans' paradox. Furthermore, Sovacool and Halfon argue that countries like Iraq, in this instance, "[serve] as crucial [sites] for articulating an emerging discourse of reconstruction that [merge] security and development discourse in new and powerful ways," and that "the structure of this discourse helps explain why reconstruction continues to bring about 'instrument-effects' of violence and conflict" (225; 2007). When they use the term *discourse*, Sovacool and Halfon are describing narratives that have evolved out of particular historical contexts, and that have configured the world in particular ways (ibid). *Discourses* act like Huysmans' signifiers, and like the rhetoric I've described around defense and environmental research (*threat* construction). A national *reconstruction* discourse, therefore, frames our national security signifier which, in turn, frames issues of reconstruction. This symbiotic framing and reframing illustrates the term *instrument-effects*, which Sovacool and Halfon describe as the “latent and often counterintuitive effects that consequently extend and reproduce that discourse” (226; 2007).

Sovacool and Halfon elaborate that “discourses do not merely ‘float’ in the world”

> but are tied to specific actors and organisations, thus becoming institutionalised into practices and ways of reasoning. Such institutionalised discourses frequently function as knowledge industries — with their own organisations and experts — that generate coherent policy narratives and conceptualise, plan, and implement projects (226; 2007).

In the context of the Davis article, we can understand Sovacool and Halfon’s claim as follows: discourses of reconstruction efforts — ones that make and remake, and in-turn are made and remade, by international security narratives — are particular instances of knowledge-creation. Landsat ETM+ was used to create knowledge within the context of both international security and of reconstruction and, indeed, its instruments lend itself to doing so — were even created for such purposes. This example shows us that the institution of Landsat was formed by, and in turn forms, discourses on international security and on reconstruction. This example also extends my argument about Landsat and multistabilities. Here, the ecological stability and the military stability are entangled, coming forward in tandem. However, the nature of this entanglement, I think, is different from the ones presented in Chapter 1. While a deeper exploration of how reconstruction supports both multistabilities in a different kind of entanglement is not the task of this dissertation, I believe that it is a potential area of exploration for postphenomenologists.

Zooming Out, Zooming In

To use a visual metaphor, let's zoom out. Let's zoom out beyond our discussion of the connections between defense and conservation research — and even beyond the ways in which connections between those discourses contribute to a *data-to-action* paradigm. Then, let's zoom in again. This time, to more targeted discussions, apart from Earth Remote Sensing, about the ways in which biology and defense research have been intimately connected to quite some time. Consider Edmund Russell's *War and Nature*, which is closer to the themes discussed earlier in this chapter. In *War and Nature*, Russell discusses the complicated relationship between conservation and warfare within the context of pesticides. Pesticides, on one hand, are used to control invasive species and to preserve crops that we consider beneficial. On the other hand, the chemical compounds in pesticides were initially developed for use in chemical warfare (for instance, organophosphates).

Russell argues that this is no coincidence and that, indeed, the connections between pesticides and chemical weapons are *both* chemical *and* ideological, by way of this blurring of the boundaries between the civilian and the military. "We have often seen a distinction between war and the military, on one hand, and peace and civilian life, on the other," says historian of science Edmund Russell.

> As one observer put it, Americans in particular are inclined to see peace and war as two totally separate quanta. War is abnormal and peace is normal and returns us to the status quo ante! [...] [Rather], war and control of nature coevolved: the control of nature expanded the scale of war, and war expanded the scale on which people controlled nature. More specifically, the control of nature formed one root of total war, and total war helped expand the control of nature to the scale rued by modern environmentalists (2-3; 2001).

In other words, Russell may be addressing chemical warfare in particular, but this is all in service of revealing that control of vegetation, species, crops, biospheres, etc. in the ecological sense has deep ties to control of boundaries, territories, nationalities, etc. in the military sense. Russell's call for us to rethink our taken-for-granted distinctions between civilian and military technologies research remind me very much of the distinctions we often draw about Landsat, and the lines often drawn around its civilian nature.

In her book Who Wrote the Book of Life, Lily Kay outlines the ways in which the Cold War shaped genetic research in the 1950s. "Both qualitatively and quantitatively," says Kay, "the structure of patronage for molecular biology changed markedly as the Rockefeller Foundation eased out of the life sciences in the United States":

> During the 1950s, biologists, particularly geneticists, became firmly tied to the military establishment. [This research was] all subject to security clearance and the loyalty oath, as McCarthyism swept through federal institutions. Though not as deeply as the physical sciences, life sciences in the 1950s, too, were embedded within what Senator J. William Fulbright termed the *industrial-military-academic complex* and what Admiral Rickover critically labeled the *military-scientific complex*. The relationship altered the modus operandus, policies, and attitudes to research in the military and in science. [...] The web of military institutions sponsoring scientific research defined the conditions of possibility for the production of particular forms of knowledge (10-11; 2000).

Kay even makes mention of the curious separation, in some cases, between wartime research and the values of the researchers themselves. "I do not mean to suggest," she writes, "that all those who contributed to the genetic code supported the military consensus; indeed, Jacques Manod and Leo Szilard were political activists against the cold war" (8; ibid). In many cases, I think this speaks deeply to the issue of *original sin*, and possible defenses against involvement in military research by those in the Remote Sensing field. Kay's writing speaks to the sensitivity of the issue: the criticism is not of the individual, or even the research itself. Rather, it is important to reflect on this history as a means to understand "how military institutions [...] defined the conditions of possibility for the production of particular forms of knowledge" (11; ibid).

Conclusion

I bring Russell and Kay to your attentions to clarify the rest of this chapter: defense-funded research has shaped the ways in which we create knowledge — and the ways in which we understand and interpret that knowledge. At the beginning of this Chapter, I introduced Don Ihde's conception of hermeneutics, or *philosophy of meaning*. In subsequent pages, I have shown the ways in which we can read further *meaning* into the Landsat program. This hermeneutic analysis is different from the ways in which we usually contextualize Landsat. For instance, we often discuss Landsat's technical prowess, or its extensive, long-term data archive. A research project involving Landsat, we often say (in a variety of implicit and explicit ways), is one that uses its data to enhance our understanding of the world. How can we continue to use Landsat as a lens through which to view the world, though, when we haven't fully contextualized where that lens comes from, and the narratives from which it came?

This chapter seeks to make explicit just that: an exploration of Landsat's uses, accompanied by a context for the narratives that underlie those uses. The deliverable for this chapter, as with all chapters in this dissertation, comes in the form of questions rather than answers. What do defense and conservation stabilities — and their entanglements —

mean for the ways in which Landsat creates knowledge? And, now that we have a better understanding of the stabilities that have shaped the *data-to-action* paradigm, what can we do to continue to imbue more meaning into our data, our action, our notions of some preferred societal outcome, and *all* processes in between? We must begin to answer these questions if we are to shift into a different paradigm: one in which we emphasize clearer processes between data analysis and resultant desired outcomes. In Chapter 3, I will attempt to create a more specific framework for how we may begin to go about doing this.

Chapter 3:
How to Engage Landsat in STS Contexts, and STS in Landsat Contexts

In Chapter 1, I provided a history of Landsat's instrumentation, to illustrate connections between *original sin*, battlefield technologies, and the *data-to-action* paradigm. Then, in Chapter 2, I further contextualized this *data-to-action* paradigm within the larger narratives surrounding Landsat, and Earth Remote Sensing more generally: narratives of conservation and of national security. Furthermore, I provided some context for the ways in which conservation and security are deeply entangled. These analyses are meant to help us understand the ways in which the *data-to-action* paradigm in Earth Remote Sensing came to be and, therefore, they are intended as the very first step in a larger project. This larger project, I hope, is one that helps us clarify the processes between data analysis and some action desired action in light of said analysis.

However, in order to embark on that endeavor, a framework is required. What framework will we use to talk about the processes between data and action? This discussion will necessarily be facilitated by, and between, researchers from very different backgrounds: some from the humanities and social sciences, some from STEM fields, and perhaps even some from the arts. Engaged STS, a subfield of the interdisciplinary field Science, Technology, and Society (STS), offers a strong framework for such discussions. In the first part of this chapter, I will outline the mission of Engaged STS, and connect that mission to other, closely-related theoretical frameworks in STS and Philosophy. Then, I offer what I believe are some best ways to *begin* shifting the *data-to-action* paradigm, as put forth by these fields.

We begin this shifting process by way of a *diffractive* intervention. In other words, as I am reading these two fields through each other, I recognize that, by intervening in one, I will intervene into another (and vice versa). In my attempt to build an intervention into the *data-to-action* paradigm in Earth Remote Sensing, I am also attempting to intervene into the field of STS itself. Namely, I am intervening in something I call the *critique-to-action* paradigm. This is a phenomenon in which STS leaves a critique of a technoscientific field, and assumes that field will naturally (1) know how to interpret the critique across disciplines and (2) know how to build processes in between the critique and some desired action (presumably toward a more just technoscience). This paradigm is just as troubled as the *data-to-action* paradigm, in that both assume some specialized form of knowledge will automatically manifest in a generalized kind of intervention. Engaged STS, along with related theoretical frameworks, can set us on a path where we can make such *diffractive interventions*: where we can simultaneously address the issue of the data-to-action paradigm, and the critique-to-action paradigm, as these frameworks allow us to see them as reinforcements of each other.

Dipping Our Toes into the Theory

But before I embark on this theoretical exploration, I will define some terms for my readers from Remote Sensing, who will not be as familiar with some of the more philosophical definitions with which I'll be working. I'll begin, as one does, with a feminist philosophy of quantum physics. In such a volume, *Meeting the Universe Halfway*, Karen Barad puts forth the idea that ontologies (how things are, in reality), epistemologies (how knowledge about things is created), and ethics (the study of making choices that are moral, alongside an exploration of why certain choices may or may not be moral) are inseparable (26-33; 2007). This framework is key to this chapter. For this reason, I'll explore this theme, briefly, in the context(s) of Landsat.

Landsat takes pictures of Earth. These images represent something ontological, in that Landsat tells us something about how Earth *is*. These images are also epistemological, in that they are creating knowledge about the earth. The ethical implications are more difficult to suss out. In our data-to-action paradigm, they involve our murky notions of societal good. You may not have realized it, but the previous two chapters were dealing with these entanglements, just as they were dealing with the entanglements of military and the civilian; of conservation and security. When we ask what kinds of actions Landsat produces in the world, we are also asking questions about the entanglements of what the world consists of, what we know about that our world (and how we know it), and what kinds of decisions we should make. The connection between these three components — what's out there, how we know what's out there, and what we do about it — are not linear. Rather, ontologies, epistemologies, and ethics all feed into each other, always and all of the time. These entanglements underlie my call for a paradigm that transcends one in which we see data-to-action as a linear process.

I laid the groundwork for such discussions in my introductory chapter, as well as Chapter 2, with Habermas. I lead with Habermas because his work is abstract; generalizable. In other words, I believe he offers broader ways of thinking through all of these entanglements: of ontologies, epistemologies, and ethics; of conservation and security. Engaged STS, too, brings together what we know, how we know it, and how we act on what we know. Engaged STS tends to be site-specific; to make itself visible within particular fields of inquiry (i.e. a project focused on Landsat and remote sensing, or one that investigates how engineers know what they know). This makes sense, as the subfield seeks to make theoretical knowledge within STS inseparable from knowledge located in specific sites, and inseparable, therefore, from community participation as well. I began with Habermas because he isn't site-specific, and because I think other scholars, regardless of position or discipline, could draw from my use of Habermas. However, I am not a Habermassian in the strongest sense, nor is it my task to become a Habermas scholar. Therefore, serious students of the Frankfurt School should not look to my writings for theoretical expansion. However, I now aim to extend Habermassian ideals of dialogue and community engagement to STS, and specifically to a subfield called *Engaged STS*.

Origins of the Critique-to-Action Paradigm

Historically, two oversimplified views of STS have battled: (1) STS-ers as handmaidens of science: consultants that forward scientific fields for the better by helping scientists think through social processes that they do not address in their research, namely because they have better things to do; and (2) STS-ers as ruthless critics, deconstructing scientific practices which they do not entirely understand, making a critique only to then leave and let the scientific communities figure out solutions that work best for them. Neither of these views is fully salient. However, no STS-er can deny the grain of truth that exists within each of these stereotypes — and, subsequently, the tiny grain of each of these stereotypes that exists in all of our collective conscious. A little bit of each of these techniques can be helpful. However, out of a recognition of — and fight against — these tendencies to either over-assist or over-critique grows a third kind of STS: one in which we imagine projects as action-oriented; as instances of Engaged STS; as *making and doing*.

Making and doing offer what I will call a *diffractive ethic*.[49] A diffractive ethic is one in which we are members of our home field AND the scientific community we study. By proxy, we then bring the community we study into our field as well. Ultimately, a diffractive ethic and, therefore, a diffractive STS, allows us to see that Remote Sensing and STS suffer from a very similar kind of problem: the problem of translating research into *instrumental* action. Instrumental action, as defined by S&T policy scholar Yaron Ezrahi, is "action within which actors are held accountable in terms of the perceived consequences of their actions [which, in turn] is likely... to favor goals which actors can visibly advance" (16; 1990).

[49] This term is inspired by Dr. Jen Henderson's 2017 dissertation (also from the VT STS department, and the Remote Sensing IGEP), who used the terms *ethics of accuracy* and *ethics of care*, among others, to describe the ways in which forecasters made predictions about disastrous weather — and how they say those predictions through in the form of warnings to communities. I will explain this in further detail later in the chapter.

In other words, instrumental action is goal-oriented: it turns any set of complexities into a step-by-step, result-driven, procedural thing that enacts change in a given system. The institution of science, says Ezrahi, is one in which we favor goal-oriented action, and one in which we criticize modes of knowledge that do not result in goal-oriented, implementable action (16; 1990). However, this does not mean that all science — irrespective of whether or not we consider it to be *good* science — naturally leads us to goal-oriented action. In fact, some of the most practical, well-intentioned procedures can lead us away from that which is practical in reality. And indeed, my positioning in both Remote Sensing and STS has allowed me to see this dynamic, in practice, in both fields.

Remote Sensing practitioners do something STS-ers refer to as "normal science." According to philosopher and historian of science Thomas Kuhn, *normal science* "articulates the theories that a given paradigm supplies" (24; 1962) by working on small and highly specific issues. Researchers in the Remote Sensing field work on problems in normal science, at least partially, because they are aware of the complexity of the environmental factors they are studying. It is impossible to observe every aspect of an entire ecosystem as a whole (see: the flawed complexity of the Bretherton Diagram from the previous chapter). Therefore, small data projects are an act of humility *and* they are necessary; a sort of eating an elephant one bite at a time.[50]

I'll provide some examples of normal science specific to those from my Remote Sensing readership. Normal science can be the unsexy, everyday work of drawing

[50] Recently, a good friend was describing Analytic Philosophy to me in much the same way.

polygons around groups of pixels that look similar, scanning lines of code in R, Fortran, or Python for a cursed bug, or creating a cloud mask. Unsexy, of course, does not mean unimportant — far from it. What makes your normal science cloud masks so interesting to STS-ers is that they are exhaustive, tedious work on very small issues that, for all intents and purposes, seems unconnected to larger, paradigmatic issues like, "Is climate change real and how can we show that it is?"; "How can we better understand invasive species of insects that degrade forests in the United States?"; "How do we know that what we are seeing in a digital image corresponds to what we are seeing on the ground?"; even "How do we improve evapotranspiration modelling in the Amazonian Rainforest?"

Of course, the normal science is always, always, *always* (read: always) related to these bigger, paradigmatic issues. The clincher is: when and how do we lose sight of the connections between the big science questions (the ones we learned to ask when dreaming up satellites designs in my Remote Sensing Principles and Techniques class) and normal science? This cuts to the heart of the problems with the data-to-action paradigm. Truth be told, important connections between smaller projects do not occur naturally. They must be built into the infrastructure of the field. Ultimately, the lack of infrastructural connection between smaller projects makes it more difficult to argue specifically about how these Earth Remote Sensing data projects are crucial in our understanding of larger earth systems like atmospheric aerosols, forest degradation, and erosion, to name a few (i.e. the shape of the elephant which we are eating one bite at a time).[51]

[51] I'll make note, again, of our highly important — if highly flawed — Bretherton Diagram.

STS has a related, if inverted, problem: we tend to seek out the holistic pictures. We focus on broad theoretical frameworks: systemic injustices, systems of power, ideological influences on technoscience and how they manifest in our cultural landscape. We do zoom in and out, locating site-specific instances of these larger dynamics. However, once we arrive at a system that does not fit into *our* theoretical lens, we have trouble viewing it through any other kinds of lenses. And so, we can erase the importance of the micro-project; the importance of muddying our theoretical lenses for the sake of digging into new, important kinds of problems. STS projects do have a different sort of humility, though: one that knows that important connections between worlds exist, and that we can never categorize, know, or commodify a thing (like an ecosystem) to its fullest extent. However, too intent on making things more complex and inclusive, we can easily lose sight of the importance of an instrumental intervention, even if it's imperfectly instrumental and theoretically messy. We aren't trained to make instrumental connections that are pleasing to us as theorists, and so we often assume that these instrumental connections must be black-boxing and simplifying and naturalizing. But we *must* make these kinds of connections to in order to make an intervention.

I'll provide you with some examples of this, that I think are specific to STS projects related to Remote Sensing. In *The Gendered Eye in the Sky: A Feminist Perspective on Earth Observation Satellites*, Karen Litfin writes:

> Of all of our senses, vision requires the least engagement; the advantage lies in separation rather than closeness. The photograph, anad most especially that of the earth from space, 'places the final seal on the disengagement from participation that vision allows, on the standing back so that subject views object across a void. It transforms the external world into a spectacle, a commodity, a manipulable package . . . [through] the predatory nature of the camera.' The miniaturization of the earth made possible by satellite photography appeals to the managerial impulse; the 'blue-and-white Christmas ornament' can be

> 'managed' far more easily than a world of 5.5 billion people and thousands of cultures. [...] From space, the ultimate domination of the earth, or at least the illusion of it, becomes possible (40; 1997).

In many senses, I find this a salient critique. It's true that we value visual data over other kinds of data, and it's true that viewing the earth from above represents a certain kind of power dynamic, although I think that one can surrender power when one views from above as well. And it's true that the view from above offers one of many ways to understand the environment. It is also true that those who view Earth from above have power; access to certain technologies and others do not; expertise that the average person on the ground does not have. But how, I wonder, will this land with folks in Remote Sensing? Other Earth scientists *do* study environmental systems from the ground, but the specific task of the Remote Sensing researcher *is* to study from above. And although there is great power in doing so, it is also the single most effective way to understand large-scale ecosystems dynamics; to understand the anthropocene and the causes and effects of climate change, for instance.

As someone who is in both communities, I feel torn by critiques like this. The STS-er in me knows it is salient, but the Remote Sensing scholars knows it will land oddly. What, for instance, would critical theorists have the Remote Sensing community do with the critique that they're turning the Earth into a spectacle; a commodity? And can we know, unequivocally, that this is what every Earth Remote Sensing researcher is doing, and how every project manifests? They might say, *to answer that question, you'd have to understand my project at a more technical level*. And in truth, we might say, *To fully understand this critique, you ought to read some more Foucault*. Here, we have a stalemate. In both cases, the answer looks like this: *You just have to know the kinds of specifics we know*. This is one moment in which the parallels between the critique-to-action paradigm in STS, and the data-to-action paradigm in Earth Remote Sensing, become clear. Both communities assume there is an apparent takeaway from their (extremely valuable and expertly fine-tuned) work when, indeed, there isn't.

Here's a broader example from Donna Haraway, who, in this chapter, I am both invoking and critiquing. In one of my very favorite pieces, *A Cyborg Manifesto: Science, Technology, and Socialist-Feminism in the Late Twentieth Century*, she writes:

> Our best machines are made of sunshine; they are all light and clean because they are nothing but signals, electromagnetic waves, a section of a spectrum, and these machines are eminently portable, mobile — a matter of immense human pain in Detroit and Singapore. People are nowhere near so fluid, being both material and opaque. The ubiquity and invisibility of cyborgs is precisely why these sunshine-belt machines are so deadly. They are as hard to see politically as materially. They are about consciousness - or its simulation. They are floating signIfiers moving in pickup trucks across Europe, blocked more effectively by the witch-weavings of the displaced and so unnatural Greenham women, who read the cyborg webs of power so very well, than by the militant labour of older masculinist politics, whose natural constituency needs defence jobs. Ultimately the 'hardest' science is about the realm of greatest boundary confusion, the realm of pure number, pure spirit, C3I, cryptography, and the preservation of potent secrets. The new machines are so clean and light. Their engineers are sun-worshippers mediating a new scientific revolution (53; 1991).

Haraway, of course, is not talking specifically about Earth Remote Sensing. She is talking about electronic technologies. But I interpret the critique as quite applicable to it, as Earth Remote Sensing, too, uses invisible signals; hardware (including silicon wafers) that are produced in ways that may not be as environmentally friendly or just if we further investigated. And, again, I feel the critique is salient. The Remote Sensing community doesn't often discuss where or how the raw materials for these satellites are developed, or that both the development — and use — of these materials often involves some kind of power dynamic and some form of exploitation.

My issue with Haraway's critique, however, is that it hues closely to that *original sin* argument. Firstly, nothing is without *original sin*, even and especially the humanities and the social sciences (even and especially Philosophy and Anthropology, which have deep, dark histories of colonialism, exploitation, and insidious institutional power). I can see, then, how this critique, if brought upon the Remote Sensing community, could provoke defensive questions, like, *What makes you so high and mighty in your research that you get to critique mine?* And, as we saw in Chapter 1, this kind of critique can provoke the counter-critique: *sure, Remote Sensing can be exploitative. But look at all of the good that it brings to the world.* We then become stuck in this cycle of talking past each other, an endless merry-go-round of *this technology is bad; no it's not.* That gets us nowhere.

Furthermore, Haraway's point that the 'hardest' science is about the realm of greatest boundary confusion is half-right, if you subscribe to Latour's critique of modernism and postmodernism in *We Have Never Been Modern* (see my introductory chapter for further definitions and context). I do subscribe to that critique. I tend to agree with Latour's point: that both technoscience and *its critics* — that's us! — engage in acts of hybridization and connection-making, while subsequently denying that those hybrids exist in the world and covering up their tracks. I think that Haraway is very aware of this dynamic, actually, and of course the whole concept of *the cyborg* is supportive, broadly, of the notion that we must acknowledge and respect those hybrids. But why then, can't we hybridize that beautiful critique into something that the RS community — or other *light and clean* communities — could hear, take with them, and subsequently put into some kind of action with our help?

I will reveal one more site in which I find the critique-to-action paradigm, this time as it relates to Vandana Shiva's ecofeminism. Like Latour, Shiva addresses acts of purification of science and culture. In her book *Monocultures of the Mind*, she argues that "[c]onservation of diversity is, above all, the production of alternatives, of keeping alive alternative forms of production. [...] [U]niformity and diversity are not just patterns of land use, they are ways of thinking and ways of living" (6). In other words, Shiva draw parallels between biodiversity and diversity of thought — just as Russell (see the end of Chapter 2) draws parallels between our attempts to control nature and our attempts to control other countries, governments, and regimes.

Furthermore, Shiva argues that dominant knowledge systems (let's say, a Landsat image of a given field) "disappear" local knowledge (let's say, a farmer's knowledge of that field, which she works on each day), and create monocultures in this way. "Emerging from a dominating and colonising culture," she says, "modern knowledge systems are themselves colonising":

> The knowledge and power nexus is inherent in the dominant system because, as a conceptual framework, it is associated with a set of values based on power which emerged with the rise of commercial capitalism. It generates inequalities and domination by the way such knowledge is generated and structured. The way it is legitimised and alternatives are delegitimised, and by the way in which such knowledge transforms nature and society. Power is also built into the perspective which views the dominant system not as a globalised local tradition, but as a universal tradition, inherently superior to local systems. However, the dominant system is also the product of a particular culture (9-10; 1993).

In other words, dominant systems of understanding ecology — namely Western, scientific systems (which were at one time local) — exert power over more continuously local understandings of ecology. This exertion of power de-legitimizes other ways of knowing about nature, while simultaneously altering nature, argues Shiva. After all, we make decisions about what to do with our respective ecosystems based on the scientific knowledge we gather.

Shiva's arguments are salient, in the same ways that Litfin's and Haraway's are. It's true that Remote Sensing of the Earth is a largely Western science, and that the view from above, as Litfin says, disappears forms of local knowledge. However, if we continually argue that the view from above and the local are fundamentally in opposition to each other, how will we ever put them back together? If, for instance, (1) understanding climate change involves modelling global cycles with long-term data at the "view from above" level and, if (2) the poorest, most marginalized, most colonized people will be most affected by climate change (displaced from their homes, lacking in resources and livelihood, risking bodily injury due to disastrous and unpredictable weather), how do we reconcile this? If the Earth Remote Sensing researchers' analyses could, ultimately, help the farmer on the ground foresee the future of her low-lying farm, ought we continue to live in a world where these two views are opposed?

The critiques are valid, much like an analysis of Landsat data. However, in order to rectify the problem identified by the critiques (and the data analysis), we *must* learn how to make connections between multiple scales. This makes me wonder if Shiva herself, at times, perpetuates a monoculture by functionally disallowing us to find connections between the view from above and the view on the ground, and to move forward with solutions. If we are to revisit Latour, we might ask: are the monocultures really monocultures, as Shiva is suggesting? Or are they monocultures on the surface, but ones that both proliferate and hide deeply hybrid ways of thinking? To move beyond the true monocultures, we must *diffract*.

Diffractive Interventions: Engaged STS and Other Philosophies

My diffractive intervention — indeed, a new-but-old kind of *diffractive STS* — is a patchwork of theories. Engaged STS is the fabric I begin with, as it offers some of my favorite ways to bypass the critique-to-action paradigm. This is in part because the subfield has found ways to simultaneously and diffractively intervene into other fields, alongside its interventions into STS. In their 2016 chapter *Making and Doing: Engagement and Reflexive Learning in STS*, Gary Downey and Teun Zuiderent-Jerak outline six major elements of *making and doing*, the praxis that manifests engagement, in STS.

Elements of this intervention include *Frictions and Alternative Images*, or the uneasiness that arises when "dominant images lose their smoothness and become multiple" (227; 2016); *Techniques, Devices, and Infrastructures* refer to specific ways by which STS-ers implement interventions in their given sites of study (227; 2016); *STS Expertise and Identities*, or how the educational background of an STS-er influences the kinds of intervention they will make (227; 2016); *Audiences, Partners, and Engaged Practitioners*, or the persons for whom the intervention will be enacted and written (228; 2016); *STS Sensibilities Out There*, or the ways in which "STS sensibilities in the wild relate to STS making and doing practices" (228; 2016); *Feedback and Reframing*, or the ways in which the STS scholar's interventions are received and understood by the community they are intended for (229; 2016).

Downey and Zuiderent-Jerak also put a name to three major camps or "clusters" of *Making and Doing*: these are *Boundary-Crossing STS Claims*, *Meta-Activism*, and *Experiments in Participation*. They describe *Boundary-Crossing STS Claims* as "building practices that aim to help STS claims travel across the boundaries of the field into [their respective] worlds" (229; 2016). This cluster, say Downey and Zuiderent-Jerak, "[tends] to highlight the benefits of apprehending deep interrelations among science, technology, and society, typically evidenced in a specific case or cases at hand" (230; 2016). In other words, boundary crossing is about making inroads into scientific/technological communities in which STS perspectives would be beneficial, and helping a given community understand that its practices do not exist in a vacuum.

For the Remote Sensing community, for instance, this might involve highlighting historical contexts and multiple narratives in the development of a given satellite. This would demonstrate that Earth Remote Sensing satellites are not only important resources for understanding Earth systems, but also vehicles upon which particular systems of values have been inscribed. I have tried to enact these kinds of interventions in Chapters 1 and 2 of this dissertation. My stake, here, is that I am (1) part of the Remote Sensing community and (2) therefore I know that by exploring these issues — and bringing forward my theses about the *data-to-action* paradigm, I believe I am instantiating a critique that the Remote Sensing community will care about as well (although, that remains to be seen, and my project may have to cycle through some more iterations of itself to speak as well as it can to my fellow colleagues in Remote Sensing).

The second cluster, *meta-activism*, "[refers] to the activities deployed by STS scholars to assist or support actors who may already be resisting, challenging, or seeking alternatives to dominant images in their fields" (234-235). These projects are ones that seek to expand on activist work in scientific and technological fields. Downey and Zuiderent-Jerak say that these projects are "meta" because "they still largely delegate the contents and work of advocacy itself to actors in the field" (235). However, they lend STS expertise to these already-existent instances of activism, with the goal of providing important resources, connections, and arguments. This project is *not* an instance of meta-activism. But if it were, I would choose to unite myself with citizen scientists, for instance, within Earth Remote Sensing. However, my project's marriage to heavy instrumentation — and particularly heavy instrumentation with connections to powerful institutions — prevents the project from being an instance of *meta-activism*.

Finally, the third cluster is defined as *experiments in participation*. These are projects that "formulate, enact, and reflexively learn from novel, STS-inspired practices within their fields of study [...] participatory experiments [that] actively blur boundaries between the project and the field" (239). In other words, these projects make less clear the distinctions between STS scholarship and the fields in which it intervenes.

> [...] participatory experiments actively mobilize the localized complexities in technoscientific practices that dominant images and practices hide or ignore. Whereas scholarship in the first cluster mainly envisioned boundaries between empirical fields and STS scholarship, work within this cluster rather emphasizes internal inconsistencies *within* empirical fields (240).

Here, Downey and Zuiderent-Jerak put forth the idea that *experiments in participation* are different from *boundary crossing STS claims*, in that the latter, by way of its boundary-crossing nature. This nature inherently focuses on key differences between STS ways of thinking, and thinking in the given scientific or technological field of study. Meanwhile, the former, because of its reflexive nature, tends to focus on frictions within both of its fields, and therefore within the project itself.

I am not convinced that this distinction is necessary. Should projects that "actively blur boundaries between the project and the field" not also blur boundaries between STS and the "empirical" field at hand? These two attributes seem intimately connected: after all, if an *experiment in participation* blurs boundaries between itself and, say, the Remote Sensing field, mustn't it also break down boundaries between STS and Remote Sensing? I want to put forth the argument that this kind of project *must* create a sort of unique diffraction pattern between STS and Remote Sensing, which in turn involves a recognition of the boundaries that have defined the two fields not just in relation to each other, but in relation to the rest of the world. I bring up this *diffraction* between *Boundary-Crossing STS* and *Experiments in Participation* because I wish to position my project within it.

In this way, I see my project as a continuation of Downey and Zuiderent-Jerak's *making and doing*. One major and important difference, however, is that *making and doing* is largely situated within a kind of social scientific participation/intervention. Its focus is on communities of practice, forging new social scientific methodologies within those communities. My project is a *yes, and*, in that its site of intervention begins with reasoning. The data-to-action paradigm — along with the critique-to-action paradigm — are thought experiments of sorts. They are theoretical models that I've created to describe dynamics in two fields that I participate in. My project is to theorize and contextualize these paradigms, and to seek intervention that begins with theoretical frameworks.

There is also this: I do not see reasoning as an entirely apart from the social scientific participant-observer and/or practitioner. These two kinds of scholarship are intimately connected. We see this, even, in Downey and Zuiderent-Jerak's piece. It is about social-scientific participation/intervention, yes, but it is also a theoretical framework that makes such participation/intervention possible. Nonetheless, most scholars who align themselves with the *making and doing* movement would not necessarily describe themselves as philosophers or theorists. I want to change that. Participation and intervention can — and should — be a diffraction between theory and practice.

There are facets of my project, too, that set me apart from most practitioners of social science. I am more-or-less the same Samantha — with the exception, perhaps, of some academic code-switching — in both STS and Remote Sensing. Nonetheless, I feel a sense of self that stays with me in both spaces, and I do not feel those values changing as I move between them. Nor do I feel the need to hide them, or describe myself differently (even though I feel, at times, that I go very much against the grain in both communities, and have questioned my identity in both communities equally). As I peruse my "field notes" from the many hours I've spent sitting in Remote Sensing courses and lectures, I notice that they are not field notes at all. Rather, they are straight-up notes; the kinds I might take in a History of Science or a Philosophy of Technology class. I note the topic at hand, and take meta-notes that incorporate that topic into my project and worldview. And the way in which I feel allied (or not) with colleagues in the Remote Sensing community has felt similar to the ways in which I have felt allied (or not) with colleagues in the broader STS field. As with any space, there are some thinkers with which I feel a great connection, and some with which I feel a great incommensurability.

In both fields, I have noticed, we young scholars have strong visions about what our collective futures should look like, and we are attached to those visions as we are simultaneously attached to future success in our careers. We stake out claims and hope to see them through to our retirements (a consequence, indeed, of both fields belonging to an academic system that is more about business than an idealistic pursuit of knowledge — this is our livelihood). Will Lidar techniques help us understand canopy cover in better ways than other kinds of sensing? Remote Sensing questions like this are simply different configurations of questions in STS. Questions like: Is feminist science studies a more just lens than that of infrastructure studies? I also do not want to seem to cynical: these questions are not only about staking out a career, but I believe they are also passionate commitments to what our collective futures ought to look like.

This brings me back to a *diffractive STS*. I have gestured to it, and I have even provided some examples of what I think it might look like in scholarship. But I have not talked much about where it comes from, or what it looks like as a theoretical framework. And, as I feel the intervention is in the theory in tandem with the practice, I will spend the next several pages familiarizing you with the structure and history of those feminist theories that we call *diffractive*.

Diffractive Theories: A Brief Review

In her 1992 essay *The Promise of Monsters: A Regenerative Politics for Inappropriate/d Others*, Donna Haraway first put forth this idea of diffraction in feminist theory. The idea originated initially with literary theorist Trinh Minh-Ha in a more abstract sense, and Haraway adopted it for feminist epistemologies in this essay. “Trinh Minh-Ha’s metaphors,” she says

> suggest another geometry and optics for considering the relations of difference among people and among humans, other organisms, and machines than hierarchical domination, incorporation of parts into wholes, paternalistic and colonialist protection, symbiotic fusion, antagonistic opposition, or instrumental production from resource. Her metaphors also suggest the hard intellectual, cultural, and political work these new geometries will require. [...] [P]erhaps a differential, diffracted feminist allegory might have the ‘inappropriate/d others’ emerge from a third birth into [...] a place composed from interference patterns. Diffraction does not produce ‘the same’ displaced, as reflection and refraction do. Diffraction is a mapping of interference, not of replication, reflection, or reproduction. A diffraction pattern does not map where differences appear, but rather *maps where the effects of difference appear* (300, italics my own).

Here, Haraway calls for a new way to compare without comparing — to study boundaries and collapse without building further boundaries and creating further collapse. When she puts forth the idea that “[a] diffraction pattern does not map where differences appear, but rather *maps where the effects of difference appear*” (300, italics my own), she makes clear that, up until this point, critical theorists, feminist theorists, STS-ers, philosophers, ethnographers, et. al. had not yet formally created a way to do this. And while the ethic of a *diffractive* criticality is ever-present in Haraway’s work, she leaves the stitches of this new diffractive project to be picked up by another theorist who wants to continue to knit in that direction.

Karen Barad's *Meeting the Universe Halfway*, a feminist exploration of feminist theory and quantum physics *does* fully take up the issue of diffraction, and elaborates on it in a way that is quintessential to this dissertation project. However, Barad's work does not merely take up the idea of diffraction — it is diffractive, in the sense that it reads quantum physics and feminist theory through each other, bringing insight to issues of observation, and blurring the distinction between what it means to study something or even provoke an issue, and what it means to intervene. In order to do this, Barad builds worlds with her theory. She picks up Haraway's stitches: "Donna Haraway proposes diffraction as an alternative to the well-worn metaphor of reflection. As Haraway suggests, diffraction can serve as a useful counterpoint to reflection: both are optical phenomena, but whereas reflection is about mirroring and sameness, diffraction attends to patterns of difference," (29) and diffraction patterns "[read] important insights and approaches through each other" (30).

Here, Barad proposes diffraction as a new way to contemplate and understand the world, and therefore a new way to be in the world as well. Blurring distinctions between contemplating/studying/being/influencing is essential to Barad's framing device and, I believe, to carving out a new kind of space for an interventionist STS:

> [...] while it is true that diffraction apparatuses measure the effects of difference, even more profoundly they highlight, exhibit, and make evident the entangled structure of the changing and contingent ontology of the world, including the ontology of knowing. In fact, diffraction not only brings the reality of entanglements to light, it is itself an entangled phenomenon. [...] So at times diffraction phenomena will be an object of investigation and at other times it will serve as an apparatus of investigation (73).

Barad reiterates that yes, unlike other ways of viewing the world, diffraction is not comparative. Rather, it reads differences through each other which, in turn, means that it is both an object of study and a framing of that investigation. In this way, diffractive kinds of interventions are important alternatives to ethnographic ones, in that they scrap the distinction between the studier and the studied, and in that they assume intervention. When one is reading two communities through each other, one necessarily creates something new, has effects on those communities, and intervenes in both of them.

Furthermore, for Barad, ethical issues are deeply entangled in this diffractive lens, and are part and parcel of enacting an intervention:

> Ethical concerns are not simply supplemental to the practice of science but an integral part of it. [...] [V]alues are integral to the nature of knowing and being. Objectivity is simultaneously an epistemological, ontological, and axiological issue, and questions of responsibility and accountability lie at the core of scientific practice. [...] Realism, then, is not about representations of an independent reality but about the real consequences, interventions, creative possibilities, and responsibilities of intra-acting within and as part of the world (37).

In other words, concerns about what is good and right in a given situation (ethics), are deeply intertwined with ways of knowing (epistemologies) and ways of being (ontologies). And therefore, when one studies, one cannot avoid the responsibility of intervention, nor the responsibility of thinking about what the best kind of intervention might be — one that is in the best interest of all actors involved. Later, Barad relates this conviction to Ian Hacking's (sort of famous) imperative: "Don't just peer, interfere" (189; 1983). "Reflection is insufficient," she says; "Intervention is key." (50). So where does this leave us? We know that diffraction is by nature an intervention, and so, not only can we not avoid an interference when we join another group or field, as we do in STS, but we also cannot avoid ethical or normative inquiries into how we *ought* interfere. However, Barad's task is not to explore ethics as intensely as, say, a virtue ethicist might.

Enter Shannon Vallor's *Technology and the Virtues*. In this book, Vallor "[shines] a light on [an] all too real disease: a widening cultural gap between the scope of our global technosocial power and the depth of our technomoral wisdom" (249; 2016). Vallor argues that we must find new orientations toward technological criticisms; that we must reconstruct as much as we deconstruct; that we must, collectively, find a new paradigm through which to view technosocial processes. This paradigm is one of virtue ethics or, as Vallor says, the cultivation of *technomoral wisdom* and *technomoral virtues*. These are not black-and-white rules for good and bad relationalities to technology, but rather a learned and reasoned sort of judgement for dealing with ethically complicated and emergent technological relationalities. Vallor roots these kinds of judgements in Aristotelian, Confucian, and Buddhist ethics.

Vallor's ethics have a strong feminist feel and, to me, feel related to Barad and Haraway. "[R]elational understanding," says Vallor, "[...] is essential to the practice of moral self-cultivation [...]" (76; 2016). She then goes on to explain that, even in traditional virtue ethics, people are "relational being[s] [...] whose [identities are] formed through a network of relationships" (ibid). This is in contrast to much of traditional philosophy:

> Most classical virtue traditions view the self as partly or wholly constituted by concrete roles and responsibilities to specific others in one's moral community. The responses of others to my actions continually inform, and often correct, my sense of who I am in the world. Though these responses can often be unfair or misguided, reliable self-knowledge could not be formed or maintained in their absence. Contrary to Kant (and Nietzsche), the purely autonomous agent can never be a fully realized, self-aware human being. Indeed the ideal moral agent is *not* the person who most successfully detaches her deliberations

> from her own relational context, but rather the one who understands and responds to that context most fully — that is, a person of practical wisdom (77; 2016).

In other words, taking constructive, normative, and moral action is not so much an act of detachment, but an act of situatedness. One must understand one's place in the context of one's experience and community to act on or to recommend a moral judgement of best fit. Vallor then goes on define this *relational understanding* as "not a noun but a verb, not a possession but an active, sustained achievement" (ibid). Vallor likely does not realize she is doing this, but she perfectly connects the Baradian/Harawayan situated entanglement with the Downeyan and Zuiderent-Jerakian *making and doing*. When one is taking what one believes is the correct stance, one recognizes their own context, the necessity of normative action and, indeed, one's simultaneous critique and interference. This is an amended version of Ian Hacking's *Don't just peer, interfere*. It begs us to interfere *while considering the best outcome for all parties involved*.

Of course, a *best outcome for all parties involved* is not a quick or easy fix; not something that can be gleaned from a singular interaction; not something that has one correct and obvious answer; one that is simultaneously process- and result-oriented. "The world does not give us advance notice of every kind of moral scenario it might throw at us, and there is no definitive moral playbook that tells us in advance how to read or respond to any situation," says Vallor (100; 2016). This is what *technomoral cultivation* is all about. One must learn how to reason in a particular way to glean technomoral values, and there is no one-size-fits-all algorithm for this kind of reasoning. Vallor refers to this brand of reasoning as *practical wisdom*.

The Aristotelian ideal of *practical wisdom* is about "[aiming], [deliberating], and [calculating] well[,] concerning how what is 'best for man' can be attained by action in the present" (106; 2016). It is also about "reasoning rather than simple intuition or guesswork, but [a kind of] reasoning that is both correct and *expedient* — that is, effective within the time allowed for a decision. Thus a person who always calculates the right course of action, but not before the window of opportunity to put that decision into practice has closed, is not prudent" (ibid). In other words, *practical wisdom* is not only relational and situated — it is also attuned to what can be done to achieve a desirable and moral outcome in a given circumstance, under potentially limited conditions.

I bring up this practical aspect of Vallor's technomoral virtue because I think it makes a particularly diffractive intervention within both STS and Remote Sensing. Up until now, making and doing — or actionable intervention — has been laid out as the territory of STEM practitioner or social scientist, and is not often associated with the humanist purview. This creates a situation where it is hard to imagine researchers within this purview — philosophers and historians, for instance — intervening in actionable and practical ways. Additionally, in the world of Remote Sensing, intervention is typically imagined in the form of a dataset (new or continuing), an algorithm, or an instrument on a satellite or a drone. My second community, too, often does not think of intervention as something that can be done by way of a specific brand of measured reasoning. And so, Vallor offers a *diffractive* kind of intervention to, well, intervention in *both fields*. And as both STS and remote sensing are concerned, sometimes covertly and sometimes not, with a *greater good* and a better collective future, it would make sense that this measured reasoning takes such an ideal, in all of its complexities, into account.

There is one final ingredient in my framework of the *diffractive ethic*: Jen Henderson's[52] work on ethics of accuracy, ethics of care, and ethics of resilience in weather forecasting. According to Henderson, accuracy is a multistable thing. That is, it means different things to different people (see my exploration of multistabilities in the introductory chapter). However, she synthesizes the notion of accuracy in her theorization of it. Ethics of accuracy are, she says, "[values] forecasters place on correctly identifying unfolding meteorological conditions before weather phenomena occurs and potentially [affects] people in their communities" (12; 2017). In other words, accuracy is the best interpretation of a forecast, always with an eye toward predictive action. Ethics of care are "[values] that [are] revealed most clearly in moments of crisis but [exist] in [forecasters'] daily work as well. Forecasters are bound by care in their desire to protect [...]" (xxxix; 2017). Care compels forecasters to turn their predictions into actions that potentially save lives. And finally, ethics of resilience are "as much about [forecasters' livelihoods] and their own survival as a profession as about those they protect" (xxx; 2017). *Ethics of resilience* are the ways in which forecasters substantiate themselves by way of ethics of accuracy and ethics of care, diffracted through each other. Henderson builds this diffraction into her conception of these three kinds of ethics — and the

[52] Henderson completed her doctoral work at Virginia Tech, in the Department of Science, Technology, and Society (STS), a few years ago. Despite very different styles — Henderson is an ethnographer, and I am a philosopher at heart — there are major similarities between us. Henderson was the first student in the Remote Sensing Interdisciplinary Graduate Education Program (IGEP), a group to which I belong as well. Additionally, she was advised by Saul Halfon (as I am), and Gary Downey (on my committee, and an important mentor in my academic career). And if you were to diffract our work through each other, you might find patterns of Engaged STS, of contributory expertise, of earth remote sensing, and of emphases on data-to-action.

relationality between them. "I term this interrelated set of ethics 'empathetic accuracy,'" she says,

> As a way of illustrating the interconnectedness of accuracy, care, resilience, and relationality more explicitly. It calls attention to the fact that these values are entangled in forecasting science, co-constituted in forecasters' technological developments and policies, and reflected in the activities that direct their interactions with different publics. In fact, etymology of accuracy in early sixteenth century Latin is *accuratia*, or to care or give attention, as in 'executed with great care.' Perhaps ironically, then, the linguistic root of the term accuracy contains care. But accuracy as a term in forecasting is so overly burdened by scientized connotations about precision, truth, and objectivity, that it is difficult to merely point to its roots and have that suffice as a way of enrolling forecasters to consider care as an important ethic in their labor (xxxvii-xxxviii 2017).

The interconnectedness of the accuracy, care, and resilience ethics make sense, in other words, given that the Latin origins of the word *accuracy* include connotations of care and attention. However, says Henderson, even that etymological diversion isn't enough to convince forecasters that care and resilience are just as crucial — and deeply interconnected with — accuracy. Rather, accuracy is seen as a value apart from other values: a pure one that speaks of scientific objectivity.

The "[...] ideal of objectivity attempts to eliminate the mediating presence of the observer," write Lorraine Daston and Peter Galison. "[S]ome versions of [scientific objectivity] rein in the judgments that select phenomena, while others disparage the senses that register the phenomena, and still others ward off the theories and hypotheses that distort the phenomena" (82-83; 1992). The notion of objectivity forcibly disconnects the forecast, the observation, or the data from its forecaster, observer, or collector/ analyzer. In turn, we separate notions of care from notions of accuracy when we ought not.

And so, with this patchwork framework in mind, I will attempt to lay out the beginnings of a *Diffractive STS* framework. Drawing from Downey, Jerak-Zuiderent, Haraway, Barad, Vallor, and Henderson, I put forth *diffractive STS* — and *diffractive ethics* within STS. *Diffractive ethics* are an extension of Henderson's interrelated ethics (see my introductory chapter), with the added concept that we must read different kinds of ethics through each other to arrive at the most productive version of our research. *Diffractive ethics* are also an extension of Engaged STS. The following are characteristics of the *Diffractive STS*, and the *diffractive ethic* within it:

- Diffractive STS is community-driven. That community is interdisciplinary, and likely extends beyond multiple boundaries;
- It is propelled by friction as much as it is found through agreement (Downey and Zuiderent-Jerak 227; 2016). This friction can be generative. Actors with different perspectives are searching to reconcile, even where perfect reconciliation is difficult or impossible. Here, I must refer back to Habermassian ideals of dialogic change;
- It is contributory and/or interventionist, rather than interactive or participatory (Collins and Evans 254; 2002);
- Arbitrary conceptions of theory and practice dissolve — they are not merely in service of each other, but inseparable;
- These projects are, by nature, impure. They become more impure over time, as they contribute to — and thusly are amended by — the communities that take them into account.

Beyond Henderson, I find Wylie et. al.'s *Materializing Exposure: Developing an Indexical Method to Visualize Health Hazards Related to Fossil Fuel Extraction* a particularly good example of a diffractive, Engaged STS — particularly with respect to accuracy, measurements, and environmental science. Wylie et. al. study *in situ* measurements, rather than remotely-sensed ones. However, environmental science and STS are diffracted through each other by way of this research, and there is an emphasis on those forgotten places between data, action, and societal good.

Engaged STS and In Situ Measurements of Fossil Fuel Emissions

The premise of the Wylie et. al. paper is this: There are areas of Wyoming for which extraction of oil and natural gas is common. People who live around these areas of extraction often "report odors of rotten eggs and describe symptoms of H_2S exposure" (426; 2017). H_2S, or hydrogen sulfide, is a highly poisonous gas that oil and natural gas extraction leave behind. Oil and gas companies have preempted this, to some extent:

> H_2S is recognized as an acute and chronic threat to human and environmental health[,] and oil and gas companies are required to have plans in place to prevent and respond to accidental, high concentration releases of H2S. They are not, however, required to monitor, report[,] or prevent routine daily emissions. Yet 15-25% of the oil and gas wells in the US are predicted to contain H2S, and some communities surrounded by multiple wells report chronic, routine exposure (426-427; 2017).

In other words, while oil and gas companies are required by regulation to monitor H_2S emissions generally, and to prevent extreme leakage of the compound, they are not required to do so on a regular basis, or for daily kinds of exposure. Nonetheless, people who live around these sites in Wyoming report daily exposure. Furthermore, say Wylie et.

al., "[c]hronic exposure is difficult to represent with current tools for monitoring H_2S[,] because they are designed to measure acute workplace exposure" (ibid).

The article begins when Wylie and her team of researchers visit the Bakers: a local family in Frannie, Wyoming. The Bakers describe their symptoms: foul odor, lightheadedness, fatigue, and a high presence of rust around the oil wells near their property. This, say Wylie et. al., is a sure sign of "the presence of this corrosive, toxic gas" (428; 2017). Additionally, Wylie et. al. verify that these symptoms are *both* individually and scientifically pertinent:

> Acute exposure to high levels of H_2S can cause headaches, seizures, nervous system damage, paralysis, and death (Skrtic 2006), and is a leading cause of workplace injury in the United States. Between 2011 and 2014, 1,157,410 contracted workers incurred occupational injuries or illnesses from H_2S gas poisoning, 19 of which were fatal (US Department of Labor, Bureau of Labor Statistics 2015). Considering these statistics, H_2S has been primarily regulated as a workplace hazard, resulting in regulations tailored to emergency notification of acute high dose exposures. However, as oil and gas extraction grows, it is increasingly possible that many communities could be chronically exposed to H_2S. Long-term exposure to low levels of H_2S [also] poses a serious hazard to human health (Legator et. al. 2001; Kilburn, Thrasher, and Gray 2010). One human epidemiological control case study compared populations in Texas and Hawaii, where H_2S is naturally present in low concentrations. The exposed groups' odds of suffering a central nervous system problem were found to be 12.7 times higher than the control groups (Legator et. al. 2001). Another study in New Mexico showed similar patters (Kilburn, Thrasher, and Gray 2010). In the Southeast US, Centers for Disease Control and Prevention (CDC) researched H2S emitted from drywall manufactured in China and found health effects at levels as low as 1.4 ppb. This value is approximately 7,100 times lower than the recommended workplace exposure established by the National Institute for Occupational Safety and Health (NIOSH) of 10,000 ppb for 10 minutes (J. Allen et. al. 2012). [...] In 2011, the EPA also concluded that chronic, low dose exposures could be harmful to human health (Environmental Protection Agency 2011). Despite these conclusions, oil and gas facilities are not required to prevent or report H2S emissions (Environmental Protection Agency 2011) (431-432; 2017).

This low-level, long-term H_2S exposure is not simply an issue propagated by noisy laypeople who do not understand science, or who are imagining things. Studies have shown that, while high exposures to H_2S can be fatal — and are significant contributors to workplaces injuries in the United States — low exposures to H_2S have serious effects, but in different ways. However, these studies on low, long-term exposure have not moved oil and gas companies to regulate more strongly on these emissions, or to consider that low exposure is a problem worth putting as many resources into as high exposure. To address this issue, Wylie et. al. speak the language of scientists, of laypeople, of industry professionals, and of STS practitioners simultaneously.

How, then, do we empower families like the Bakers to provide scientific evidence of their struggle, and to represent that evidence in a way that will be institutionally respected? How to undergird all of this with an ethic of Engaged STS? Wylie et. al. respond to these calls to action by collaborating with "Wyoming residents and Public Lab, an online community that develops open source tools for community-based environmental monitoring" (429; 2017). Then, they set about creating a new kind of measurement device that meets three additional standards: (1) it measures emissions on a daily basis; (2) it isn't black-boxed, so communities can read the measurements themselves; (3) instructions for building and reading the device should be democratized, or shared "with an online community" (430; 2017).

Wylie et. al. zero in on an issue of instrumentation, and one that perpetuates the current problem: the "gold-standard occupational device for monitoring H_2S" (434; 2017), the Jerome meter, is a bureaucratic instrument readable and affordable to oil and gas companies looking for real-time, high-exposure H_2S measurements. Furthermore, the Jerome meter needs to be coupled with an additional test — and one that requires exposure "to a known amount of H_2S" (435). Therefore, the Jerome meter is not accessible or interpretable to people like the Bakers, and would likely not provide them with useful data anyway.

Wylie et. al. experiment with photographic paper, as an affordable and interpretable (*and* supplemental) alternative to the Jerome meter. They place the paper near the Bakers' property, at the sites of concern, and take measurements after one week, and after three weeks. and Photopaper, Wylie et. al. contend, is better for measuring chronic, low-level exposure to H_2S. Specifically, it is better at "[making] emissions and contamination visible and thereby collectively witnessable" (453; 2017). "The indexicality and performativity of the photopaper method potentially challenge the imperceptibility of low-dose exposure and shift the register of social debate from the emissions concentrations to their reach across landscapes and communities," they argue:

> [...] photopaper can be plotted to form what we describe as 'data-rich' maps that show the relative intensity of corrosive sulfuric gases over time in a large geographic area. Once fixed and dried, exposure strips can be arrayed over a map of the testing area to show a landscape of the relative intensities of corrosive gases. This way, the photopaper offers a qualitative look at levels of H_2S and provides an easily readable assessment of where the gas may be found at higher and lower levels over a longer window of time, such as one or three weeks. The 'data-rich map' can help illustrate relative spatial variability of the gas and relative concentrations of the gas at each location. [...] Exposure to H_2S is noxious; its rotten egg smell physically ties the exposed person to a particular location and time. However, such smells do not travel well as representations. [...] Unlike numeric representation of exposure, the photopaper test strip materially attests to exposures in a manner similar to the ways bodies experience it. The test strip stands in for the nose as a reactive, recording surface that, unlike smell, can travel to represent recent exposures (442-443; 2017).

In other words, while instruments like the Jerome meter cannot measure low, consistent exposure to H_2S, photopaper can. Not only that, but it is an affordable, mappable, and visualizable project. While rotten egg smells and dizziness are difficult to represent as data, photopaper can be used to map those smells and physical experiences:

to represent them both quantitatively and qualitatively. However, Wylie et. al. make clear that, while the photopaper solves the issue of democratization of measurement, and underregulation of low-exposure H_2S, it “should not be misrecognized as a replacement to tools like the Jerome meter" (454; ibid). Rather, both instruments should be considered crucial to the measurements. This, I believe, is the contributory expertise of Engaged STS: adding different *kinds* of sensing tools that elevate different *kinds* of stories, in ways that complement already-existing tools.

Additionally, Wylie et. al.’s photopaper fills in important gaps in the *data-to-action paradigm*. In some ways, the Jerome Meter is comparable to the Project WIZARDs, the BOMARCs, and the Project MICHIGANs of Chapter 1, in that there is a clear(ish) action involved when experts determine H_2S exposure reaches disastrous levels: stop the disaster. How, exactly, to stop the disaster may be a conundrum in and of itself. But, when there is an emergency and a clear, singular cause of that emergency, we need not worry about the data-to-action paradigm. Data, action, and societal good become more obvious to answer, and I think that feels good to us — not just as scientists, but as people.

Low-exposure to H_2S presents a problem with less implementable solutions, it seems. How much is too much? Which areas and people are most detrimentally affected, and in what ways? How do we even know when to make a decision, and what regulations are in place to back that decision up? The answers to these questions are much less clear and, therefore, so is the data-to-action paradigm. We are not as sure how much data to collect, and where and how to collect that data. It is also less clear what actions we ought take with regard to low-exposure. And, most importantly, how do we determine what is best for people in this case? These questions, in our current Jerome Meter paradigm, are quite difficult to answer, Wylie et. al. show us. The Jerome Meter on its own, for instance, only has one kind of answer to the question of societal good: avert immediate disaster. Wylie et. al. break this paradigm by *beginning* with far more complicated questions of societal good: how do we elevate the stories of individual people affected by perpetual, low-level exposures to H_2S, and use those stories to help us figure out which instruments to build?

One thing Wylie et. al. did not address, however, is how to turn data into action in this case. I have concern that, for these authors, the mere existence of the measurements — via photopaper — appears self-explanatory toward solutions. In other words, once we quantify the individual stories of people, thereby translating them into the language of mainstream science, subsequent action that speaks to those stories will become clear. I question this kind of thinking, and scholars like Sheila Jasanoff and Bruno Latour have long-questioned this as well. In 1994, Jasanoff argued that, when we strive to reach a scientific consensus by adding more and more kinds of data, what often results is a "[polarization] of scientific opinion": "Far from promoting consensus, knowledge [...] risks being fractured along existing lines of discord" (8). In 2004, Latour argued that, rather than going out into nature and collecting more evidence that we bring back in the form of analyses, we ought to reframe environmental research altogether. Environmental research, he argued, was political research all along (52). Arguably, though, Wylie et. al. have made a big move toward Latour's vision.

Of course, I have critiqued critique (a move that originated with Latour as well), and so I must make sure that my own critique is purposeful. I would not have Wylie et. al. do anything differently. I would only caution that their research, as it doesn't (and perhaps can't, as it already does so much that is new) deal directly with the ways by which we might turn that new photopaper data into action, someone ought to (perhaps Wylie et. al. would write a follow-up paper, or collaborate with scholars whose research is in that arena). This is to say that, we should start thinking of research projects that don't deal with each aspect of the *data-to-action* paradigm as incomplete. This would require leaving the paradigm, ultimately: a move that would require a cultural shift not just in science, but across all disciplines in collaboration with each other. This is not something Wylie et. al. could enact on their own.

Engaged STS, Remotely-Sensed Measurements, and Climate Change

I wonder what it would look like to build an intervention like Wylie et. al.'s into Remote Sensing and STS. The research itself, for starters, is an interdisciplinary endeavor. This is evidenced by the types of researchers involved in the paper: Wylie and her students (Elizabeth Wilder and Lourdes Vera), as well as an expert in environmental testing (Deborah Thomas) and a citizen scientist from Wyoming (Megan McLaughlin). Currently, the makeup of environmental Remote Sensing is interdisciplinary: experts on the building of instrumentation (electrical engineers) and the needs it addresses (environmental scientists); experts on the archiving and analysis of data (computer scientists and engineers). However, there are few who focus on not only the ways in which the resultant data products and analyses are created, but on the reasons *for which* they are created, and what they ought to do in the world. I argue that, for this reason, the Remote Sensing community should include STS practitioners (and practitioners-in-training, as the Remote Sensing field at Virginia Tech has made me one of its own), and people in the communities affected by the instrumental research, data collection, data analysis and interpretation, and action-based results.

I have a few preliminary suggestions for *diffractive interventions* into STS and Remote Sensing, based on the theories/parameters set up by Downey and Zuiderent-Jerak, Haraway, Barad, Vallor, and Henderson, and in-line with the Wylie et. al. piece. I do not intend my preliminary ideas to be a prescriptive. Rather, they are suggestions for starting places toward future collaborative, sociotechnically-based projects of engagement in environmental remote sensing.

Expanding Normal Science Outward, to Different Scales

Many papers in Remote Sensing are small technical amendments to theories, instruments, techniques, and analyses (and sometimes amend more than one of things, if not all of them). This is not unlike analytic philosophy, where projects act as small and important amendments to theories that came before them. It is also not unlike STS, in which each publication is a small attempt to change and to build on conceptions of expertise, of knowledge-creation, of power in the institution of technoscience, put forth by scholars who came before us. All three of these kinds of research are the work of normal science (see pg. 99, earlier in this chapter, for a reminder of this definition). We all, in our academic territories, are building new things on top of the foundations that have already been given to us, and are simultaneously checking those foundations for structural issues. But we ought not forget about our neighbors, also building and checking their foundations for issues, in a neighborhood where we are all doing this, and where our infrastructures all rest upon common ground. There must be something in the soil, the air, the culture we all share, that affects the efficacy of that foundation, and that we should all talk about together.

Here is my worry for both Remote Sensing and STS: there are many small amendments to coefficients, to analyses, to data and instrumentation; to theoretical worlds, maps of power, and understandings of cultural narratives. The amendments in Remote Sensing fix some kind of inaccuracy, i.e. standardizing evapotranspiration for a general kind of crop, or incorporating LiDAR models to better understand tree canopy structure, and that is a good thing. But it is easy to lose sight of why we care about this in the first place, when we become so focused on the coefficient; the analysis; the data; the instrument. What narrative does this story help us tell? Here, I'll invoke the Wylie et. al. piece: the argument is not to get rid of the Jerome meter. We need the Jerome meter. The Jerome meter, on its own, however, only tells one kind of story. The argument, rather, is to use photopaper *in tandem* with the Jerome meter, so that more kinds of stories can be told about H_2S leakage, by more kinds of people. This is an issue of accuracy too: we must do this so that we can adequately measure more *kinds* of H_2S exposure.

I imagine a somewhat-parallel dynamic with Remote Sensing. We should not stop using Landsat data for studies, for instance, about crop coefficients and evapotranspiration. Or LiDAR, or hyperspectral data, for additional understandings of our vegetation, our soil and, more broadly, our resources. And we should definitely not stop conducting those studies in the first place. Rather, we ought to use this data — Landsat, LiDAR, hyperspectral, what have you — to shape a story with intentionality: to actively connect these stories about micro-dynamics to larger narratives about the environment as well — narratives like environmental change and degradation, about food security and sustainability across different boundaries: cities, states, countries, ways of life. My vision is in-line with a priority put forth by the 2017-2027 Decadal Survey for Earth Science and Application from Space, put forth by the National Academies of Sciences, Engineering, and Medicine. "A changing Earth is one we can never understand just from past experience," the survey reads.

> It's evolving and emerging characteristics must be continually explored through observation. Our scientific curiosity must seek and reveal the new and altered processes that will result from change, if we are to continue applying our knowledge effectively *for society's benefit*. Decisions we make this decade will be pivotal for predicting the potential for future changes and for influencing whether and how those changes occur. Embracing this new paradigm of understanding a changing Earth, and building a program to address it, is our major challenge for the coming decade and beyond (4; 2018, italics are my own).

Here is a reminder of this data-to-action paradigm: not only of its existence, but that it must be resolved. In other words, it is crucial that we in environmental remote sensing keep an eye toward larger-picture, action-oriented narratives as we discuss "a changing Earth," and "how we might address" that changing [E]arth. Furthermore, this imperative has been building for quite some time. Twenty years ago, Paul Edwards, an STS-er and an expert on issues of climate change and big data, wrote that "[c]omputer models are arguably the single most important tool of global climate science," and that "[m]uch of climate science could not exist without them, since by definition, world-scale processes cannot be studied by controlled laboratory experiments" (438; 1999). Edwards does not go here in this paper, but his remarks conjure the tension between the global and the local; the birds' eye view and the individual view; the problem of scaling from long-term models to individual and local solutions that we might work toward collectively. Environmental degradation and resource scarcity can only really be seen from above, across long periods of time, with the data and analysis that earth remote sensing has provided us — and, in part, that Landsat and satellites like it have provided us. Therefore, these projects — even the ones that do not seem immediately related to climate change, like the Allen et. al. paper — reveal important pieces of a larger narrative to us.

These scaleable connections, between smaller-scale studies and larger-scale pictures of the environment, do not happen on their own. They must be prioritized in earth remote sensing. However, turning data into narratively-compelling action — and connecting analyses across subdisciplines toward a wide variety of needs across the globe — is no easy feat, and cannot be done in 1, 100, or even 1,000 publications. Furthermore, I do not believe this is an issue that other stabilities of earth remote sensing must deal with in the same way: specifically, Landsat (or any other kind of digital Earth) data used to cross check reconnaissance or countermeasures, or even identify swaths of land where oil and gas may be present. As I pointed out in Chapters 1 and 2, those Remote Sensing *stabilities* do not have to deal with scale in quite the same way. Therefore, the field — as a whole, I believe, has not had to contend with these issues. I imagine that those who take these kind of interdisciplinary projects on, for that reason, may be met with some resistance.

STS has an analogous problem. We are often so busy retooling our visions of idealized systems of justice and equitability that we would refuse the workable solution over the one that sounds the best. In different hands, this analysis of Landsat's instrumentation — and of the narratives that underlie them — might have looked different. It might have been stubborn in its treatment of Landsat's military history, and it might view Remote Sensing — and satellite instrumentation more generally — as irredeemable systems of military-industrial power, for which it will always be impossible to achieve bottom-up (rather than top-down) interventions.

As a scholar of Remote Sensing, I know that this is simply not true. In fact, it is its own kind of oversimplification. Many of the folks who do Earth Remote Sensing — and, specifically, those who work with Landsat — have a deep commitment to ecological justice. Many of them, too, are aware of the complicated nature and history of satellite systems, and of the complicated nature of conservation itself (perhaps, more aware than some STS-ers — in fact, they may see our analyses of power as a kind of simplification of these systems). They work with this top-down view because they see it as the most practical, large scale way *to* intervene. Just like STS-ers, we are limited in certain ways by our disciplinary understandings of what is possible (and fundable), and acceptable scholarship in our fields.

In many ways, I am able to see this because of comparable problems in STS, that diffract through Remote Sensing to show me certain kinds of patterns (and vice versa).

The truth is, professionalization in Remote Sensing means perpetuating the data-to-action paradigm, just as professionalization in STS means perpetuating the critique-to-action paradigm. The canonized form of STS calls for us to constantly make critiques — without considering how they might actually play out in their sites of intervention — just as the canonized form of Remote Sensing calls for us to focus on more data and more accuracy, over intentionally building out the processes between data and action into our research, or considering that societal good is not a single, unified thing for people at the long-term, global scales with which Remote Sensing is concerned.

The good news is this: both STS and Remote Sensing are fairly new, interdisciplinary endeavors. They have not been around for hundreds of years, and thusly canonized in long-term ways. Our habits are not as ingrained as they may be in older, non-interdisciplinary fields. I believe, therefore, that it will be easier (but not easy) to resolve the data-to-action/critique-to-action problems. I see this site of diffraction, therefore, as a good place to start. In this way, this project is meant to be connected with other ones that address some form of the data-to-action/critique-to-action problem, and not necessarily at the same sites of intervention.

I offer more questions than answers, here. How do we begin to do this, in STS and in Remote Sensing, and beyond? What would it look like to build action-oriented processes that are *attuned* to sociotechnical systems into Remote Sensing — *and* STS — projects? I am stopping here because I am a *modest witness*. I don't have all of the answers to these questions, and I cannot answer them on my own. They are interdisciplinary questions that require partnerships with people who think very differently than I do, and that require continuous, long-term engagement. This is what I have been doing for the past five years. It wouldn't feel right to pretend I have the answers, in order to give this project the illusion of being finished. In fact, the illusion of the *finished* project perpetuates the data-to-action/critique-to-action paradigms that I wish to move beyond.

However, I can offer some reflections about what it is to do *diffractive STS* across two very different communities. I have been doing this for the last five years, across the STS and Remote Sensing community. In the conclusion, I will provide some of my thoughts on what I think it looks like to do this work, and some additional thoughts on other kinds of scholars that ought to continue this work where I left off.

Conclusion:
Where have we been? And where do we go from here?

Here's where we've come from. I've argued that there exists a *data-to-action* paradigm in Remote Sensing and, more broadly, in technoscience. In Chapter 1, I placed this narrative in a historical context, within Landsat's instrumentation. In Chapter 2, I further contextualized the multiple narratives in Earth Remote Sensing, and within the Landsat satellite system, through a deep reading of Landsat-related (and other Remote Sensing-related) publications, new and old. Here, I pinpointed the manifestations of this data-to-action paradigm throughout the field. Finally, in Chapter 3, I argue that I notice these issues in Remote Sensing because comparable problems exist in STS: specifically, there is a *critique-to-action* paradigm. To address the issues of these two paradigms in a simultaneous manner, I've developed a *diffractive* framework within STS. I ended by offering a starting point to solutions, rather than solutions themselves, as I believe any solutions must be interdisciplinary collaborations, involving many voices in a variety of fields.

What does it mean for one to do *diffractive STS*? From an experiential standpoint, it means feeling a little uncomfortable in both communities, all of the time. It means wishing the two communities had shared language to talk about the things I've come to realize they both care about (in this case, environmentalism and postphenomenology). It means a constant frustration with the ways in which both interdisciplinary fields have become professionalized. It means being wrong, a lot. I began this journey in *diffractive STS* long before I knew how to do it, or even what it was. In my very first semester at Virginia Tech, I was introduced to both of my communities of theorization and practice.

For instance, one of my very first classes at Virginia Tech was a 5000-level Electrical Engineering class (which contained experienced undergraduate seniors and graduate students). I had never taken an Electrical Engineering class in my life, let alone a Physics class. There was a lot that was confusing to me, and not because Electrical Engineering is inherently harder than STS, but because there was a whole disciplinary language that I had to learn in order to begin to understand the course material.

My favorite example of this is a conversation I had with Dr. Joseph Baker, the professor of the class. He was very patient with me while I was learning the material, and I was always in his office asking questions. On one particular day, I was confused about Maxwell's Equations. Maxwell's Equations, for those of you who don't know, are four equations that explain the behavior of electricity and magnetic fields (and the ways in which they interact with each other).

There are dots and x's between the symbols in these equations. These dots and x's, I thought, represented multiplication. That is what I had learned them to mean in time I'd spent with equations. What they actually represent, Dr. Baker explained, are the dynamic behaviors of electric charges and magnetic fields. Multiplication, I'd learned, wasn't a dynamic to consider when thinking about Maxwell's equations. These equations were about the behaviors of invisible forces, and not about numbers. This was why I was having trouble understanding them.

In short, I wasn't thinking about Maxwell's equations with the right descriptive language. However, it took a while for Dr. Baker and I to figure this out: some negotiation and some question-asking. This interaction was about my learning of Maxwell's equations, but it was also about something more: the question of how two people with very different ways of understanding the world arrive at a description of the same phenomenon. This is particularly tricky when one person's language lends itself quite well to describing that phenomenon, but is not understandable — or is simply new — to the other person. Getting those people on the same page is a new kind of learning; a new kind of pedagogy.

This moment was my first experience in being *diffractive:* in understanding two disciplines through each other. I learned Maxwell's equations, of course, but I learned them as an STS-er in a Remote Sensing world. This means that I learned things about STS and Remote Sensing — about what's important to each field, what each field takes for granted, how they imbue meaning into words and symbols — through this interaction. This new type of learning focuses not only on translation between STS and Remote Sensing, but on the diffractive insight gained in such a translation. The translation and the subsequent insight are made possible only if one is an active learner in both fields, because discovering the boundaries of both fields prompts such insights. This *diffraction* makes room for further interdisciplinarity, which then allows those who are diffracting to use that interdisciplinary to create a problem-solving space.

To be clear, there are many kinds of interdisciplinarity. However, *diffraction* is a particular kind of interdisciplinarity: one in which we read two or more disciplines through each other. That specific combination of (inter)disciplines creates a specific pattern, which allows us to recognize a specific set of problems. I had similar moments, even, in my own advisor's Science & Technology Policy class, another required course for everyone in the Remote Sensing Interdisciplinary Graduate Education Program at Virginia Tech. I struggled, at first, to find STS language that would help me explore issues relevant to Remote Sensing. And I observed, too, that STS-ers had trouble communicating their ideas to scientists from my IGEP, and from other departments. We talked past each other a lot, and I noticed my advisor doing frequent work to translate between the two groups. In this case, he was the one being *diffractive*.

Of course, I think a diffraction between STS and Remote Sensing, while perhaps bearing some similarities to other kinds of diffractions, is its own beast. If I diffracted STS and pharmacology through each other, I imagine I would find different sets of parallel of problems, rather than those parallel problems of data-to-action and critique-to-action. The parallel nature of these problems allows us to define them more clearly, to understand them through different lenses, and to collaborate with different kinds of thinkers to decide what to do about those problems. In a way, this is a different kind of *ground-truthing* or *data validation*: one in which we check the lenses of different fields not *against*, but *through* each other. This creates the expectation that, by opening up this space to include more ways of thinking, we'll have a better, more informed sense of what to do. Quite simply, interdisciplinarity helps us solve problems.

With *diffractive STS*, this *checking through* cuts both ways. With the STS community, I feel a strong emphasis on the social constructedness of technoscience; on augmenting experiences of marginalized groups; on democratizing technoscientific worlds. However, I don't see many negotiations between these critiques — and the fields for which we provide the critique — on how to best solve the problem. The rhetoric about bringing justice or equity to bear on the situation is there, but there is often no plan for how we might go about doing that. Similarly, within the Remote Sensing community, the emphasis is on understanding the design and building of satellites; on data analysis and data validation; on matching best kinds of data (and analyses) with particular Earth systems. However, there isn't a focus on the ways in which we can bring these instruments, data, and data analyses to life by taking our findings into communities (large-scale and small scale), and building practices around those findings.

This, too, would require negotiations with given communities at different scales, and across (inter)disciplinary boundaries. And, again, the rhetoric about bringing societal good to bear on the world is there, but there is no plan for how we might do that. This is to say that disciplines (even professionalized, interdisciplinary ones that draw from a few traditions, like STS and Remote Sensing) are good at creating and proliferating information. However, they are not necessarily good at using that information to solve problems. I belabor these points because problem-solving is not some cliché or hand-wavy notion. It is what would make both of these fields do what we say they will.

To make STS (and Remote Sensing) into problem-solving field(s), I am emphasizing translation and mediation as academic pursuits in and of themselves, and taking the focus, for one moment, off the sole pursuit of creating more data and more information. By way of translation and mediation, I am doing three (3) things:

1. Moving STS from theory to action by positioning theory and action as inseparable, thereby embodying a form of STS theory/action;
2. Intervening in Remote Sensing by embodying a type of diversified data (in other words, data that is not only quantitative or digital or visual);
3. Dealing with the frictions these two embodiments create by articulating a kind of practical interdisciplinarity.

I have been doing these things for the past five-or-so years. And so, in the following pages, I will embark on a the creation of a *diffractive STS* framework by telling you, practically, about what I've been doing. This will include my mistakes and mishaps.

Theory/Action

Three and a half years ago, about a year and a half into my time as an (unaware) *diffractive STS-er*, I made my first presentation to the Remote Sensing community. I was given three to five minutes to talk about my research. This is a difficult task for graduate students developing a theoretical project. For instance, I really only became good at summarizing my project a couple of months ago. From what I have seen, this is pretty typical. So, it is very difficult and nerve-wracking for a 2nd-year PhD student, trying to find her way in two very different communities, to present such a quick-and-dirty summary. How *does* a very junior scholar draw meaningful connections between these two communities who share no common language?

Plagued by this question, I did what any reasonable young interlocutor would do: I attempted to hit everyone over the head with the notion that a Capital-T Truth might not exist. Invoking Daston and Gallison's criticism of objectivity. *Objectivity*, they argue, is a nice-sounding word, but there is an important history to it, it is a constructed and changing notion, and therefore isn't as *real* as we often assume it is (Daston and Galison 2007).[53] With this criticism, I took on the notion of ground truth. Ground truth, again, is the process of checking a digital earth image against an on-the-ground sample (i.e. how this dirt pixel corresponds with this physical sample of dirt). My argument was: we are so hung up on calibrating these two types of data to some notion of objectivity when, really, there is no such thing!

One senior faculty member asked me, *How could I ask such questions without being a scientist?* The (lowercase-t) truth is that I found his question a salient and provocative one that matched my own provocations, while also feeling deeply offended by it. Why was I asking these kinds of questions, given my unique positioning in the group? Why was *I* qualified to ask them? And what did I hope to contribute to the field by asking these questions? Another colleague jumped to my aid while I silently wrestled with these counter-provocations.

This presentation, I should note, was a hit with my Philosophy of Technology family. By theoretical-STS standards, it is a good critique. However, a major objection became clear: *What else would I have these researchers do?* I couldn't answer the

[53] "Scientific objectivity has a history," they write. "Objectivity has not always defined science. […] To be objective is to aspire to knowledge that bears no trace of the knower — knowledge unmarked by prejudice or skill, fantasy or judgement, wishing or striving. Objectivity is blind sight, seeing without inference, interpretation, or intelligence" (Daston and Galison 17; 2007).

question constructively. So, not only did the critique not fully land with the Remote Sensing community, in and of itself, but it became apparent that it wasn't entirely helpful. Of course, if I'm making a list of things I think I need at the grocery store, I will check this list with what I actually have in my kitchen before I leave. In this capacity, ground-truthing — or data validation, as the field has moved to call it — makes sense. It's not foolproof, of course. I will always come back from the store having forgotten a thing or two, or having bought a duplicate or something here or there. But it works most of the time. Who cares if there's a theoretical disagreement with it from someone who isn't in my kitchen, and knows nothing about my shopping list?

I should say that the whole experience was very uncomfortable, and embarrassing, and humbling. After months of moping (and occasionally seething: *Why do we only value kinds of knowledge that can be validated?!?!*, I would sometimes think exasperatedly), my biggest takeaway was that my fellow scholars in Remote Sensing were kind of perplexed by that critique, and had no idea what to do with it. It didn't matter how theoretically nice it was, or how well I incorporated quotations from dense and well-respected STS tomes. The critique didn't do anything, other than serve to continue to create divides in our ways of thinking through interdisciplinary problems.

I actually had a parallel experience at an STS seminar, a couple of months later. I gave the same presentation, this time with an attempt to explain, in technical terms, how the digital Earth image was comprised of wavelengths and band combinations, and how supervised classifications (analyses that "group" like pixels in an image together, i.e. *Here are all of the soil pixels, here are all of the water pixels*, etc.) work. *During* the presentation (yes, while I was presenting), I realized that I wasn't yet very good at explaining all of the technical Remote Sensing ideas and theories to a non-Remote Sensing audience. One of my STS colleagues made this ardently clear after my presentation, exclaiming, *I didn't understand any of that!*

This also frustrated me. I spent the weeks following this episode grumbling to myself about how much more of a task it was to not only understand one field in-depth, but to understand *two* fields in-depth — two fields that are very different from each other — at the graduate level, *and* to attempt to explain them to each other. It occurred to me then that this was just the problem. To be really effective, I was going to have to learn to translate across fields. And I realized after these two seminar mishaps that the ability to translate effectively was going to take some time to develop, likely a couple more years (or more).

What was I going to do? I realized that two things were clear: (1) I was going to have to speak to both fields about each other before I was ready to present a body of research that translated evenly and effectively, and (2) it is just the nature of academia that you are asked to present and recite and write and report over and over, and I was going to have to do some form of this while I worked on a more sprawling project underneath. So, for the next few years, I tried to find ways to teach the STS community (and, more specifically, the Philosophy of Technology community) about Remote Sensing technologies in the language of critical theory, and to teach the Remote Sensing community about humanistic ways of creating knowledge, in a language that took seriously the importance of data, and of technoscience more broadly.

Postphenomenology, I find, connects Remote Sensing and STS. It is also, unfortunately, not a language that the communities share naturally. I learned this when I attended the big 4S (Society for Social Studies of Science) conference a few years ago. 4S is the largest STS conference there is, and is a pretty good representation of the field. When I was there, I felt that I didn't quite fit in, and searched high and low for my favorite band of postphenomenologists. Sure enough, they were gathered in the bar area of the conference hotel, a handful of people who (to me, at least) felt a bit out of place.

But I digress. The theory of multistabilities, developed by Philosopher of Technology Don Ihde, has use for us here. Remote Sensing satellites — and their respective data — are multistable: they have military stabilities, extractive stabilities, and ecological stabilities. However, these stabilities are not discrete, separable things. They are deeply connected through each other. It is impossible, for instance, to invoke an ecological stability without invoking a military one, and/or an extractive one. This is something we can only reckon with once we acknowledge the historical and theoretical (multistable) context of it all.

I found, too, that multistabilities were a handy way to explain the science of Remote Sensing to critical theorists in STS. This looked like using the lens of *multistabilities* to explain remotely-sensed imagery and spectral bands to folks in Philosophy of Technology. It also looked like using the lens of digital image creation and superimposition to explain to folks in Remote Sensing the processes by which one could turn, say, Landsat imagery, into knit-able Fair Isle patterns.[54] This pursuit became a side

[54] See http://samknitstheworld.wordpress.com to learn more about this project.

project called *Knit the World*, and allowed me to open up some space to talk about the ways in which all digitization finds its roots in the Jacquard Loom, and that image interpretation and classification — exactly what I was doing to turn Landsat images into knitting patterns — was just as much an art as a science. In other words, it was always interdisciplinary and sociotechnical.

I would also like to make the following clear: I had absolutely no idea what I was doing. I thought hard about this, of course, but it wasn't a plan I knew how to execute. No one wrote a handbook on engagement between STS and Remote Sensing that I could follow. This just seemed like the way I needed to be at the time; the best way to be *diffractive*. And in hindsight, I am able to explain it as if it were some grandiose plan. It really wasn't. In hindsight, though, I can say that I was **turning STS theory into action**, by blurring the sociotechnical boundaries between STS and Remote Sensing with my work, and by moving about both disciplines with diffractive, feminist values. Recall Karen Barad's thoughts on diffraction, which I block-quoted in the previous chapter:

> [W]hile it is true that diffraction apparatuses measure the effects of difference, even more profoundly they highlight, exhibit, and make evident the entangled structure of the changing and contingent ontology of the world, including the ontology of knowing. In fact, diffraction not only brings the reality of entanglements to light, it is itself an entangled phenomenon. [...] So at times diffraction phenomena will be an object of investigation and at other times it will serve as an apparatus of investigation (73; 2007).

My work was (and still is) to *be* the "diffraction apparatus" that "measure[d] the effects of difference" between the STS and Remote Sensing communities; to "make evident," by way of my engagement with both communities, "the entangled structure of the changing and contingent ontology of the world, including the ontology of knowing" (ibid). By speaking about multi-stabilities and spectral bands — and by treating image interpretation as having a *context* beyond accuracy or validation, and a practice that is both art and science — I was trying to "bring the reality of entanglements to light" (ibid), while being, myself, an entangled scholar.

This wasn't, of course, limited to presentations or other kinds of formally structured moments of teaching/learning. I kept this notion in the back of my mind when asking and answering questions during classes and seminars, when responding to exam prompts, and even in daily interactions and conversations. Really, I tried to hold these theories of *diffraction* in my mind as if they were a theistic guidepost. For instance, in a Spring 2017 exam for my GRAD 3154 course — an interdisciplinary course in Remote Sensing — I was asked to diagram the structure of Remote Sensing as a field. The diagram was to have these parts: input, output, process, and feedback. I, however, don't think Remote Sensing is that linear. And so, I created my own diagram, based on a fractal system known as a *Mandelbrot set*.

Here is how I answered the question:

> In her book Meeting the Universe Halfway, a feminist philosophy of quantum physics, Karen Barad posits that it is impossible to observe a system without interfering with it, and affecting it in some way. As such, I find the premise of this question incomplete. No simple systems drawing could possibly describe the processes of [R]emote [S]ensing, their entanglements with the [E]arth systems and, subsequently, [R]emote [S]ensing entanglements with the human condition. To reduce the process of [R]emote [S]ensing to five elements (input, process, output, feedback) would be a disservice to all of the interdisciplinary work we have done in this class and in this IGEP. Additionally, the [E]arth itself is not really a closed system — it isn't entirely clear where, exactly, the [E]arth ends and where space begins. And as [R]emote [S]ensing unites [E]arth and space through tools that extend our senses, it further complicates our idea of [E]arth as a system that we can cordon off from the rest of the universe (even the Bretherton diagram insinuates this dynamic by including solar/space plasmas in its outermost circles).
>
> I see our knowledge of [E]arth and space systems via [R]emote [S]ensing as something that is best mapped using a Mandelbrot set — or some other fractal depiction of an open system with infinite complexity. Each node in the set represents some important aspect of [R]emote [S]ensing knowledge-creation (for instance, the field itself is the largest node, and its tools of measurement [branch] off from that node). When one zooms in on the

> node that represents tools of measurement, one finds it branches off into nodes that represent satellites, drones, in-situ tools, etc. When one zooms in on the node that represents satellites, one finds satellites built by different institutions: for instance, NASA, NOAA, the DoD, etc. And if one were to look closely at the NASA satellites node, one would find individual satellites, funding sources, research scientists, identified earth systems needs, etc. And each of these aspects of NASA satellite infrastructure would be infinitely undergirded by smaller subgroups of components.
>
> Perhaps the most important connotation of my fractal diagram is this: when we map [R]emote [S]ensing as a process, what we are really doing is mapping knowledge creation. And while any kind of knowledge creation has societal feedbacks — that knowledge is adopted and used in unpredictable and unintended ways — the knowledge itself arises from a specific set of societal conditions. And so in each fractal node of remote sensing processes, which all branch into infinite complexity, you will find deeply entangled human feedbacks. In an emergent, fractal system, everything is entangled with everything. Indeed, [R]emote [S]ensing knowledge is inseparable from the humans that created it, and becomes more and more embedded in our understanding of our own role within earth and space systems over time.

There were many more moments like this, in which I did my best to reflect the Remote Sensing field back to itself as something complex and nonlinear, and as something that creates knowledge that comes from *somewhere*. These moments are small, but essential. Many of them can change the way the field conceptualizes itself. And systemic diagrams themselves (like the Bretherton diagram from Chapter 2) are so important to Remote Sensing. To create a new one *is* to contribute something back to the community in the form of a reconfigured, systemic framework. These kinds of moments are examples of how one does *diffractive STS*, by putting forth its central tenets as theory/action.

Diversified Data

The above-described diagram is also a gateway into another type of *diffractive* intervention: the notion of diversified data. I began thinking about this especially after my awry seminar to my Remote Sensing colleagues. *What was the point of that critique*, I wondered to myself a lot: my critique that ground-truthing (or data validation) was a misguided pursuit. What was really behind it, I realized as I grumbled about my outcast form of knowledge, was my outcast form of knowledge. With this critique, I was really seeking to take the kinds of concerns that STS-ers — and humanists more broadly — might have, and make them *matters of concern* for the Remote Sensing community as well. And so, I began to think about more effective ways of doing just that.

This kind of intervention had to go beyond more traditional moves that STS scholars might make, and appeal to dominant images (Downey 5; 1998)[55] in Remote Sensing while simultaneously subverting them. Of course, it also had to appeal to dominant images in STS, while simultaneously subverting them as well. To be sure, I can't emphasize how difficult it is to engage these narratives with each other already, let alone to imagine alternatives for both fields that might interact with each other.

It is sometimes hard to imagine, for instance, in a canonical STS sense, other ways of engaging beyond showing the Remote Sensing folks how their brand of accuracy is socially constructed; revealing to them the power dynamics of watching from above; making visible narratives of marginalization, colonialization, and militarism (Foucault 1991; Haraway 1992; Litfin 1997; Shiva 1993). Not only is it difficult to imagine alternative forms of engagement, but it is necessary: the Remote Sensing community is already — at the very least marginally, if not completely — aware of all of these things (which I will delve into in a couple of pages). It seems even clearer, then, that awareness

[55] "[D]ominant images," writes Downey, "establish and enforce the terms of everyday theorizing, everyday habits of imagination" (ibid). On the next page, he asks: "What would it take to shift the dominant cultural image to something better, to move it onto new ground?"

of these critiques does not automatically cause those in Remote Sensing to value new and different forms of knowledge; new and different forms of data. So what will?

I spent a lot of time thinking about this, and a lot of time listening. Paying attention. Observing. Not as a participant observer, but as someone *in the Remote Sensing community*. *What do we care about?*, I would often ask myself after Remote Sensing seminars and conferences. After my 2nd Pecora conference in 2017 — a conference around Landsat that happens every three years — the answer became clearer. It helped that this was the first time that I went to presentations and really understood what they were about, at both a conceptual and a technical level. This gave me a new kind of understanding about the field, and I had a realization that may seem obvious to you, but was groundbreaking for me: a significant portion of folks in Remote Sensing are hardcore environmentalists. It was like discovering something I already kind of knew.

I had to understand the technical language of the field (and some of the more informal language, too) as if it were my own, to fully absorb the purpose of these data projects. Sustainable living and environmentalism are the main foci: data is a means by which we might get there. All of these different techniques and refinements assure us that we know how to get there. Data is a form of listening to, caring about, further understanding the things the environment has to tell us. Data helps us figure out what to do. Just like critique helps us figure out what to do. We're not so different, I realized. The STS-er in me knows, though, that quantitative data can't do everything for us (just like the Remote Sensing scholar in me knows that critique can't do everything for us). How to bring in new kinds of data — data that might push us toward some action on these goals of sustainability and environmentalism?

I began to realize that this was my intervention: not in criticizing the data that was already there, but by adding to it: by making the improvisational move of *yes, and*. This would involve the creation of a project that shows both the depth and breadth of remotely-sensed data — and the instrumentation that created it — and the ways in which that data and instrumentation is limited. At this point, I was already reading environmental history, reading about the many uses of Landsat data, and beginning to discover the history of the instrumentation itself. I realized that these were ways of creating *alternative* data: data can be historical context; data can be sociopolitical context; data can even be philosophical reasoning. And so, the first two chapters of my dissertation are just that: an attempt to create new kinds of knowledge in Remote Sensing: **diversified data**, you could call it. Perhaps this is a sort of meta-data. You may notice, too, the ways in which diversified data and an STS theory/action are inseparable from each other (I see them as inseparable in all ways).

Resolving Frictions Through *Practical Interdisciplinarity*

These inseparable modes of *diffractive intervention* culminate in a state of being that I will call *practical interdisciplinarity*. However, this notion of practical interdisciplinarity isn't a new concept by any means. In his article *Interdisciplinary Problem-Based Learning: An Alternative to Traditional Majors and Minors*, Robert Sternberg writes:

> Responding to [...] almost any serious problem at a global or even national or local level [...] requires problem-based, interdisciplinary thinking. [...] In today's world [...] few problems of any significance are either confined or narrow. Rather, they aggressively cross boundaries that render the perspectives and methods of single disciplines incomplete and inefficacious (14; 2008).

Sternberg goes on to write that, to an economist, these big issues may appear as though they'd fit into an "idealized economic model," while the benefits of the idealized economic model for addressing issues at the individual level may be lost on the psychologist (ibid). "Like the carpenter desperately looking for some task in which to use a hammer, the student may come to believe that his or her field provides *the* answers, and that practitioners in other fields have less to offer in the solution of complex problems. **A problem-based approach puts the problems before the tools**" (ibid, bold font my own).

Here is a problem: A couple of years ago, I was at Pecora, a Landsat conference that occurs every three years. I was musing to one of my colleagues that Earth Remote Sensing around Landsat seemed to be a dominantly environmentally-conscious pursuit, and I asked why this wasn't more explicit in the work presented at the conference. They told me that there is a fear, among scientists in Earth Remote Sensing, that research presented as explicitly environmentally conscious veers toward activism. And if a project is perceived as an activist one, it isn't taken as seriously: it is perceived as biased, sullied, and not-trustworthy. I asked them if this bothered them at all, and they expressed to me that this is a central tension among environmentalists in Earth Remote Sensing. Everyone is doing this work because they care about the environment, but they fear that any too-explicit expression of environmentalism would cause them to lose credibility, and possibly prevent them from doing the work they feel passionate about.

This was a defining moment for me, as both a Remote Sensing scholar and as an STS-er. I used to think that scholars in Remote Sensing had a certain kind of privilege that STS-ers did not have: more money, more access to equipment, access to the power of the scientific institution, and so on. But after this conversation, I realized that this was the wrong way to look at things. Critiquing the big-I Institution of Remote Sensing was not quite right either. The problems ran deeper than *the discipline of STS* or *the discipline of Remote Sensing*. Both fields care deeply about *societal good*: it isn't, to most in Remote Sensing and STS, just a buzzword. Rather, it is a description of why scholars are called to do such work. And yet, concepts like *societal good* become buzzwords. They become buzzwords when expanding on notions of societal good — and actions toward it — make it harder for scholars to obtain credibility, respect, and/or funding.

Talking about action toward these ideals, or how such research might lead to some kind of *societal good*, of course, can also disadvantage a scholar's livelihood. STS-ers, I know, share this concern (and frankly, have quite a chip on their shoulder about it — some days, I do as well); this sadness about the limitations of technoscience, and of academia more broadly; this sadness that we don't have the language or the ability, within the infrastructure of our academic world, to have our research address the things we are passionate about to their deepest extent, while still remaining credible. To address this sadness, we have to begin to rework the concepts of academic credibility, of academic funding structures, and of what it means to be well-published and well-cited. This is no small task.

This was the real problem, I realized. And this problem is bigger than just STS, or just Remote Sensing. I can show you the ways in which I think it manifests in those fields, but I believe the problem is actually far more expansive: this *x-to-action paradigm*. The interventions into STS and Remote Sensing — and beyond — must be interdisciplinary. And as we are thinking about actions and outcomes, the intervention must necessarily be practical. This is what I mean when I talk about a practical interdisciplinarity. However, I do not yet know what this looks like in practice. But I am certain that it is what we need to resolve the *critique-to-action* and *data-to-action* paradigms. It is also what we need to engage in more civically-oriented research: research that isn't detached from our most pressing issues. To be clear, I do not see theoretical depth and civic engagement as separable things. Putting theory and practicality in deeper conversation with each other also necessitates interdisciplinarity.

Continuing with Additional Perspectives

I will continue to do the work of *diffractive STS*. There are additional kinds of scholars, though, that I see as being contributors to this project. The most direct intervention, I believe, is the building of processes between data, action, and some agreed-upon societal good *directly* into Remote Sensing projects and publications. This will mean a shifting of narratives beyond accuracy and application, and toward clearer definitions of action-based intervention and societal benefit. Simultaneously, once the field has defined what desirable actions and outcomes might be, it must work to build suggestions for these processes into projects and publications themselves. These processes, I have argued, will not happen automatically. And who better to recommend solutions than the very researchers who are familiar with these problems?

The issue is that Remote Sensing researchers do not have training in thinking about *sociotechnical systems*, and the theoretical frameworks that undergird them. This must be addressed at two levels. Firstly, there is a pedagogical element to this: remote sensing needs courses that teach humanistic and social-scientific ways of thinking, so that they are not so unfamiliar to those in the field (Virginia Tech's interdisciplinary curricula — including the one in the Remote Sensing Interdisciplinary Graduate Education Program — is a good first-step toward this). However, pedagogical intervention is not enough, and sociotechnical awareness does not replace the relevance — and importance of expertise — of actual humanists and social scientists to projects and publications in Remote Sensing. These scholars offer a depth and breadth of knowledge that will be necessary in building processes between data, action, and societal good into projects.

Typically, the conception in Remote Sensing is that data will be implemented at the level of policy. I think this is true, to some extent. However, even the connections between data and policy are not entirely clear. This has not been the focus of my project. However, I believe it should be *a* focus in the deconstruction (and reconstruction) of the data-to-action (and the critique-to-action) paradigm in Remote Sensing and STS. How, exactly, does Landsat (and other Earth Remote Sensing) data inform policy, and by way of which networks? What gets lost in translation, and how? These are a few of the questions that this kind of project would address. Conversely, rather than hand-waving them away, STS scholars tend to deconstruct issues of policy, to the point where one wonders if a solution might ever be possible. The task of the STS intervention, in this case, would be to decide upon which aspects of *our* analyses we'd be willing to sacrifice to come up with a policy-based solution that feels right.

Of course, then there is the issue of: how do we know when something *feels right*? This, I think, has a lot to do with issues in ethics and meta-ethics. Ethicists address those things that do or do not contribute to our collective flourishing, and meta-ethicists decide how we know when decisions are ethically-based. We need this kind of expertise to arrive at more solid understandings of what we mean by *societal good* when we invoke it, in the realm of environmental science and beyond. Until that point in time, I don't believe the Remote Sensing community (or, really, the STS community), can fully or knowledgeably invoke more specific moves toward some societal good.

There are other humanists, with different kinds of expertise, that I think should have a voice in this kind of project. For instance, I provided a very light historical analysis of conservation, as it related to water-use in the arid Western United States. I mentioned that there were deep connections between water, soil, and vegetation conservation. However, I did not provide in-depth explanations of those connections, nor am I qualified to do so (at this point in time, at least). I believe that a deeper historical analysis of conservation as it relates to Remote Sensing — across various kinds of natural resources and global territories — is necessary for a fuller understanding of the *data-to-action* paradigm, and of Remote Sensing itself. This will aid interdisciplinary communities in building stronger frameworks to address this paradigm, whose roots, I imagine, are much deeper than military histories of Landsat.

Additionally, I addressed the military roots of Landsat — and of remote sensing more generally — as they relate to the stabilities offered by the satellite system. However, I only breezed over the concept of extraction, and what that looks like — or ought to look like — if we address it in a fair, interdisciplinary way. Of course, extraction is a broad concept, and some kinds of extraction are more innocuous than others. What would it look like to extract/preserve in a way that is most sustainable for our environment? This is an ethical *and* an ecological question, and there may even be scholars who answer these questions directly (as this is not my area of expertise, I am not aware of them).

There is also more research to be done on oil and natural gas companies, the ways in which they extract, and the ways in which they use remotely-sensed data *to* extract. What techniques do they use? What higher- and lower-resolution instruments to they use, and in what ways? What is their understanding of the precarity of extraction vs. preservation, and how do different kinds of policies and standards provide infrastructure for such understandings? I suspect the answers to these questions are more nuanced than we realize and, if we were to look at them with a lens that is situated within both STS and Remote Sensing, we may be able to better speak to that nuance. Additionally, oil and natural gas companies sometimes conduct their own ecological research. What is this research like? What kinds of instruments do they use to conduct this research? And what do we see when we diffract this kind of research through one that we see as a more *pure*, less privately-motivated ecological research? These are all highly disciplinary questions, and must be answered by fields across the academic spectrum — and beyond.

Then, of course, there are the more practical questions: How do we actually go about building processes that take us from data to action *into* environmental Remote Sensing data analyses? How do we standardize such a practice within the field, and across publications? This, too, is an interdisciplinary question, and a question of professionalization. To study it, it may be useful to understand how other major standardizations — or norms, perhaps — across technoscientific fields came to be. We could also look to the very history of Earth Remote Sensing (and Landsat) for some hints: perhaps symposia, not unlike the ones held by the Environmental Research Institute at Michigan (ERIM) could be good places to start. The symposia could be open or advertised to a wide variety of backgrounds and expertise. That way, one way of thinking about the issue isn't overvalued.

And while we are on the subject of practicality, there is the issue of funding. How would we position this new kind of Remote Sensing research — that reconstructs the data-to-action paradigm — in a way that is fundable? This involves research on agencies that provide funding for Remote Sensing research (including the DoD). What do different agencies and institutions (DoD, NSF, NASA, USGS, AGU, private companies, and more) find important, and why? What agencies have values that best line up with this new kind of Remote Sensing research, and how could we strategically frame such projects in ways that make the fundable? As I noted in Chapter 2, for instance, the DoD considers environmental change to be an issue of national security. Perhaps the *data-to-action* paradigm could come full-circle, and end with one of the institutions where it began. Some STS-ers will cringe at this. I will double-down and say that I am not an activist: rather, my academic stance is to find solutions within the system.

My project is the way it is because of my own personal inclinations and talents. However, I believe that environmental activism is *crucial* to various extensions of this project, and that environmental scientists should not shy away from activism. While it's true that satellites will always have military and extractive applications — and that those applications are not as divorced from the ecological ones as we might think — we should still fight toward a future where we emphasize clean energy, use of renewable resources, sustainable practices. And we must push on those shifts from the outside, as much as we try to reconfigure them from the inside. We should continue to push an agenda that embraces climate science, and that acknowledges that climate change is real, and caused by humans. We should also continue to acknowledge our position of power within the broader scale of satellite-based science, as one that has specific national interests in mind. Those interests often that do not consider the needs of the marginalized, or those without access to similar kinds of technologies. This coordinates with our embrace of the notion that not all technological problems require technological solutions (Postman 1992). Some of us, like me, are perhaps more quiet about these kinds of political agendas, and I believe that those in the Remote Sensing field are too: not because they want to be, but because they have to be. We ought to recognize those positionalities across institutions, stay strong in our value systems, and find our allies across different styles of advocacy and activism. We share similar goals, and therefore ought to work together where we can.

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