Science as Prophecy:
Paleo Perspectives on Environmental Change

By

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Abstract

This dissertation explores developments in paleoecology, a scientific subfield which reconstructs past environments and attempts to determine the mechanisms of environmental change. In particular, this dissertation examines how and why scientists who studied environmental change through deep time came to forecast environmental futures. I argue that various environmental disasters – desertification, mass extinction, and climate change – spurred ecologists to use the past to look to the future in order to understand and mitigate threats from anthropogenic change. I further contend that, although these scientists sought to forecast the outcome of these disasters, there was a constant struggle to establish whether historical data, which formed the basis of forecasts, properly captured past events and could be used to predict the future.

The historical methods at the heart of paleoecology mainly involve the use of proxy evidence: fossilized columns of microorganisms such as pollen, algae, and sloth dung, all of which can be used to indirectly measure past environmental change. Proxy evidence has become a key way of knowing past ecological or climatological conditions, but we do not yet have an account of how scientists reason with these indirect data. My dissertation offers the first account, showing how work with proxies led to different perceptions and framings of the environment, and environmental problems, over the course of the twentieth and twenty-first centuries.

Acknowledgments

When I came to Wisconsin, I did not expect that one day I would ride behind a snowmobile packed in with various piston corers. But a series of fruitful discussions with my advisor, Gregg Mitman, as well as with friends in the History of Science and at Center for Culture, History and Environment (CHE) quickly had me hooked on fossil pollen and the environmental past. I was soon exploring the history of paleoecology in various archives and, thanks to paleotea, I was also learning about the discipline as it develops today.

Various fellowships allowed me to complete the archival research for this project. Once at these archives, I always found myself in good hands. In particular, I would like to acknowledge the support of the Elmer L. Andersen Research Scholars Award from the University of Minnesota, which allowed me to examine Margaret B. Davis archives. With the help of Erik Moore, Head of the University of Minnesota's Archives, I also found myself in a vault below the Mississippi River, cutting open newly arrived boxes of materials belonging to Herb Wright Jr. That experience made for a special archival trip. The Consortium for the History of Science, Technology and Medicine also supported my archival work, funding research trips to Yale University, the Smithsonian Institution, the Academy of Natural Sciences of Drexel University, and the Linda Hall Library. Mary Caldera at Yale helped me cross-reference conflicting finding aids, Jennifer Jett at the Smithsonian helped digitize materials so that I could concentrate on research (and she introduced me to the triceratops holotype not far from her office), and Benjamin Gross and Tania Munz at Linda Hall ensured I found some really wonderful material, and some delicious lunches. The Howard T. Odum Research Travel Grant from the University of Florida allowed me to visit the Edward Smith Deevey collections, while the Center for Culture, History, and Environment and the University of Wisconsin-Madison

Graduate School supported various research trips. One of these trips was to the University of Arizona, where Wendell Cox helped me sort through two substantial collections and hike in the desert that Paul Martin had once called home.

Friends and colleagues helped me turn all this material into a dissertation, and made sure I had a good time doing it. From the start, Irene Toro Martinez, Spring Greeney, and Daniel Liu made me welcome in Madison. My dissertation writing group (Daniel Liu, Vicki Fama, Bridget Collins, Nick Jacobson, Molly Laas, and Stephen Neal) helped guide the project. Dana Freiburger made sure that I had a place to sleep and a scrambler to eat, fuel for long writing sessions. Rachel Gross, Kate Wersan, and Sarah Dimick helped me work out various arguments, and Megan Raby, Andrew Stuhl, and Anna Zeide showed me the path forward. Friends at CHE connected me back to the things and ideas that mattered to me. Scientists in the Hotchkiss lab eagerly welcomed me into their midst, always keen to answer a question about their discipline or read an old historical paper with me. For that, I thank Sara Hotchkiss, Randy Calcote, Patricia Sanford, Marjeta Jeraj, Shana Ederer, Kristin Michels, Soo Hyun Kim, Kevin Barrett, Lisa Schomaker, Katie Laushman, and Rachel Brunner. Writing fellowships from the Institute for Research in the Humanities, Grey Towers, the University of Wisconsin-Madison Graduate School, and the Social Science and Humanities Research Council of Canada gave me time to concentrate on these ideas. A predoctoral fellowship at the Max Planck Institute for the History of Science allowed me to continue writing and introduced me to a wonderful group of scholars, including Raine Daston, David Sepkoski, Brooke Penaloza Patzak, Vincent Deluz, and Christian Flow. Faculty at Wisconsin shaped this project, especially discussions with Nicole Nelson, Tom Broman, Lynn Keller, Bill Cronon, and Rob Nixon. Jim Fleming at Colby College and Gene Cittadino at NYU also provided key insights. Friends at LGBT Books to Prisoners (especially

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Most of the heavy lifting for this project came from my committee who went out of their way to make sure that everything came together and that I had the support that I needed along the way. Liz Hennessy pushed me to figure out the frame for the project. Sara Hotchkiss did her best to make sure I grasped the technical aspects of paleoecology, understood developments in her field, and read and re-read with care. Lynn Nyhart was always available to read and discuss ideas. Her perceptive questions helped me clarify some of the key issues and her careful reading always improved my writing. From the beginning to the final hour, Gregg Mitman provided invaluable support, ensuring that I had the intellectual and financial backing to undertake this project. He always had an idea or new body of scholarship to consider, which helped me step back and reevaluate the implications of the history of paleoecology and proxy research. This project was significantly enriched by his guidance.

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Introduction:
Prophecies and Proxies

At the end of the nineteenth century, ecology emerged as a named discipline. This new field initially sought to understand plant dynamics by measuring the factors that influenced the development and stability of individual plants as well as vegetation communities.[1] By 1916, some ecologists thought that they would better understand these dynamics if they traced the progress of vegetation communities over longer time periods. These ecologists proposed using the term “paleoecology” to describe ecological research which studied ecological change over thousands, or tens of thousands, of years.[2] Paleoecology differed from ecology because it studied ecological phenomena beyond the realms of direct observation and experiment, but, like its parental discipline, paleoecology aimed to document processes that change over time and to identify the causes of change.[3]

Yet ecologists and paleoecologists did not just document and account for change. As historian Sharon Kingsland has argued, practitioners pursued ecological topics because “ecology promised to address questions dealing with the control of life and the problems of adaptation in

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[1] Donald Worster, *Nature’s Economy: A History of Ecological Ideas* (San Francisco: Sierra Club Books, 1977); Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900* (Cambridge: Cambridge University Press, 1990); Sharon E. Kingsland, *The Evolution of American Ecology, 1890-2000* (Baltimore: The Johns Hopkins University Press, 2005); Gregg Mitman, *The State of Nature: Ecology, Community, and American Social Thought, 1900-1950* (University of Chicago Press, 1992); Robert P. McIntosh, *The Background of Ecology: Concept and Theory* (New York: Cambridge University Press, 1985).

[2] Frederic Clements, *Plant Succession: An Analysis of the Development of Vegetation* (Washington, DC: The Carnegie Institution of Washington, 1916); Frederic E. Clements, “Scope and Significance of Paleo-Ecology,” *Geological Society of America Bulletin* 29, no. 1 (June 30, 1918): 369–74. Most paleoecological studies focused on the last 20,000 years or so, where records are more easily accessed. Clements encouraged ecologists to go deeper; he believed that ecologists could trace climate and vegetation over millions of years. On the time period studied by paleoecologists, see Alistair W.R. Seddon, “Paleoecology,” *Oxford Bibliographies*, May 23, 2012.

[3] Scientists have used the term “paleoecology” to refer the study of many different aspects of past environments. This means that various paleoecologists have argued that “paleoecology is a nebulous field; paleoecology is a term meaning different things to different people.” See, for example, David R. Lawrence, “The Nature and Structure of Paleoecology,” *Journal of Paleontology* 45, no. 4 (July 1971): 594. In this dissertation, I use paleoecology to describe research aimed at reconstructing past environments and research aimed at determining the mechanisms of environmental change. I primarily look at paleoecology that used pollen analysis as method.

diverse landscapes."[4] Those who studied ecology hoped that insights from their field could be applied. This desire for applied ecology was particularly pronounced in America where people were settling new regions and hoping to exploit new resources.[5] With a better understanding of ecological processes, settlers thought that they would be able understand, predict, and control those processes, thereby improving their ability to thrive in new landscapes.

While ecologists had spent a couple of decades trying to predict and control where plant and human communities were headed, during the Dust Bowl of the 1930s they advanced a program where ecology would play an active role in shaping the future.[6] During the severe drought, ecologists, particularly those with an interest in paleoecology, became especially vocal about how humans had modified vegetation development. They warned about the ecological disaster that was likely to occur if people did not adopt ecological principles that would restore the natural development and stability of vegetation communities.[7] This kind of argument, that ecology could properly manage the future, became one of the main ways that ecologists

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[4] Kingsland, *The Evolution of American Ecology, 1890-2000*, 98. On notions of control in the life sciences, see, for example, Mitman, *The State of Nature*; Philip J. Pauly, *Controlling Life: Jacques Loeb & the Engineering Ideal in Biology* (New York: Oxford University Press, 1987); Helen Anne Curry, *Evolution Made to Order: Plant Breeding and Technological Innovation in Twentieth-Century America* (Chicago: University of Chicago Press, 2016); David P. D. Munns, *Engineering the Environment: Phytotrons and the Quest for Climate Control in the Cold War* (Pittsburgh, PA: University of Pittsburgh Press, 2017); Luis Campos and Alexander Schwerin, "Transatlantic Mutants: Epistemics, Evolution, and the Engineering of Variation 1903-1930," in *Heredity Explored: Between Public Domain and Experimental Science, 1850-1930*, ed. Hans-Jörg Rheinberger and Staffan Mueller-Wille (Cambridge, MA: MIT Press, 2016), 395–415; Donna Jeanne Haraway, *Primate Visions: Gender, Race, and Nature in the World of Modern Science* (New York: Routledge, 1989); Gregg Mitman, "In Search of Health: Landscape and Disease in American Environmental History," *Environmental History* 10, no. 2 (2005): 184–210; Susan D. Jones, "Mapping a Zoonotic Disease: Anglo-American Efforts to Control Bovine Tuberculosis before World War I," *Osiris* 19 (2004): 133–48; Helen Tilley, "Ecologies of Complexity: Tropical Environments, African Trypanosomiasis, and the Science of Disease Control in British Colonial Africa, 1900-1940," *Osiris* 19 (2004): 21–38; Peder Anker, *Imperial Ecology: Environment Order in the British Empire, 1895-1945* (Cambridge, MA: Harvard University Press, 2001).

[5] Ronald C. Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955* (Berkeley, CA: University of California Press, 1981).

[6] Donald Worster, *Dust Bowl: The Southern Plains in the 1930s*, vol. 25th Anniversary Edition (New York: Oxford University Press, 2004).

[7] For examples, see Paul B. Sears, *Deserts on the March* (Norman, OK: University of Oklahoma Press, 1935); Frederic E. Clements, "Experimental Ecology in the Public Service," *Ecology* 16, no. 3 (July 1, 1935): 342–63.

promoted the importance of their discipline as they engaged with various environmental disasters. As they worked on problems like desertification, extinction, and climate change, ecologists explicitly argued that the purpose of ecology was to foretell the future so that the direction of change could be properly managed. As I argue, insights from timescales longer than individual human experience were particularly important in these forecasts: in an article about the goals of paleoecological reconstructions, Ohio ecologist and early promoter of ecological studies of long-term change, Paul Bigelow Sears, argued that the basic function of science, and ecology in particular, was "to give perspective to the human adventure and from that perspective to provide direction for the long future."[8]

While historians like Kingsland have documented ecologists' interest in prediction and control and other historians have described the increasing desire for predictions in the earth and environmental sciences through the course of the twentieth century, this scholarship has not given much space to the nature of scientists' forecasts, the methods scientists use to foretell the future, and the reasons why scientists' predictions have become an acceptable way to examine the future.[9] This dissertation attempts to fill in these gaps by examining the differences between ecological prophecies and predictions and their relation to the historical methods that formed the

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[8] Paul Bigelow Sears, "The Goals of Paleoecological Reconstruction," in *The Reconstruction of Past Environments*, ed. James J. Hester and James Schoenwetter, Proceedings of the Fort Burgwin Conference on Paleoecology: 1962 3 (Fort Burgwin Research Center, 1964): 4–6.

[9] Kingsland, *The Evolution of American Ecology, 1890-2000*; Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970," in *The Struggle for the Long-Term in Transnational Science and Politics: Forging the Future*, ed. Jenny Andersson and Eglė Rindzevičiūtė (New York: Routledge, 2015), 38–62; Libby Robin, Sverker Sörlin, and Paul Warde, *The Future of Nature: Documents of Global Change* (New Haven, CT: Yale University Press, 2013); Naomi Oreskes, "Why Predict? Historical Perspectives on Prediction in Earth Science," in *Prediction: Science, Decision Making, and the Future of Nature*, ed. Daniel Sarewitz, Roger A. Pielke Jr, and Radford Byerly Jr (Washington, D.C.: Island Press, 2000), 23–40; Naomi Oreskes, "From Scaling to Simulation: Changing Meanings and Ambitions of Models in Geology," in *Science Without Laws: Model Systems, Cases, Exemplary Narratives*, ed. Angela N.H. Creager, Elizabeth Lunbeck, and M. Norton Wise (Durham, N.C.: Duke University Press, 2007), 93–124; Katrina Forrester and Sophie Smith, eds., *Nature, Action and the Future: Political Thought and the Environment* (Cambridge: Cambridge University Press, 2018); Matthias Dörries, "Politics, Geological Past, and the Future of the Earth," *Historical Social Research* 40, no. 2 (2015): 22–36.

basis of these forecasts. It briefly examines the reception of these forecasts to understand how policymakers and the public viewed conclusions from methods that were regularly described as uncertain.

These three themes – forecasting, methods, and authority – come together in the answer to the central question that this dissertation attempts to solve: how did scientists who studied environmental change through deep time come to forecast environmental futures? I argue that various environmental disasters – desertification, mass extinction, and climate change – spurred ecologists to use the past to look to the future in order to understand and mitigate the threats from anthropogenic change. I further contend that, although these scientists sought to forecast the outcome of these disasters, there was a constant struggle to establish whether historical data was a useful forecasting tool. These methodological problems meant that the value of history, as well as the status of paleoecology, were both questioned.

My argument focuses mostly on the subfield of paleoecology, especially paleoecologists who relied on proxy evidence, an indirect measurement of environmental change, to form conclusions about the past. Paleoecological forecasts are worth examining because paleoecologists were some of the most likely ecologists to forecast; they claimed that their studies of the past gave them perspective to project to the future. Further, the proxy evidence that these paleoecologists use in these forecasts has become one of the main ways of understanding long-term environmental change more generally: much of the data we have that describes ecological or climatological conditions over long time periods has been obtained with a proxy of some kind because instrumental and historical records are often not available. Despite the importance of these forecasts and the ubiquity of these data, we still do not have an account of how practices like the measurements of changes in the composition of fossil pollen or algae communities over time, and the resulting inferences about past and future vegetation and climates, led to authoritative conclusions about the past or future. By showing how proxy methods developed and gained authority, I argue that models were not the only way to envision the past, present, and future of the environment, as some historians have maintained.[10] Instead, paleoecologists found analogs for the future in the geologic past, and these similarities became powerful at forecasting the future. In these pronouncements, prophecy, proxies, and authority all intermingled, influencing what scientists could know about environmental change and whether they and their methods would be acceptable interpreters of that change. In this introduction, I briefly outline these three themes.

Paleoecology: The Science of Prophecy or the Science of Prediction?

Recently, historians have sought to understand how scientists have theorized about environmental futures.[11] Increasingly they have also noticed that the future of the environment has been tied to the geologic past.[12] As Paul Warde, Libby Robin, and Sverker Sörlin have argued, tying deep time together with the future has become an interest in discussions of the Anthropocene, especially as stratigraphers search for a golden spike to indicate that we have,

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[10] In the introduction to *A Vast Machine*, for example, Paul Edwards maintains that "everything we know about the world's climate – past, present, and future – we know through models. See, Paul N. Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (The MIT Press, 2010), xiv. On this issue of using models to understand possible future scenarios and the ways that discussions, representations and news about these model outcomes is projected to a certain future, see Mike Hulme, "Reducing the Future to Climate: A Story of Climate Determinism and Reductionism," *Osiris* 26 (2011): 245–66.

[11] For some examples, see Forrester and Smith, *Nature, Action and the Future: Political Thought and the Environment*; Daniel Sarewitz, Roger A. Pielke Jr, and Radford Byerly Jr, eds., *Prediction: Science, Decision Making, and the Future of Nature* (Washington, D.C.: Island Press, 2000); Robin, Sörlin, and Warde, *The Future of Nature: Documents of Global Change*; Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970."

[12] Dörries, "Politics, Geological Past, and the Future of the Earth"; Alessandro Antonello and Mark Carey, "Ice Cores and the Temporalities of the Global Environment," *Environmental Humanities* 9, no. 2 (November 1, 2017): 181–203. As part of this trend, the scientific journal *Earth's Future*, issued by the American Geophysical Union, began publication in 2013.

indeed, entered a new epoch where humans are a geologic force.[13] This search for “future fossils” has led geologists and historians to imagine the deep future of the Earth, rather than just its deep past.[14] But, as I argue throughout this dissertation, these links between the deep past and future have a long history. Deriving insights from the geologic past was a leading paleoecological project during various moments of environmental change: beginning during the Dust Bowl of the 1930s, paleoecologists began to call their discipline the “science of prophecy,” but extinction and climate change also spurred discussion of the predictive capacity of paleoecology.[15]

During three environmental crises – desertification, mass extinction, and climate change – the future became a key focus of paleoecological work, but the nature of paleoecological forecasts changed as paleoecological methods developed through the course of the twentieth century.[16] In some instances, paleoecologists argued that *prophecy* best described their methodological and epistemological commitments. In other instances, they argued that their use of the past to forecast the future constituted a *prediction*. I use the term *forecast* to describe any future-oriented pronouncement, prophecy and prediction alike.

Prophecy consisted of a broad forecast of future harm if humans did not change their actions.[17] For secular ecologists like Sears, prophecy was not the inspired declaration of divine

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[13] Paul Warde, Libby Robin, and Sverker Sörlin, “Stratigraphy for the Renaissance: Questions of Expertise for ‘the Environment’ and ‘the Anthropocene,’” *The Anthropocene Review* 4, no. 3 (2017): 249.

[14] Jennifer Newell, Libby Robin, and Kirsten Wehner, eds., *Curating the Future: Museums, Communities and Climate Change* (London: Routledge Environmental Humanities, 2017); Gregg Mitman, Marco Armiero, and Robert S. Emmett, eds., *Future Remains* (Chicago: The University of Chicago Press, 2018).

[15] Paul Bigelow Sears, *Deserts on the March*, 2nd ed. (London: Routledge & Kegan Paul, 1949), 177.

[16] For an overview of some of the different ways that scientists have used the past to project the future, particularly the ways that proxy data is used to understand climate change, see, J. A. Dearing et al., “Human–Environment Interactions: Learning from the Past,” *Regional Environmental Change* 6, no. 1–2 (March 2006): 1–16.

[17] Towards the end of a radio talk which explained many of Oklahoma’s problems, Sears spent some time looking “ahead into the Oklahoma of the future. In doing,” he said, “I shall depart from the usual role of a prophet in one respect. A prophet generally talks about what will happen if things go on as they are at present. If I did this for the soil and vegetation of Oklahoma, it would not be particularly cheerful reading. Being somewhat of an optimist, I should like to talk about what Oklahoma can be like if we really mean business.” See, “Looking into the Future,” *Norman Forum Series* (WNAD, May 14, 1937), Box 120, Folder 30, Coll. 663, Manuscripts and Archives, Yale

will and purpose.[18] Instead, prophecy was grounded in scientific observations, which, found human actions wanting in times of environmental crises. Prophecies did not outline the scientific evidence, but took the form of cautionary tales and had a story-telling quality: rather than quantified or technical forecasts and descriptions, ecologists described former civilizations and past environments that had been altered by human actions. Their work aimed to show the consequences when ecological principles went unheeded. By writing in this style, paleoecologists sought to provide accessible guides to living within the bounds of nature and a call for greater knowledge of ecological principles.[19]

Ecologists also aimed to warn of the dire consequences that would result if ecological prophecies were not followed by writing in this style. As Sears argued in *Deserts on the March,* written in 1935 in response to the Dust Bowl, desertification

> was not only predictable to anyone who knew the vegetation and climate of North America, but was predicted without causing anything but resentment. With the turf gone

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University. Reviews of Sears' work also recognized how he was encouraging action; as one review in *Science Newsletter* wrote, Sears "is no mere Jeremiah, prophesying only inescapable doom. Ways out can be found by serious application… [which will allow] human society… [to] achieve a stable culture pattern, comparable to the stability that slowly develops in organic communities in the subhuman world." See, "Ecological Method Applied to Problems of Human Life," *The Science News-Letter* 33, no. 1 (January 1, 1938): 9.

[18] Sears never talks about any religious affiliation and received all his education at public schools and secular colleges. His parents both attended secular colleges and his daughter does not mention religion in her remembrance of her father. A chapter in *Deserts on the March* entitled "Only God can make a Tree" is one of the few instances where Sears (1935, p. 105) talks about religion. While he uses those words as title, he suggests that the chapter's heading "has been vulgarized into sticky, repulsive sentiment, smearing out sight of the beautiful wisdom which it expresses." That wisdom, according to Sears, was that the growth of trees took time and nurture, but humans were interfering in their development. I do not take this statement to be a religious reflection about the role of God in nature. Instead, it is a reflection of natural processes that do not require a God: in commenting on the chapter heading, Sears distinguishes the scientist from the unnamed poet making the statement who could see God's hand in the growth of the tree. In contrast to the poet, the "slightly jaundiced eye of the scientist" saw in the tree "a marvelous, intricate laboratory whose workings are a perpetual, unsolved challenge." See, Sallie Harris Sears, "Paul B. Sears: Through a Daughter's Eyes," *Ohio Journal of Science* 109, no. 4–5 (December 2009): 119–27; Sears, *Deserts on the March*, 1935.

[19] On the related concept of prediction and policymaking, see Sarewitz, Pielke Jr, and Byerly Jr, *Prediction: Science, Decision Making, and the Future of Nature*.

> and the cycle of moisture past its peak, with the winds maintaining their normal behavior, the country literally started to blow out the ground.[20]

In making a statement like this, Sears was trying to carve out a larger role for ecologists in public affairs by promoting the role that they could play in planning. He argued that ecologists knew the vegetation and climate of North America from their studies of environmental change, especially long-term environmental change, so ecologists were in the best position to provide direction for the future and to manage those changes.

In many ways, paleoecologists' prophecies looked similar, in both style and message, to other scientists writing about the damage humans were inflicting on the natural world. Historians such as Finis Dunaway have discussed how American environmental reformers used their pens and cameras to critique human actions and to call for ecological harmony. These calls, in Dunaway's assessment, often drew on the Puritan legacy of the jeremiad sermon to condemn the abuse of the natural world or critique technological hubris.[21] Paleoecologists also used this legacy, lamenting how human actions were accelerating environmental degradation, and exploring the consequences of these actions at the same time as they enjoined people to follow ecological principles and re-envision the future. Similarly historians Paul Warde and Sverker Sörlin describe the impassioned narratives of "polymath prophets" like William Vogt, Lewis Mumford, Rachel Carson, and Paul Ehrlich, who invoked the vulnerabilities of an increasingly global environment as a call to action.[22]

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[20] Sears, *Deserts on the March*, 1935, 119–20.

[21] Finis Dunaway, *Natural Visions: The Power of Images in American Environmental Reform* (Chicago: The Univeristy of Chicago Press, 2005).

[22] Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970." Similar arguments have been made in Richard Kool, "Limits to Growth, Environmental Science and the Nature of Modern Prophecy," *Ecological Economics*, New Climate Economics, 85 (January 2013): 1–5; Matthew Schneider-Mayerson, "From Politics to Prophecy: Environmental Quiescence and the 'Peak-Oil' Movement," *Environmental Politics* 22, no. 5 (2013): 866–82. For an example of a historian writing using the "lessons from

Even though this prophetic mode remained an important trope in discussing human-caused harms to the environment, by the middle of the twentieth century, prediction had become more common in the environmental sciences. Prediction provided numerical forecasts about the likelihood of particular events. Using statistical similarities with the past as a guide, paleoecologists aimed to quantify the magnitude and direction of future changes. Their predictions would then serve as warnings of when ecological tipping points might be reached.

In first two decades of the twentieth century, ecologists like Frederic E. Clements began to suggest that predictions were possible because of the regular cyclical patterns he observed in nature. The Dust Bowl of the 1930s spurred him to claim a larger, predictive role for ecology, although he also admitted that predictions were as yet limited because scientists did not yet fully understand the interactions of various cycles and their effects.[23] Thus, as historian Naomi Oreskes has argued, prediction was not common in any scientific field, other than in astronomy and meteorology until after World War II. Before the second half of the twentieth century, scientists in these non-predictive fields questioned the value of prediction, concerned that there was little basis for predictions. Oreskes, for instance, describes how geologists expressed concern that prediction was beyond the realm of the observable and there were no repetitive patterns in the Earth's history or in human history from which to discern laws and make predictions. Instead, the geological record comprised a unique pattern of singular events and

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history," see Donald Worster, "Climate and History: Lessons from the Great Plains," in *Earth, Air, Fire, Water: Humanistic Studies of the Environment*, ed. Jill Ker Conway, Kenneth Keninston, and Leo Marx (Amherst, MA: University of Massachussetts Press, 1999), 51–77.

[23] Frederic E. Clements, "Climatic Cycles and Changes of Vegetation," in *Reports of the Conferences on Cycles,* Carnegie Institution of Washington (Washington, D.C.: Press of Gibson Brothers, Inc, 1929), 64–65.

geologists' goal was to explain the observable. Oreskes describes how the hypothetico-deductive method, which became popular in the middle of the twentieth century, encouraged predictions.[24]

New tools also aided this shift towards prediction. In 1934, British botanist and ecologist Harry Godwin had been forced to admit that paleoecological methods could only provide insights into the *direction* of the changing composition of the woodlands and the climate, rather than the *magnitude* of those changes.[25] With better statistical techniques and computational power by the 1950s, ecologists began to quantify the rates and magnitudes of change. The sheer amount of environmental data that scientists had accumulated about the state of the earth and its vegetable, animal, and human inhabitants since the 1920s also facilitated this shift to prediction during the Second World War. As Warde and Sörlin have argued, by the 1940s, scientists were integrating multiple strands of knowledge about the environment and transforming the data into a mathematical understanding of earth's dynamics and likely directions. This integration and quantification, they claim, led to the emergence of "the environment" as a concept, a term which "emerged historically as a crisis concept, closely wedded to prediction, and fears of the future."[26] With this kind of quantification and integration of data, science scholars Daniel Sarewitz, Roger A. Pielke Jr, and Radford Byerly Jr. have argued that the future of nature has "become the business of scientists."[27]

I use three environmental disasters – the Dust Bowl of the 1930s, discussion of mass extinction during the Cold War, and recent discussion of anthropogenic climate change – to show that paleoecology followed many of the trends towards prediction outlined in these brief

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[24] Oreskes, "Why Predict? Historical Perspectives on Prediction in Earth Science."

[25] H. Godwin, "Pollen Analysis: An Outline of the Problems and Potentialities of the Method. Part I: Techniques and Interpretation," *New Phytologist* 33, no. 4 (October 12, 1934): 292.

[26] Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970," 39.

[27] Sarewitz, Pielke Jr, and Byerly Jr, *Prediction: Science, Decision Making, and the Future of Nature*, 4.

overviews of prediction in the environmental sciences. Yet it is worth exploring paleoecologists' goals and methods in their own right because a close examination of these cases reveal just how much technique and scientific data affect environmental forecasts and our notions of environmental futures. I argue that paleoecologists' sense of whether they were prophesizing or predicting was based on their assessment of the uncertainties of proxy data and their methods for overcoming those uncertainties. Broadly speaking, as I show in chapter 2, in the 1930s, paleoecologists had only coarsely described the geologic past, so they could only find broad similarities between past instances of environmental disaster and current moments of degradation. Led by Sears, Dust Bowl forecasts thus claimed to be prophetic, although someone like Clements was also hinting at the possibility of prediction once he had better data. During the next environmental crises – the possibility of nuclear annihilation, which scientists related back to the problem of the mass extinctions at the end of the last Ice Age, and which I discuss in chapter 3 – paleoecologists began to move away from cautionary tales. By the 1970s, paleoecologists such as Paul S. Martin had improved statistical tools, more computational power, and a better understanding of proxy data, so he now claimed to be able to quantify the direction and magnitude of change. They called their engagement with the future "predictions," and they claimed that they would soon be able to make better predictions as their mathematical and computing powers improved.[28] Although predictive ecology was an important strand in ecology by the 1970s when anthropogenic climate change rose to the public consciousness, my final chapter shows how this environmental crisis lowered paleoecologists' confidence in their prediction powers. Paleoecologists worried that the future would be too dissimilar to the past to

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[28] The rise of mathematical theoretical ecology also reflects this trend. Through the 1970s, historian McIntosh describes how many ecologists claimed that quantitative theories combined with the recognition of widespread patterns in nature would revolutionize ecology into a predictive science. See, McIntosh, *The Background of Ecology: Concept and Theory*, 245.

justify their predictions, but they still saw a role for history in shaping the future: history, once again, would provide cautionary tales about anthropogenic climate change, rather than precise forecasts. They rarely called these tales prophecies, preferring the more neutral term "scenario," which provided a description of possible future situations based on the analysis and understanding of current and historic trends.[29] These shifts illustrate how, with each environmental disaster, paleoecologists reexamined the ways in which history could be used to forecast the future, and the limits of the proxy evidence that would support their forecasts.

These changes also illustrate the changing ways that scientists, the public, and policymakers came to expect scientific data about environmental futures to be presented. This is not just the story of increasing quantification and trust in numbers, but also a story of the role ecologists would play in interpreting changes in the past discerned both quantitatively and qualitatively, and the role that history could play in visions of the future.[30]

Proxies: The Basis of Prophecies and Prediction

This dissertation argues that ecologists relied on inferences from the geological past in order to make claims about the future during moments of environmental crisis, but how did ecologists access information about the last 12,000 years or longer? How did that data influence the kinds of forecasts that paleoecologists could make? This dissertation argues that paleoecologists were not only united by their interest in the future as they responded to

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[29] T. Webb III and T.M.L. Wigley, "What Past Climates Can Indicate About a Warmer World," in *Projecting the Climatic Effects of Increasing Carbon Dioxide*, ed. Michael C. MacCracken and Frederick M. Luther, DOE/ER-0237 (Washington, DC: United States Department of Energy, 1985), 243. Some of the scenario building language goes back to the Cold War, and the various scenarios nuclear policymakers devised to understand possible threats. See, Joseph Masco, "Bad Weather: On Planetary Crisis," *Social Studies of Science* 40, no. 1 (February 2010): 7–40.

[30] Some, for instance, critiqued this kind of reasoning for being too speculative: authors such as Rachel Carson, Paul Ehrlich, and Barry Commoner wrote "prophecies of calamity," which John Maddox's dismissed in his 1972 book for being too speculative. See, John Royden Maddox, *The Doomsday Syndrome: An Attack on Pessimism* (New York: McGraw-Hill Book Company, 1972). On Maddox, see Dörries, "Politics, Geological Past, and the Future of the Earth," 31.

environmental degradation, but they were also united by the use of what came to be known as "proxy" evidence. Proxies indirectly measure past environmental conditions. For example, a proxy such as fossilized pollen can be used to determine changes in past climates: by taking a sediment core from the bottom of a lake or bog, ecologists are often able to extract thousands of well-preserved pollen grains from the various layers of the core. By tracking the changes in the composition of the different pollen types found in these layers, ecologists could then make inferences about vegetation communities that resulted in the various pollen profiles.[31] From there, they could determine the past climate by comparing the climatic tolerances of similar modern vegetation profiles. Proxy data could also be used to predict the future: by finding analogs between the past and the present communities, scientists could make inferences about the likely direction of change, using past change as a model for future change. Overall, the techniques used to interpret these data mean that pollen and other proxies act as a "natural archive" that ecologists can use to make inferences about the past and future once they collect, count, organize, and interpret that evidence.

The use of proxy evidence in ecology and the environmental sciences is pervasive. Throughout the twentieth century, scientists have developed a number of different proxy techniques to study past and future environments. Techniques to analyze fossil pollen and correlate changes in the pollen record with climatic, vegetation, and anthropogenic changes

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[31] Paleoecologists use the term "pollen type" to refer to pollen identified with various degrees of specificity. Some pollen can be distinguished at the species level (such as the red maple, *Acer rubrum*), others at the level of the family (such as the grasses, *Poaceae*), others at the genus levels. Some can be identified as a subgenus grouping (such as *Pinus* subgenus *stobus*, or white pine). Sears provided one of the first guides to fossil pollen in North America, sampling modern pollen so the pollen workers would have a reference guide for the pollen found in sediment. Improved microscopes and reference materials have helped with identification. See, Paul B. Sears, "Common Fossil Pollen of the Erie Basin," *Botanical Gazette* 89, no. 1 (March 1930): 95–106; Boris Zimmermann and Achim Kohler, "Infrared Spectroscopy of Pollen Identifies Plant Species and Genus as Well as Environmental Conditions," *PLoS ONE* 9, no. 4 (April 18, 2014): 1–12; Andrew C. Martin and William J. Harvey, "The Global Pollen Project: A New Tool for Pollen Identification and the Dissemination of Physical Reference Collections," *Methods in Ecology and Evolution* 8, no. 7 (July 2017): 892–97.

emerged in Scandinavia in the 1910s before spreading around the world in the 1920s.[32] Tree-ring analysis and dendrochronology developed around the same time, largely through of the work of astronomer Andrew Ellicott Douglass at the University of Arizona.[33] Paleoecological studies of diatoms, an abundant class of unicellular algae, looked more similar to pollen analysis, since they involved taking a sediment core in an aqueous environment and tracking changes in the various diatom communities. A number of pollen analysts, notably Edward S. Deevey at Yale who was influenced by G. Evelyn Hutchinson's work on limnology and Richard Foster Flint's work on geology, began to work with diatoms in the 1930s, developing the new field of paleolimnology from these data in the 1940s and 1950s.[34] In the 1950s, those initially trained in pollen analysis, such as Paul S. Martin at the University of Arizona, looked to other proxies to understand extinction. These other proxies included the organic matter found in fossil sloth dung and packrat middens, the nests built by wood rats. Packrat middens gave insights into species-level vegetation change, rather than change at the level of the family or genera, which pollen analysis typically provided.[35] By the 1980s, scientists added inorganic evidence, such as bubbles

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[32] On the history of pollen analysis, see A.A. Manten, "Lennart Von Post and the Foundation of Modern Palynology," *Review of Palaeobotany and Palynology* 1, no. 1–4 (March 1967): 11–22; Gunnar Erdtman, "Glimpses of Palynology 1916–1966," *Review of Palaeobotany and Palynology* 1, no. 1 (1967): 23–29; A. A. Manten, "Half a Century of Modern Palynology," *Earth-Science Reviews* 2 (1966): 277–316; Linda C. K. Shane, "Paul B. Sears' Contributions to the Development of Paleoecology," *The Ohio Journal of Science* 109, no. 4–5 (December 2009): 76–87; Christer Nordlund, "Peat Bogs as Geological Archives: Lennart Von Post et Al., and the Development of Quantitative Pollen Analysis during World War I," *Earth Sciences History* 33, no. 2 (July 2014): 187–200; Anna F. Edlund and Zachary A. Winthrop, "Sharing What He Saw: An Appreciation of Gunnar Erdtman's Life and Illustrations," *Grana* 53, no. 1 (2014): 1–21; H. John B. Birks and Björn Berglund, "One Hundred Years of Quaternary Pollen Analysis 1916-2016," *Vegetation History and Archaeobotany* 27 (2018): 271–309.

[33] Bernd Becker, "The History of Dendrochronology and Radiocarbon Calibration," in *Radiocarbon After Four Decades*, ed. R.E. Taylor et al. (New York: Springer, 1992), 34–49; George E. Webb, "Solar Physics and the Origins of Dendrochronology," *Isis* 77, no. 2 (June 1986): 291–301.

[34] Eugene F. Stoermer and John P. Smol, eds., *The Diatoms: Applications for the Environmental and Earth Sciences* (New York: Cambridge University Press, 1999).

[35] Paul S. Martin, Bruno E. Sabels, and Dick Shutler, "Rampart Cave Coprolite and Ecology of the Shasta Ground Sloth," *American Journal of Science* 259, no. 2 (February 1961): 102–7; Philip V. Wells and Clive D. Jorgensen, "Pleistocene Wood Rat Middens and Climatic Change in the Mohave Desert: A Record of Juniper Woodlands," *Science* 143, no. 3611 (1964): 1171–74.

of greenhouse gases found in ice cores, to their list of proxies.[36] With each proxy, or sometimes by drawing on multiple different proxies, paleoscientists were able to make inferences about past vegetation and climate. By the 1980s, collections of these data were regularly referred to as “proxy records” and proxy evidence has now become one of the main ways to study environments of the past.[37]

This dissertation is the first focused account that tracks the history of proxy data, something surprisingly missing from histories of science considering how important knowledge from proxies is to our understanding of environmental change. If we have left the stable Holocene environment – as the notion of the new geological epoch, the Anthropocene suggests – we will need some way to understand where we are headed, and many have suggested that the best predictor for the future will be the past.[38] The instrumental and historical records are not deep enough to capture these changes, since they only encompass the Holocene-period of incredible stability in which all human cultures have developed. Where these records end, proxy evidence becomes our main data source, both for predicting the future in its own right and for validating models. And, because paleoecologists study that record, they have been increasingly cited as an important contributor to understanding environmental futures.[39]

By tracing a century of work on biological proxies, I show that the use of proxy data was controversial. Many scientists, and paleoecologists themselves, wondered whether thousands of

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[36] Richard Alley, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change, and Our Future* (Princeton, NJ: Princeton University Press, 2000); Chester C. Langway Jr., *The History of Early Polar Ice Cores* (Hanover, NH: US Army Corps of Enginners: Engineer Research and Development Center, 2008); Janet Martin-Nielsen, “The Other Cold War: The United States and Greenland’s Ice Sheet Environment, 1948–1966,” *Journal of Historical Geography* 38, no. 1 (January 2012): 69–80; Janet Martin-Nielsen, “The Deepest and Most Rewarding Hole Ever Drilled: Ice Cores and the Cold War in Greenland,” *Annals of Science* 70, no. 1 (2013): 47–70.

[37] See, for example, Stephen Henry Schneider, *Global Warming: Are We Entering the Greenhouse Century?* (San Francisco: Sierra Club Books, 1989).

[38] Dearing et al., “Human–Environment Interactions.”

[39] Brian Huntley, “Quaternary Palaeoecology and Ecology,” *Quaternary Science Reviews* 15, no. 5–6 (1996): 591–606.

microfossils could reliably say anything about the geologic past. Critics argued that paleoecologists' data was opaque, and their inferences too subjective to be of much use understanding the future. So how did proxy data become one of the main sources of data in the earth and environmental sciences?

I show that proxy data were one of the few ways to understand the past environments, which became an increasingly important point of comparison with likely futures as scientists identified large-scale anthropogenic change. Even though some of the same problems that had caused critics to dismiss proxy evidence persisted once proxy data was elevated to a necessary guide to the past and future, scientists became increasingly willing to overlook these shortcomings. Minimizing these problems, or finding ways to overcome them, became especially important as scientists sought an independent way to verify the models, which became a competing way to predict the future by the 1980s.[40]

The development of proxy data and methods matters to the kinds of predictions about the future scientists are able to make. This history also influences the kind of data that is now available to verify our models. Paleoecologists had worked for decades, in particular intellectual and technological milieus, to make these biological proxies say something about the past so that they could, in turn, make predictions about the future, either by analogy or through modelling. Our sense of environmental futures is strongly influenced by this history.[41]

Proxies are also worth examining because scientists working on issues of deep time often strongly identify with their proxies. We cannot understand the development of scientific

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[40] Edwards, *A Vast Machine*.

[41] Paleoecologist Linda Shane makes a similar argument, showing how technology and prevailing thinking at the time of investigation influenced the conclusions that paleoecologists made at different moments of time. Shane looks at how three different investigators drew conclusions about the climatic history of the same bog when they sampled in 1930, 1958 and 1986. See, Linda C.K. Shane, "Changing Palynological Methods and Their Role in Three Successive Interpretations of the Late-glacial Environments at Bucyrus Bog, Ohio, USA," *Boreas* 18, no. 4 (December 1989): 297–309.

disciplines that focus on environmental change without understanding their methods. Scientists in these fields regularly describe themselves by their technique, rather than identifying with the larger fields such as ecology, geology, botany, or climatology. Those who worked with fossil pollen, for instance, saw themselves as pollen analysts, who were using pollen statistics to study past vegetation and climate. They were certainly interested in ecological and geological questions, but their bibliographic lists, textbooks, circulars, and conferences were united by their methods rather than the questions about the past and future that these techniques would solve.[42] In fact, the technique was so important that, by the 1940s, pollen workers had a new name for their field: palynology.[43] Even today, proxy techniques connect many of the paleosciences, as is evident by the University of Maine's paleoecology blog "Ecology by Proxy," regular discussions of technique in paleoecological papers, and the continuation of technical journals like *Pollen et Spore*, and *Palynology*.[44]

Proxy techniques, I argue, were central to the identities of paleoscientists because these scientists spent much of their careers discussing how to obtain and interpret proxy data. As I explore in chapter 1, after Swedish peat geologist Lennart von Post presented pollen analysis as a method to elucidate the history of vegetation and climate, skeptics immediately demanded to know whether the proxy data corresponded to the past in the ways that von Post claimed.[45] After all, the skeptics argued, the method required a deep understanding of how different pollen were produced, dispersed, deposited, and preserved, and also required specifying how the pollen record corresponded to vegetation, climate, people, and other environmental factors. Each genera

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[42] For an early examples, see Gunnar Erdtman, "Literature on Pollen-Statistics Published before 1927," *Geologiska Föreningen i Stockholm Förhandlingar* 49, no. 2 (April 1927): 196–211.
[43] William A. S. Sarjeant, "'As Chimney-Sweepers, Come to Dust': A History of Palynology to 1970," *Geological Society, London, Special Publications* 192, no. 1 (2002): 273–327.
[44] Jacquelyn Gill, "Ecology by Proxy," accessed May 9, 2018, https://paleoecology.wordpress.com/.
[45] Lennart von Post, "Forest Tree Pollen in South Swedish Peat Bog Deposits," trans. Margaret Bryan Davis and Knut Faegri, *Pollen et Spores* 9 (1967 1916): 378–401.

of pollen differed from others in terms of production, dispersion, deposition, and preservation, so critics asked whether the graphs of changing compositions of pollen genera could be used to say anything about the past.

These problems have endured: as I show in chapter 4, in 1994, nearly a century after von Post developed pollen analysis, paleoecologist Margaret Bryan Davis argued that the field has "remained in almost the same state since von Post introduced the method in 1916." She went on to say that the "complexity of the data and the subjectivity of interpretation have made the literature opaque" for non-specialists, since practitioners were using a variety of different methods to obtain and interpret their data.[46] Despite this sense that paleoecology continued to experience problems interpreting proxy data, many paleoecologists had sought new ways to understand their data: their publications often focused on improving their methods, which united paleoecologists as they sought to justify their techniques and defend their conclusions.

Despite this sustained focus on improving method, methodological questions in paleoecology remained. A 2014 study asked 127 individuals, laboratories, and organizations in 26 countries and five continents to identify their research priorities. Forty percent of the research questions that respondents identified as important "related to methodology, either directly by focusing upon improved precision and accuracy or by finding new ways to apply and interpret paleoecological data to address broader questions of, for example, landscape management."[47] The authors thought that continued questions about methodology were unsurprising because indirect measurements that relied on proxy data and inferences were at the heart of paleoecology.

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[46] Margaret Bryan Davis, "Ecology and Paleoecology Begin to Merge," *Trends in Ecology & Evolution* 9, no. 10 (October 1994): 357–58. On the history of ecological complexity, see Laura J. Martin, "Mathematizing Nature's Messiness: Graphical Representations of Variation in Ecology, 1930-Present," *Environmental Humanities* 7, no. 1 (2015): 59–88; Joel Bartholemew Hagen, *An Entangled Bank: The Origins of Ecosystem Ecology* (Rutgers University Press, 1992); McIntosh, *The Background of Ecology: Concept and Theory*.

[47] Alistair W.R. Seddon et al., "Looking Forward through the Past: Identification of 50 Priority Research Questions in Palaeoecology," *Journal of Ecology* 102 (2014): 265.

They thought that indirect measurements, in particular, required continuous questions about what proxy data can and cannot reveal, as well as sustained interest in understanding uncertainties, in order to ensure that paleoecology came to rigorous conclusions.

The history of paleoecology reveals unrelenting discussions about methods, which is one of the reasons discussions of proxy techniques are at the heart of this dissertation. In many ways, working with proxies was similar to other fields that deal with the past: in all cases, the past is not directly accessible, so scientists use traces found in the geological record, such as bones, pottery shards, and microfossils to stand in for direct observations. This record has long been questioned for its incompleteness, as historians have ably described how geologists, paleontologists and archeologists have struggled with the gaps and missing links that exist in the macrofossil record and archeological record.[48] A central difference between the microfossil record and the macrofossil record is that uncertainties arise not from scarcity, but from abundance.[49] In the aqueous environments where von Post first began to trace changes within pollen profiles over time, he found thousands of grains within each layer of sediment. As paleolimnologist Daniel Livingstone reflected over a half-century later, "ordinary organic lake mud commonly contains some hundreds of thousands of these microfossils [pollen grains and spores] per milliliter."[50] Even in arid environments, which were initially thought to be a difficult environment for preservation, pollen analysts often had hundreds of grains to work with. The widespread preservation of proxy evidence, often due to pollen's tough outer walls, combined

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[48] Martin J. S. Rudwick, *The Meaning of Fossils: Episodes in the History of Palaeontology* (Chicago: University of Chicago Press, 1976); Martin J.S. Rudwick, *Earth's Deep History: How It Was Discovered and Why It Matters* (Chicago: University Of Chicago Press, 2014); David Sepkoski, *Rereading the Fossil Record: The Growth of Paleobiology as an Evolutionary Discipline* (Chicago: University of Chicago Press, 2012).

[49] On some of the issues of dealing with this kind of data, see Lorraine Daston, ed., *Science in the Archives: Pasts, Presents, Futures* (Chicago: The University of Chicago Press, 2017).

[50] D. A. Livingstone, "Speculations on the Climatic History of Mankind," *American Scientist* 59, no. 3 (June 1971): 332.

with the abundant production of these particles in many environments, meant that paleoscientists had a lot of data to organize and interpret. Depending on how paleoscientists organized and interpreted these data, they could be seen as valuable contributors of environmental knowledge, or they could be critiqued for unjustified conclusions.

Paleoecologists as Experts

Given this interest in the credibility of paleoecological data, my dissertation also examines the authority-building practices of paleoecologists. Because they were temporally separated from their objects of inquiry, paleoecologists did not have the same credibility-building strategies at their disposal as other fields. Historians have shown how the field sciences have cultivated authority in situ, establishing credibility through practices of place, local knowledge, situated knowledge, bodily knowledge, and knowledge of nature through labor or play.[51] Paleoecology is also a field science, as workers take sediment cores, collect fossilized dung, and carefully record the characteristics of their sampling sites. But, paleoecologists are separated in time from the environmental dynamics that led to the creation and deposition of proxy data. This means that some of the authority-building practices gained through intimate connections with their places of study are not available to paleoecologists.

Paleoecologists also could not point to objective techniques to establish their authority. Paleoecological data was, and largely remains, open to subjective interpretations.[52] The first text-book on pollen analysis, published in 1950, suggested that it could only offer pollen workers

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[51] Robert E. Kohler, *Landscapes and Labscapes: Exploring the Lab-Field Border in Biology* (Chicago: University of Chicago Press, 2002); Richard White, *The Organic Machine: The Remaking of the Columbia River* (New York: Hill and Wang, 1995); Michelle Murphy, *Sick Building Syndrome and the Problem of Uncertainty: Environmental Politics, Technoscience, and Women Workers* (Durham, NC: Duke University Press, 2006); Linda Nash, *Inescapable Ecologies* (Berkeley, CA: University of California Press, 2007); Joy Parr, *Sensing Changes: Technologies, Environments, and the Everyday, 1953-2003* (Vancouver: UBC Press, 2009).

[52] On the history of scientific objectivity, see Lorraine Daston and Peter Galison, *Objectivity* (New York: Zone Books, 2007).

"general directives" about how to interpret the data in the sediments they were working with. Universal techniques were not possible because there were too many vagaries between local environments and their geological history. The first textbook thus went on to say that each pollen analyst was forced to "solve each problem independently by botanical judgment (and common sense!)" as well as "an intimate knowledge of the vegetation types."[53] The textbook was establishing a common strategy that paleoecologists used to gain credibility: it suggested that paleoecologists offered trained judgments, gained from intimate knowledge of present environmental processes (much like other field scientists), but that paleoecologists also knew how to extend that knowledge to the past.

Yet, because of the uncertainties associated with proxy data discussed in the previous section and more fully in chapter 1, paleoecologists used various strategies to minimize questions about whether their interpretations were warranted. They carefully documented geological processes and their potential to influence their data. They performed various experience to understand pollen production and dispersal. And they went to the field to understand the makeup of vegetation communities, all with the goal of properly understanding their proxies.

By focusing on the ongoing critiques of paleoecological methods, I am also able to show how the expectations of what constituted scientific evidence and appropriate ways of knowing were changing through the course of the twentieth century. For example, in my third chapter, I show how paleoecologists' understanding of the causes of the Pleistocene extinctions, the mass extinctions of the North America's megafauna at the end of the last Ice Age, led to debates about Native Americans' ability to manage lands: Martin was blaming Native Americans' ancestors for hunting the megafauna to extinction. He claimed that the proxy record indicated that climate

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[53] Knut Faegri and Johannes Iversen, *Text-Book of Modern Pollen Analysis* (Copenhagen: Ejnar Munksgaard, 1950), 94–95.

change was not synchronous with the extinctions, but that the arrival of humans coincided with these extinctions. Anthropologists like Shepard Krech III used this conclusion to question the picture of the "ecological Indian," the perspective that had afforded Native Americans special status for their respect of nature and careful management of the land.[54] Krech's conclusions led Canadian journalist and former politician, Douglas Fisher, to say that "Canadian Indians should not be accorded the superior sanction of high-minded environmentalism in negotiations of land settlements… Indians are neither more noble nor more ignoble than other people."[55]

Statements like these had encouraged a number of Native American thinkers, notably Vine Deloria Jr. to get involved in this debate. Deloria critiqued paleoecologists' methods, finding scant evidence for over-hunting in the macrofossil record. He also pointed to another body of literature, Native American oral traditions, as a superior source of evidence.[56] This body of knowledge, he claimed, did not support paleoecologists' conclusion that human hunters were responsible for the extinctions. Instead, Deloria argued that climatic causes were responsible, but because scientists had maintained a "stranglehold" on what respectable and reliable evidence was, even when their scientific theses had little supporting evidence, they could cast aside Native American explanations as "superstition."[57]

Using Deloria's critique as my starting point, I show how paleoecological evidence could be questioned and how paleoecologists' expertise could be undermined. Native Americans were raising important objections about the kinds of inferences that paleoecologists were drawing

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[54] Shepard Krech III, *The Ecological Indian: Myth and History* (New York: W. W. Norton & Company, 1999).

[55] Douglas Fisher, "The Myth of the Ecological Indian," *The Toronto Sun*, January 23, 2000, Final edition, sec. Comment.

[56] This body of evidence, known as traditional ecological knowledge, has been widely examined in anthropology. See, for example, Berkes Fikret, Colding Johan, and Folke Carl, "Rediscovery of Traditional Ecological Knowledge as Adaptive Management," *Ecological Applications* 10, no. 5 (October 2000): 1251–62; Berkes Fikret, *Sacred Ecology: Traditional Ecological Knowledge and Resource Management.* (Philadelphia: Taylor & Francis, 1999).

[57] Vine Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact* (New York: Scribner, 1995), 19.

from the paleo record and their proxies, especially because Native Americans were not seen as experts when they made inferences from their oral traditions. Anthropologists and paleontologists recognized these problems as well, and chastised their colleagues for their inferential styles of reasoning and untestable conclusions. By focusing on these critiques, I am able to show how the expectations of what constituted scientific evidence and appropriate ways of knowing were changing through the course of the twentieth century: where once subjective inferences had been an acceptable mode of reasoning, by the middle of the twentieth century, scholars and public intellectuals expected quantifiable and testable conclusions. Although paleoecology was beginning to use these practices, conclusions like Martin's suffered because he continued to make claims that were nothing more than "bedtime stories" in the eyes of critics.

Yet to suggest that the hypothetico-deductive method and quantified results became the expected vision of scientific knowledge by the middle of the twentieth century ignores the fact that the cautionary tales of paleoecologists were accepted in the twentieth-first century. Not every prophecy, especially once he had the guise of a "scenario," was dismissed as a bedtime story. As I show in chapter 5, with unprecedented climate change, paleoecologists recognized that they had little data from the past that mimicked current conditions or was likely to correspond to future ones. Without the ability to draw analogies, paleoecologists lost their ability to make quantified claims or test hypotheses, but still claimed to be able to offer "cautionary tales" through scenario building. They argued that these tales were some of the few that provided the integrative perspective that best captured large-scale environmental change. Because of their integrative perspective, they argued that this modified form of prophecy was better than prediction.

Chapter Outline

Ideas of prophecy, proxies and authority come together in my five chapters. My first chapter traces the emergence of ecological studies through geological time in the late-nineteenth and early twentieth centuries. I show that paleoecology was connected to geology, botany, and glaciology, and sought to determine how the environment, especially the climate, influenced the rise and fall of civilizations. Although the founder of pollen statistics, Lennart von Post, had presented his technique in 1916 as a way to answer questions about the drivers of environmental and cultural change, definitive conclusions largely proved elusive. I show that there were too many uncertainties about the ways that pollen data tracked past environments to make inroads on these questions of environmental change. These difficulties meant that much of the focus in paleoecology centered on understanding proxy data.

My second chapter explores the prophetic nature of paleoecology. It shows how, during the environmental disaster of the 1930s, paleoecologists put aside their questions about method to offer their insights on desertification. I show that, through comparisons between past and present, paleoecologists like Frederic Clements and Paul Sears found an accelerated version of human-caused vegetation change, one that threatened environmental and social stability of the Great Plains. This finding led paleoecologists to focus on the land-use practices of past civilizations as a leading reason for the downfall of many former cultures. They also discussed broader questions of whether history repeated itself, was progressive, or was a series of random events. Discussions about the direction of history underwrote many paleoecologists' forecasts: Clements, who believed that history was cyclical, thought that he could make good predictions about when climatic conditions would next exacerbated human disturbances and how such effects could be minimized. Sears, who believed that history was less regular, offered cautionary tales from the past, prophecies rather than predictions.

Chapter 3 looks at the mass extinction of North America's megafauna at the end of the last Ice Age. In order to better understand the reasoning patterns of proxy evidence, it compares conclusions generated from paleoecological data with evidence generated by Native American oral histories. Doing so allows me to show how new techniques, such as radiocarbon dating, and new forms of proxy evidence, allowed paleoecologists to fill in the geological record. Yet, even as paleoecologists described the debate about the Pleistocene extinctions as shaped by inconclusive evidence, more evidence did not cause others to accept their conclusions. Instead, other scientists and Native American activists objected to the way that paleoecology was drawing conclusions about the future from the past. Martin had argued that the ancestors of Native Americans had likely hunted the megafauna to extinction soon after crossing the Bering Strait. Some writers used this conclusion to suggest that Native Americans could not be trusted to manage their lands – a claim about the future land rights of indigenous peoples. This conclusion spurred Deloria to critique paleoecologists – and science more generally – for their reasoning structures. He argued that scientists were making claims that were unfalsifiable, since it was the paucity of evidence that supported their claims. Native Americans, on the other hand, claimed that their oral traditions could find support in the geologic record, so why were paleoecologists' claims, and their recommendations for the future, given priority?

The fourth chapter focuses on paleoecologists' epistemological and ontological renderings of climate, and puts these in contrast with earth and atmospheric scientists. Paleoecologists primarily used biological proxies whereas earth and atmospheric scientists chiefly employed physical-chemical proxies. I show how these proxies made certain phenomena visible, but blinded researchers to other aspects of climate change. By examining the stuff of these different approaches – atmospheric gases and computer models, lake sediments and microfossils – my work explains how scientific objects have shaped the contours of climate science.[58]

My final chapter also examines climate change, focusing on concerns about anthropogenic global warming and the ways that the so-called "unprecedented" nature of this event shaped paleoecological forecasts. By the 1980s, paleoecologists began to talk about the "no-analog situation" that resulted from anthropogenic warming. That is, they noted that the changes expected due to climate change were of a greater magnitude than any climatic changes visible in the geologic record. Without a point of comparison in the past, they worried about the predictive power of paleoecology: they would not be able to make predictions because they had no comparison case in the past. Rather than arguing that the field could not provide direction for climate change, paleoecologists began to argue that "history is better suited to providing cautionary tales rather than specific images of future climate and vegetation change."[59] In this quotation, we see a sense of the prophetic predictions of old, rather than specific quantified

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[58] Historians of science and STS scholars have devoted considerable attention to the ways in which scientific objects explain broader ideas and processes in science. See, for example, Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump : Hobbes, Boyle, and the Experimental Life* (Princeton, NJ: Princeton University Press, 1985); Peter Galison, *Image and Logic* (Chicago: The University of Chicago Press, 1997); Bruno Latour, "Where Are the Missing Masses? The Sociology of a Few Mundane Artifacts," in *Shaping Technology-Building Society: Studies in Sociotechnical Change*, ed. Wiebe Bijker and John Law (Cambridge, MA: MIT Press, 1992), 225–59; Michel Callon, "Some Elements of a Sociology of Translation: Domestication of the Scallops and Fishermen of St Brieuc Bay," in *Power, Action and Belief: A New Sociology of Knowledge?*, ed. John Law (London: Routledge & Kegan Paul, 1986), 196–233; Susan Leigh Star and James R. Griesemer, "Institutional Ecology, 'Translations' and Boundary Objects: Amateurs and Professionals in Berkeley's Museum of Vertebrate Zoology, 1907-39," *Social Studies of Science* 19 (1989): 387–420; Lorraine Daston, ed., *Biographies of Scientific Objects* (Chicago: The Univeristy of Chicago Press, 1999). On the ways scientific objects are being used to understand environmental change, see also, Stefan Helmreich, *Alien Ocean: Anthropological Voyahes in Microbial Seas* (Berkeley, CA: University of California Press, 2009); Antonello and Carey, "Ice Cores and the Temporalities of the Global Environment."

[59] Stephen T. Jackson and Jonathan T. Overpeck, "Responses of Plant Populations and Communities to Environmental Changes of the Late Quaternary," *Paleobiology* 26 (2000): 213.

forecasts that came to be the norm in many fields. In this way, I show that proxy data continued to shape how paleoecologists envisioned the future.

Overall, these chapters trace the future-oriented vision of ecology during moments of environmental change. My dissertation also encourages historians to confront the unique features of science by proxy, and the ways that proxy data has shaped environmental knowledge. It also attempts to answer why certain disciplines have had authority to determine the future of the environment.

Chapter 1 – The Scale of Change: Defining the Spatial Boundaries of Proxy Evidence

In 1916, Swedish peat geologist Lennart von Post presented a lecture to the 16th convention of Scandinavian naturalists.[60] Those that rely on pollen analysis to understand past environments widely cite this lecture as the beginning of their field: in his presentation, von Post described a method known as "pollen statistics," which made inferences about past climates and vegetation from fossil pollen found in sediment.[61] The method, as described in 1934 by American popularizer of pollen statistics, Paul Sears, involved identifying the distinctive kinds of pollen present in various layers of sediment and determining the proportions of each kind. "From the kinds which are present at different levels in the deposit, and their changing proportions, much can be learned about changes in the adjacent plant life," Sears wrote, adding that information about vegetation allowed pollen workers to make further inferences about climatic conditions.[62] When spruce and fir pollen dominated samples from the Midwest, for example, Sears inferred that the climate was cool and moist, but when northern pines were most abundant, Sears inferred that the climate was cool and dry. When pollen workers used pollen as a basis for these inferences, they came to say that pollen acted as a "proxy" for past climates and vegetation.[63]

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[60] von Post, "Forest Tree Pollen in South Swedish Peat Bog Deposits."

[61] Nordlund, "Peat Bogs as Geological Archives: Lennart Von Post et Al., and the Development of Quantitative Pollen Analysis during World War I"; Manten, "Lennart Von Post and the Foundation of Modern Palynology"; Manten, "Half a Century of Modern Palynology"; Margaret B. Davis, "On the Theory of Pollen Analysis," *American Journal of Science* 261, no. 10 (December 1963): 901.

[62] Paul Bigelow Sears, "Climate in Northern Hemisphere since Ice Ages: Studies of Fossil Pollen, Accumulations in Old Lake Beds, and Other Clues Reveal How Climate Has Shifted and Its Effect on Man.," *Literary Digest* 117, no. 1 (1934): 18.

[63] The term "proxy" for this kind of data and these kinds of interferences only emerged in the late 1980s, although proxy techniques went back to the beginning of the twentieth century. See, Schneider, *Global Warming,* 42. Occasionally, scientists also referred to tracers for what then became known as proxy evidence. For example, in 1963 G.H. Scott referred to paleoecology as the "study of past environments using organisms as tracers." See, G.H. Scott, "Uniformitarianism, the Uniformity of Nature, and Paleoecology," *New Zealand Journal of Geology and Geophysics* 6, no. 4 (1963): 517.

While proponents of pollen statistics like Sears were eager to use von Post's method to understand the drivers of vegetation change and the rise and fall of civilizations, many, Sears included, were aware that there were "many difficulties and much uncertainty" in using pollen to make inferences about the past.[64] One of the chief concerns was about the scale represented by pollen data. Since some pollen might travel long distances before deposition and other pollen might be local, pollen workers sought to determine if they were describing local or regional environments with their data, or some mixture of both. Demarcating the scale that proxy evidence tracked and the boundaries of ecological inquiry were important considerations as pollen workers, like many scientists in the late-nineteenth and early twentieth centuries, sought to establish the drivers of environmental change and their impacts on past and present human civilizations.

These questions about boundaries and the drivers of environmental change were also common in paleoecology's parental disciplines: geology, botany and ecology. Scientists in these fields noted that different drivers emerged on different scales, meaning that these scientists were grappling with questions about the boundaries of ecological and climatic communities. Some, such as Frederic Clements, thought that climate drove environmental change on large scales while others, including Henry Chandler Cowles, preferred local scales and emphasized factors like soil and light as exerting the most influence on organic communities. To mediate between these different points of view, some scientists turned to pollen analysis, thinking that a long-term perspective would allow them to demarcate boundaries and better understand the factors influencing environmental change. But, this chapter argues, questions about the process of turning pollen data into information about past communities came to dominate the field. The

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[64] Sears, "Climate in Northern Hemisphere since Ice Ages: Studies of Fossil Pollen, Accumulations in Old Lake Beds, and Other Clues Reveal How Climate Has Shifted and Its Effect on Man.," 18.

fixation on methodological problems united those working with proxies, but also helped marginalize the sub-discipline from mainstream ecology out of a fear that proxies were so difficult to interpret that they might not adequately represent past environments and the changes they experienced. These methodological problems largely remain for proxy sciences, but, as I argue in subsequent chapters, critics often overlooked these problems during moments of environmental degradation, thinking that the past offered some perspective on the problem and the likely direction the crisis would take. In these moments, proxy evidence served to prophesize and predict the future.

The Origins of Ecological Studies through Geologic Time

The first generation of geologists, botanists, and ecologists eager to use a method like pollen statistics belonged to a research tradition known as postglacial geology. This tradition centered on the "environmental influence idea," which sought to understand how the "distribution, character and activities of life" influenced "earth features and resources."[65] Ice Age theories, which had developed in the late nineteenth century, loomed large in this idea, as scientists considered the ways in which natural and cultural landscapes evolved after the ice sheets retreated about 12,000 years ago.[66] To explain the development of soil, lakes, and rivers, as well as the migration of flora, fauna, and humans, university chairs in geology or botany, as well as those employed in natural history museums, schools of agriculture and forestry, and geological surveys did inter-related geological, bio-geographical and archeological fieldwork. Many were especially eager to explain present-day landforms and vegetation assemblages on the

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[65] William D. Pattison, "Rollin Salisbury and the Establishment of Geography at the University of Chicago," in *The Origins of Academic Geography in the United States*, ed. Brian W. Blouet (Hamden, CT: Archon Books, 1981), 154.

[66] Tobias Krüger, *Discovering the Ice Ages: International Reception and Consequences for a Historical Understanding of Climate*, trans. Ann M. Hentschel (Leiden: Brill, 2013), 458–60.

basis of past geologic processes.[67] Others were keen to explain the supposed national characteristics of different countries or regions on the basis of climatic and environmental factors.[68] Relatedly, these scientists sought to flesh out a widely held belief of environmental determinism which posited that, before human cultures progressed to the point where they became independent of the natural conditions of life, the climate set its "trace on mankind's destiny."[69] These scientists thought that climate likely "had sweeping results on both the distribution and standards of living of mankind" and they wanted to better understand this relationship.[70]

The founder of pollen statistics, Lennart von Post, was part of this tradition, which was particularly strong in Scandinavia.[71] The Swedish geologist was surveying Sweden's peat

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[67] Mitman, *The State of Nature*, chap. 1; Rudwick, *Earth's Deep History: How It Was Discovered and Why It Matters*, 181–85.

[68] These ideologies eventually gave rise to Ellsworth Huntington's environmental determinism. On Huntington, see James Rodger Fleming, "The Climatic Determinism of Ellsworth Huntington," in *Historical Perspectives on Climate Change* (New York: Oxford University Press, 1998), 95–106; Kent M McGregor, "Huntington and Lovelock: Climatic Determinism in the 20th Century," *Physical Geography* 25, no. 3 (2004): 237–50.. On early views of ethno-climatological influence, see Daniel N. Livingstone, "Climate's Moral Economy: Science, Race, and Place in Post-Darwinian British and American Geography," in *Geography and Empire*, ed. Neil Smith and Anne Godlewska (Oxford: Oxford University Press, 1995), 132–54; Vladimir Jankovic, "Intimate Climates: From Skins to Street, Soirees to Societies," in *Intimate Universality: Local and Global Themes in the History of Weather and the Climate*, ed. James Rodger Fleming, Vladimir Jankovic, and Coen (Sagamore Beach, MA: Watson Publishing International, 2006), 1–34; Sverker Sörlin, "The Global Warming That Did Not Happen: Historicizing Glaciology and Climate Change," in *Nature's End: History and the Environment*, ed. Sverker Sörlin and Paul Warde (London: Palgrave Macmillan, 2009), 93–114; Nordlund, "Peat Bogs as Geological Archives: Lennart Von Post et Al., and the Development of Quantitative Pollen Analysis during World War I."

[69] Lennart Von Post, "The Prospect for Pollen Analysis in the Study of the Earth's Climatic History," *The New Phytologist* 45, no. 2 (1946): 217.

[70] Von Post, 217. See also, Sears, "Climate in Northern Hemisphere since Ice Ages: Studies of Fossil Pollen, Accumulations in Old Lake Beds, and Other Clues Reveal How Climate Has Shifted and Its Effect on Man."

[71] Von Post's pollen analysis method also owed much to the peat-climate classification schemes emerging around the turn of the twentieth century, which sought to answer questions related to the development and immigration of vegetation and civilizations in relation to climate. For example, in Denmark in 1876, Axel Blytt proposed a method to understand northern Europe's climatic phases by classifying different layers of peat, believing that the lighter peat was likely deposited during moister times and the darker layers in drier ones. In 1908, Swedish botanist Rutger Sernander slightly modified Blytt's classificatory scheme by adding temperature assessments to Blytt's moisture classifications, resulting in the Blytt-Sernander system. Sernander then used this system to correlate mire stratigraphy with one of the main problems of the day: he explained changes in lake levels, archeological periods, and plant immigration by linking them to climatic changes. While the Blytt-Sernander systems was not without critics, studies of peat stratigraphy helped answer questions about the development of human cultures and formed

deposits to determine the richest fuel sources for neutral and isolated Sweden during the First World War when he realized the potential insights about climate history that could be gleaned from pollen found in the various layers, one of his pre-war interests. His 1916 presentation and the Swedish-language conference proceedings, published in 1918, presented the method. As historian Christer Nordlund explains, von Post's "goal was ultimately to develop the empirical study of peat bogs – and their chronology – from a Quaternary geological point of view, and to produce knowledge significant to the interpretation of landscape and climate change after the Ice Age."[72]

This interest in climate and landscape was also pervasive in the ice-carved landscapes of North America, where a number of geologists and botanists relied on European postglacial theories to develop the new field of ecology and the named subfield of "paleoecology," as well as initiate ecological studies of environmental change which used pollen analysis.[73] Influential geologists in the United States included Thomas C. Chamberlin and Rollin D. Salisbury at the University of Chicago, and, later, Richard Foster Flint at Yale University. Chamberlin's interest, as well as that of geography colleague Salisbury, centered on the development of the physical landscape. Historian Gregg Mitman shows that the pair claimed that this interest resulted from a desire to understand how changes in the physical environment, especially geographic isolation and climatic fluctuations, affected the development and progress of organic life. In reality, they

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the main body of evidence for post-glacial climatic changes in northwest Europe until von Post suggested that pollen analysis could yield more detailed results. See, Axel Blytt, *Essay on the immigration of the Norwegian flora during alternating rainy and dry periods,* (Christiania: A. Cammermeyer, 1876); Jeff Blackford, "Peat bogs as sources of proxy climatic data: past approaches and future research," in *Climate Change and Human Impact on Landscape*, F. M. Chambers (ed.), (New York: Chapman & Hall, 1993): 47-48.

[72] Nordlund, "Peat Bogs as Geological Archives: Lennart Von Post et Al., and the Development of Quantitative Pollen Analysis during World War I"; Knut Faegri and Johannes Iversen, "Introduction to Forest Tree Pollen in South Swedish Peat Bog Deposits," in *Foundations of Ecology: Classic Papers with Commentaries,* ed. Leslie A. Real and James H. Brown (Chicago: University of Chicago Press, 2012), 456–59.

[73] Clements, *Plant Succession: An Analysis of the Development of Vegetation*.

were more interested in the development of abiotic factors, preferring to leave inquiry into the impact of physical environments on the biotic realm to colleagues, especially Henry Chandler Cowles.[74]

Cowles began a doctorate in geology at the University of Chicago in 1895, but maintained an interest in botany, which he had developed as an undergraduate. He sought out connections with botanists, including John Merle Coulter, who headed Chicago's botany department from 1896 to 1935. Coulter, who had founded the *Botanical Gazette* in 1876, was part of the generation of botanists who were professionalizing during the 1880s and 1890s: following their European counterparts, American botanists abandoned their focus on classification and description in order to take up problems in experimental botany and plant physiology.[75] These interests put botanists in conversation with the nascent field of ecology, which "represented a field approach to laboratory physiology" given its focus on responses on plants to environmental influences. Coulter was interested in ecological ideas, favorably reviewing the German edition of Eugenius Warming's study of plant distribution and its relation to soil in 1896, and introducing students like Cowles to Warming's ideas. Cowles was so taken by Warming's ideas that he hoped to learn Danish to read *Plantesamfund* (1895) in the original.[76] For his doctoral thesis, completed in 1898, Cowles used Warming's theory, along with those of Salisbury and Chamberlin, to piece together the vegetation history of the Lake Michigan dunes, explaining how the physical environment shaped organic life.

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[74] Mitman, *The State of Nature*, 10–20.

[75] On European ecological ideas, see Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900*. On Cowles, see Mitman, *The State of Nature*, 16–20; Eugene Cittadino, "A 'Marvelous Cosmopolitan Preserve': The Dunes, Chicago, and the Dynamic Ecology of Henry Cowles," *Perspectives on Science* 1, no. 3 (1993): 520–59; Charles C. Adams and George D. Fuller, "Henry Chandler Cowles, Physiographic Plant Ecologist," *Annals of the Association of American Geographers* 30, no. 1 (1940): 39–43; Victor M. Cassidy, *Henry Chandler Cowles: Pioneer Ecologist*, First Paperback Printing edition (Chicago: Kedzie Sigel Press, 2007).

[76] Cittadino, "A 'Marvelous Cosmopolitan Preserve': The Dunes, Chicago, and the Dynamic Ecology of Henry Cowles," 534.

Cowles' thesis was one of the first on succession, which sought to describe the changing patterns in the species structure of an ecological community over time. Cowles work initiated later research on succession, most notably by Frederic Clements who helped ensure that the problem of succession was the dominant problem in ecology during the first half of the twentieth century.[77] More importantly for this argument, Cowles was grappling with issues of scale that became important for ecologists at the turn of the twentieth century. In Cowles' dissertation, published in four parts in the *Botanical Gazette* in 1899, he outlined the scope of physiographic ecology.[78] Cowles argued that there were two scales on which scientists could study plant distribution: the regional and the local. Regional studies focused on the formation, a unit applied to the American grasslands by Frederic Clements and Roscoe Pound in 1898.[79] In regional studies, climatic factors were the significant determinants of plant distribution, whereas the local studies preferred by Cowles emphasized edaphic factors, including soil, slope, and light as the key drivers governing vegetation change. Cowles' preference for this scale and these drivers harkened back to the physiographic processes that Salisbury and Chamberlin had claimed transformed abiotic landscapes. Despite this preference, Cowles recognized that there were other units of analysis that emphasized different factors as controlling vegetation development:

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[77] Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*, chap. 4; Worster, *Nature's Economy: A History of Ecological Ideas*, chap. 11. See, also, Robert P. McIntosh, "Succession and Ecological Theory," in *Forest Succession*, ed. Darrell C. West, Herman H. Shughart, and Daniel B. Botkin, Springer Advanced Texts in Life Sciences (New York: Springer, 1981), 10–23.

[78] Henry Chandler Cowles, "The Ecological Relations of Vegetation on the Sand Dunes of Lake Michigan," *Botanical Gazette* 27, no. 2 (February 1899): 95–117; Henry Chandler Cowles, "The Ecological Relations of Vegetation on the Sand Dunes of Lake Michigan," *Botanical Gazette* 27, no. 3 (March 1899): 167–202; Henry Chandler Cowles, "The Ecological Relations of Vegetation on the Sand Dunes of Lake Michigan," *Botanical Gazette* 27, no. 4 (April 1899): 281–308; Henry Chandler Cowles, "The Ecological Relations of Vegetation on the Sand Dunes of Lake Michigan," *Botanical Gazette* 27, no. 5 (May 1899): 361–91.

[79] These ideas had precursors among plant geographers who had long held that vegetation formations were ontologically distinct units, separate from the individual plants comprising them. Geographers such as Alexander von Humboldt, August Grisebach, and Oscar Drude also held that formations were heavily dependent on climate. See, Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*, esp 87-99.

Cowles' review of Clements and Pound's work suggested that it was "too early as yet to predict whether the direction of future work in plant geography" would focus more on local or regional factors. Instead, he suggested a division of labor whereby phytogeography would study "larger problems of distribution" and deal "with extensive formations, while ecology will have to do more with local and habitat relations, including anatomical as well as field investigation."[80] Cowles, who took more of a teaching role after 1901, was influential in introducing his vision of plant ecology to a generation of students, including Paul Sears who took one of Cowles' many field courses.[81]

Clements' competing vision was also influential. In 1916, Clements published *Plant Succession*, which laid out the influential climax theory of vegetation change in great detail.[82] Self-consciously styled as an alternative to Cowles' model of vegetation development, the book grew out of Clements' interest in the development of forests after he spent the summer of 1898 in the Colorado Rocky Mountains, as well as engagement with the work of Alexander von Humboldt, August Grisebach, and Oscar Drude, all of whom held that vegetation was heavily dependent on climate. From fieldwork and theory, historian Ronald Tobey describes how Clements began to conceive of the formation as an organism, a concept which he developed in *The Development and Structure of Vegetation*, published in 1904, and *Research Methods in Ecology*, which appeared in 1905. In *Plant Succession*, published in 1916, Clements attempted to formally systematize the philosophy of the organism and its development. He sought to apply the

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[80] Henry C. Cowles, "The Phytogeography of Nebraska," *Botanical Gazette*, May 1898, 372.
[81] Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900*.
[82] On Clements, see Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*; Worster, *Nature's Economy: A History of Ecological Ideas*; McIntosh, *The Background of Ecology: Concept and Theory*, esp. 76-85; Joel B. Hagen, "Organism and Environment: Frederic Clements's Vision of a Unified Phsyiological Ecology," in *The American Development of Biology*, ed. Ronald Rainger, Keith R. Benson, and Jane Maienschein (Philadelphia: The University of Philadelphia Press, 1988), 257–80; Joel B. Hagen, "Clementsian Ecologists: The Internal Dynamics of a Research School," *Osiris*, 2nd Series, 8 (January 1, 1993): 178–95; Kingsland, *The Evolution of American Ecology, 1890-2000*, esp. 144-145; Hagen, *An Entangled Bank*.

concept of the irreversible directional development of the forest formation to other vegetation communities, including the prairie, which had once been Clements' main area of focus. As Clements conceived formation development, climate was particularly important: formations progressed towards the climatic climax, which represented the stable stage of mature vegetation formations.

Plant Succession reveals the ways that Clements was thinking about the existence of natural units and the scale of ecological change. For him, there were large-scale, climatically influenced natural vegetation units, known as formations. Formations, such as the deciduous forest climax of eastern North America or the prairies-plains grassland climax of the midcontinent, were biologically bounded, forming recognizably distinct units much like distinct organisms.[83] Clements claimed he could determine the boundaries of formations using the quadrat, one-to-five meter plots placed randomly over a landscape where ecologists would count the species present. Clements had developed the quadrat with Pound in the 1890s to quantify vegetation found in formations. With plots strung across the landscape, ecologists could quantitatively determine the transition from one formation to another, as the dominant species were replaced by others.[84] From these quantitative measurements, Clements could claim that formations were separated from each other by ecotones, boundary regions between formations where the plant density was too sparse to represent the true formation.[85]

Clements also thought he would better understand the development of climax communities by examining their progression through time. Returning to study the same quadrats was one option, but Clements also thought that much longer timescales were necessary to truly

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[83] Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*, esp. chapter 3.

[84] Tobey, 68.

[85] Tobey, 175–76. Clements first introduced the ecotone in the *Botanical Survey of Nebraska* (1904).

understand the factors driving development. He thus devoted over a fifth of his lengthy *Plant Succession* volume to describing plant succession through geological time, telling readers that they could use macrofossil evidence to trace climate and vegetation dynamics through the late Mesozoic, approximately 66 million years ago.[86] In this context, he introduced the neologism paleo-ecology to his readers. For Clements, long-term studies made true formations visible, and made clear that climate was driving development of these large-scale formations.

Clements' ideas were not universally accepted, which helps illustrate the concern among ecologists in the early twentieth century about the units that ecologists were studying, and the ways that different drivers of change appeared at different scales.[87] Some ecologists, including Cowles, worried about the length of time that could be appropriately investigated. Cowles thought that long-term studies of the kind Clements proposed were incredibly difficult because various cycles, "each moving independently of the others and at times in different directions," all played a hand in a process like succession. Even though Cowles was reluctant to speculate on the messy past with its incomplete fossil record and entwined cycles, Cowles remained committed to the past in his studies because, as two of his students remembered, Cowles "always regarded the present as the direct outcome of what has happened in the past."[88] In order to study the past without these problems, he focused on areas like the Lake Michigan sand dunes where changes occurred quickly enough to be observed. The dunes were an ideal environment to study change over time because their instability allowed ecologists to infer the stages of succession using the spatial distance from the water as a proxy for time. In the so-called space-for-time substitution,

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[86] Clements, *Plant Succession: An Analysis of the Development of Vegetation*.

[87] Some of the more forceful critiques centered on Clements organism, see A. G. Tansley, "The Use and Abuse of Vegetational Concepts and Terms," *Ecology* 16, no. 3 (1935): 284–307; Arnold G. van der Valk, "From Formation to Ecosystem: Tansley's Response to Clements' Climax," *Journal of the History of Biology* 47, no. 2 (2014): 293–321; Anker, *Imperial Ecology: Environment Order in the British Empire, 1895-1945*.

[88] Adams and Fuller, "Henry Chandler Cowles, Physiographic Plant Ecologist," 41.

the primitive beach formation progressed towards established oak forests as one moved farther from the shore. In this environment, Cowles argued that "exact study year by year… makes it possible to determine not only the trend of succession, but the exact way it comes about."[89] His studies of these relatively small areas found that succession resulted from the influence of local factors, including soil, slope, and light, rather than climate.

Others, notably Henry A. Gleason, expressed concern about ecologists' spatial scales and units more directly. Gleason had initially worked within Clements' theoretical framework as he studied vegetation in his native Illinois. Around 1918, Gleason began to express doubts about Clements' organismic metaphor, as well as the boundaries of formations. He articulated his objections most clearly in 1926 when he argued that Clements' formations assumed too much homogeneity when, in fact, there was much more diversity in vegetation, which varied across areas only by degree. In some instances, Gleason suggested that the distribution of plants was nearly mathematically random, and therefore not useful in determining the distribution of climate. In this reading, the formation was not a natural unit.[90]

In these disputes about the boundaries of vegetation units and the factors that affected vegetation development, we see ecologists trying to come to terms with patterns which emerged at various temporal and spatial scales. While the unit of ecological analysis remained an open question in the first decades of the twentieth century, ecologists had tools like the quadrat on which to base their preferred unit. They also had direct contact with vegetation from extensive time in the field, which helped color their interpretations of units: by reading the landscape, they

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[89] Henry C. Cowles, "The Causes of Vegetative Cycles," *Botanical Gazette* 51, no. 3 (March 1911): 181–82.

[90] As Frank N. Egerton has argued, ecologists had a variety of terms to discuss which group of organisms form a distinct entity. His work has examined why particular investigators thought that entity was organized and functioned in the ways they proposed. See, Frank N. Egerton, "History of Ecological Sciences, Part 54: Succession, Community, and Continuum," *The Bulletin of the Ecological Society of America* 96, no. 3 (July 2015): 426–74.

gained a sense of ecological units which worked well for their areas of inquiry.[91] These tools and direct connections were not available to pollen workers because of their temporal separation from the landscapes they studied. This separation meant that constructing boundaries became even more difficult once ecologists began to use proxy evidence to examine the questions of their parental disciplines by adding a geologic perspective.

The Rising Popularity of Pollen Statistics

Although pollen workers widely recognize Lennart von Post as the method's founder, he was not particularly influential in spreading pollen statistics as a method. He rarely travelled, hardly ever published in international languages, and did not attract many students. Instead, one of the few students who he did attract, Gunnar Erdtman, helped ensure that geologists interested in postglacial periods and the environmental influence idea learned about pollen statistics as a way to answer questions related to vegetation and climate development. As part of his efforts to spread pollen statistics, Erdtman authored a short piece in *Science* in 1931. The article described pollen analysis as a new technique in "paleo-ecology."[92] In the same year, he received a Rockefeller scholarship, which allowed him to undertake pollen investigations in North America and disseminate the method. The technique quickly spread, with Erdtman publishing annual or biannual bibliographies on pollen analysis to help practitioners in several countries keep track of

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[91] Clements and Pound thought that Drude had mischaracterized North America's regions because he was not properly acquainted with the vegetation, having never visited the Great Plains. As Tobey explains, Drude divided North America into fourteen regions, some of which Clements and Pound thought were grossly inaccurate because they did not correspond to their intimate knowledge of the prairies and botanical surveys. Clements, for instance, did not distinguish between the Canadian and Missouri prairies, whereas Drude did. Drude also did not include the Rocky Mountain foothills in the prairie, whereas Pound and Clements did. See, Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*, 64.

[92] Gunnar Erdtman, "Pollen-Statistics: A New Research Method in Paleo-Ecology," *Science*, New Series, 73, no. 1893 (April 10, 1931): 399–401.

the work being performed all over the world.[93] Given these efforts pollen analysts could claim that, by the mid-1920s, their method had become "the dominant method for investigation of late-quaternary vegetational and climatic development."[94]

Pollen statistics became "a new field of rich scientific activity," in part because pollen was abundantly available to solve questions related to environmental change, especially compared to macrofossils, which were the other way to study the past.[95] Unlike the scattered and fragmentary evidence from fossilized leaves, needles, cones or stems, pollen workers often collected thousands of pollen grains from different vegetation throughout their sediment columns. Some early workers estimated that pine forests could produce 75,000 tons of pollen annually, with an established pine community producing around 350 million grains each year.[96] In addition to the vast quantities of pollen found in many environments, the yearly production of pollen meant it was possible for pollen analysts to develop chronologies of environmental change using these methods. As Erdtman explained, "peat bogs and the sediment banks on lake-bottoms become archives of vegetational history imprisoning pollen grains, seasons after season, millennium after millennium."[97] These seasonal depositions allowed pollen workers to trace succession through geologic time, something that was rarely possible with fragmented evidence common in macrofossils.

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[93] Erdtman, "Literature on Pollen-Statistics Published before 1927." Erdtman continued to publish bibliographic lists over the next thirty years, until 1959, when the new journal *Pollen et Spores* began to publish literature lists as supplements. Manten estimates that, between 1836 and 1966, over 15,000 articles were published that had some relationship to fossil pollen and its techniques. See, A.A. Manten, *Bibliography of Palaeopalynology 1836-1966*, vol. Special Volume, Review of Palaeobotany and Palynology (Amsterdam: Elsevier, 1969).

[94] Roger P. Wodehouse, "Review of Textbook of Modern Pollen Analysis," *Science*, New Series, 113, no. 2938 (April 20, 1951): 459.

[95] Faegri and Iversen, "Introduction to Forest Tree Pollen in South Swedish Peat Bog Deposits," 459.

[96] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 34.

[97] Gunnar Erdtman, *An Introduction to Pollen Analysis* (Waltham, MA: Chronica botanica company, 1943), 2.

Paleoecological studies using pollen sought to solve many of the same problems that Cowles, Clements, and Gleason were dealing with, including the factors responsible for changes in both vegetation and human societies. Paul Sears, who had likely learned about the technique in 1925 once he had taken up a professorship in botany at the University of Nebraska, had a number of reasons to turn to pollen analysis.[98] Since childhood, the Native Ohioan had been fascinated by vegetation change around his Bucyrus home. As a child, he had heard his father's stories of ambushing Indians hidden in tall prairie grass around the family home. The younger Sears wondered what the vast tall-grass prairie, of which he found only remnants, had once looked like.[99] As a young adult, he set about answering this question.

Sears posited that human settlement had displaced the native vegetation known to the previous generation. To trace the progression of native vegetation, he attempted to create maps of the pre-settlement vegetation distribution of Ohio, which he would compare to present-day vegetation. He created maps using data from witness trees, the trees recorded at the corners of township lines when land surveys took place in the late eighteenth and early nineteenth centuries.[100] He shared the first of these maps in 1925, but had described the method to a group of Ohio naturalists in 1919 and in *Science* in 1921 (Figure 1).[101] Sears also overlaid soil, topography, and glaciation data on the maps, which allowed him to make claims about the distribution of trees within the state in relation to physical features in ways similar to Cowles.

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[98] Shane, "Paul B. Sears' Contributions to the Development of Paleoecology."

[99] Ronald L. Stuckey, "Paul Bigelow Sears (1891-1990): Eminent Scholar, Ecologist and Conservationist," *The Ohio Journal of Science* 109, no. 4/5 (December 2009): 140–44.

[100] Gordon G. Whitney and Joseph P. DeCant, "Government Land Office Surveys and Other Early Land Surveys," in *The Historical Ecology Handbook: A Restorationist's Guide to Reference Ecosystems*, ed. Dave Egan and Evelyn A. Howell (Washington, DC: Island Press, 2001), 147–72.

[101] Paul B. Sears, "Vegetation Mapping," *Science* 53, no. 1371 (April 8, 1921): 325–27; Paul B. Sears, "The Natural Vegetation of Ohio," *The Ohio Journal of Science* 25, no. 3 (May 1925): 139–49. Sears followed up on this original paper with two others. See, Paul B. Sears, "The Natural Vegetation of Ohio II - The Prairies," *The Ohio Journal of Science* 26, no. 3 (May 1926): 128–46; Paul B. Sears, "The Natural Vegetation of Ohio III - Plant Succession," *The Ohio Journal of Science* 26, no. 4 (July 1926): 213–31.

When Sears organized the data into township-sized vegetation units, the resulting maps showed that geological and climatic features influenced native vegetation. Since these geologic and climatic features had largely remained constant during the period under investigation, Sears claimed that human settlement had altered the native vegetation.

But Sears, reflecting widespread beliefs in climate determinism, believed that climate changes during certain periods led to cultural and vegetation shifts, and wondered at what temporal and spatial scales such climatic effects might be visible. He thought that these kinds of questions could best be answered using pollen analysis, since witness tree data was often only available for limited time periods.

One of Sears' graduate students, Phyllis Draper, offered the first American explanation of pollen analysis in 1928, indicating that it was a useful tool for studying climate. Sears had moved to the University of Oklahoma in 1927 to become professor and chair of the botany department, likely introducing students like Draper to the technique shortly after arriving at his new post. He also provided Draper with "borings" taken from an Ohio bog, from which Draper then extracted pollen. After counting the grains found in various layers, she determined that she could not discern any patterns. Still, she thought that fossil pollens from could be "valuable aids in tracing the succession of plant associations… during post-glacial times. These analyses also give clues to the various types of climates which have prevailed throughout the period; because the types of vegetation existing at any one time are dependent on the climate."[102] Her second paper, published in 1929, found successional patterns, and gave some clues about climate surrounding

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[102] Phyllis Draper, "A Demonstration of the Technique of Pollen Analysis," *Proceedings of the Oklahoma Academy of Science* 8 (1928): 63.

two bogs in Ohio, thereby displaying the potential of technique.[103]

Likely with the help of students who did most of the counting, Sears began interpreting to other Ohio bogs, publishing results from Mud Lake Bog in 1931 and Bucyrus Bog 1932.[104] From these data, he produced pollens diagram (Figure 2) that allowed him to track climatic shifts, which revealed that, in Ohio, there were two dry periods in post-glacial time, one cool and one warmer. There were also two humid periods in the past, with the present tending towards more humid conditions.[105] Through the 1930s, Sears also put his Ohio climate data together with other pollen workers who were working at sites across eastern North America. He found climatic similarities from Iowa in the west to Virginia in the east, and Arkansas in the south to southeastern Canada in the north (Figure 3). These findings suggested that climatic influences were more extensive than the local or even the regional; instead climate operated at the level of the subcontinent, with edaphic factors responsible for the different communities that thrived in different locations.[106]

Eager to determine the scale of climatic changes, he soon compared North America's climate history to Japan's and Europe's in order to suggest even larger climatic patterns.[107] The climatic shifts that Sears observed on regional and global scales indicated that the postglacial

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[103] Phyllis Draper, "A Comparison of Pollen Spectra of Old and Young Bogs in the Erie Basin," *Proceedings of the Oklahoma Academy of Science* 9 (1929): 50–53.

[104] Paul B. Sears, "Pollen Analysis of Mud Lake Bog in Ohio," *Ecology* 12, no. 4 (October 1, 1931): 650–55; Shane, "Changing Palynological Methods and Their Role in Three Successive Interpretations of the Late-glacial Environments at Bucyrus Bog, Ohio, USA."

[105] Paul B. Sears, "Pollen Analysis of Mud Lake Bog in Ohio," *Ecology* 12:4 (October 1931): 650-55.

[106] Paul B. Sears, "The Archaeology of Environment in Eastern North America," *American Anthropologist* 34, no. 4 (December 1932): 610–22; Paul B. Sears, "Postglacial Climate in Eastern North America," *Ecology* 13, no. 1 (January 1932): 1–6.

[107] Paul B. Sears, "Climatic Change in Japan," *Science*, New Series, 78, no. 2023 (October 6, 1933): 312; Sears, "Climate in Northern Hemisphere since Ice Ages: Studies of Fossil Pollen, Accumulations in Old Lake Beds, and Other Clues Reveal How Climate Has Shifted and Its Effect on Man." Deevey did the same, writing that in pollen analysis the "most important thing I can do, in view of the possibilities of transatlantic correlation" is to correlate various pollen Atlantic cores. See, Edward S. Deevey to W. Armstrong Price, November 26, 1945, Edward S. Deevey Papers, Special and Area Studies Collections, George A. Smathers Libraries, University of Florida.

period was not the climatically stable period that many scientists believed it to be because there had been “marked fluctuations of both temperature and moisture since the last, or Wisconsin, glaciation.”[108] This insight about climatic shifts caused Sears to examine the effects of those shifts.

Echoing the climatic determinism popular at the time, Sears thought these climatic shifts might have influenced the development of past civilizations. He set out to test this hypothesis about the influence of climate in a 1931 paper on the mound-building cultures in the upper Mississippi and Ohio valleys. He argued that an extensive warm and dry period had existed around 1200 BC, which had extended the maize-growing region much farther to the north and east than current growing conditions allowed. These conditions resulted in the flourishing of mound-building cultures in that region.[109] Yet, when the moisture increased and the temperatures began to cool, Sears argued that the forest moved westward and the best conditions for growing maize shifted, which helped to explain “the disappearance of the dense mound-building agricultural population and its replacement by the hunting tribes of the forest.”[110] In a subsequent article, published in 1934, Sears went global, explaining the agriculture and civilization in Asia Minor, Mexico, and the Mediterranean in terms of the prevailing dry character of the postglacial climate, which “encouraged the development of great interior grasslands, suitable to a vigorous pastoral population.” Yet, he went on to say, “so slight was the margin of safety in these areas

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[108] Sears, “Postglacial Climate in Eastern North America,” 1. Part of this desire to know more about these changes was a concern about Ice Ages. The threat of subsequent glacial periods, with their capacity to destroy landscapes, loomed large, with some positing that it was probable that we “have been, for the last 4000 years, travelling towards a new ice age from which we are, however, still separated by an unknown number of thousands of years.” Understanding the likely path of the interglacial would help humans understand the causes of “the immense wave of cold” that approached. See, Von Post, “The Prospect for Pollen Analysis in the Study of the Earth’s Climatic History,” 209.

[109] Paul B. Sears Paul B. Sears, “Recent Climate and Vegetation A Factor in the Mound-Building Cultures?” *Science* 73:1902 (June 12 1931): 640-641; and Paul B. Sears, “The Archaeology of Environment in Eastern North America,” *American Anthropologist* 34 (1932): 610-622.

[110] Paul B. Sears, “Climate in Northern Hemisphere Since Ice Ages,” 8.

that climatic oscillations not of major magnitude might cause a wholesale shifting of populations."[111] Because slight climate oscillations had occurred, civilizations had been affected. In these articles, we see Sears postulating climatic drivers of changes in both vegetation and human societies, and the suggestion that these drivers operated on quite large scales.

While Sears made confident predictions about climate as a regional or global phenomenon, with potential effects on civilizations and vegetation, the question about scale and boundaries still loomed in paleoecology. I deal with debate among lumpers and splitters in the penultimate section of this chapter, but turn here to focus on questions about methodology. Questions about how proxies represented the past became the more pressing issue when paleoecologists applied pollen analysis to these larger questions.

These questions about how pollen could be used to understand environmental change and its driving forces had surfaced immediately after von Post's lecture. Swedish botanist Henrik Hesselman had quickly asked whether pollen could yield stratigraphically reliable results because of the pollen dispersal processes, which might mix pollen coming from different regions. This critique surfaced again and again during the twentieth century.[112] For example, in 1969, Edward Smith Deevey stated that when we "ask ourselves how we might tell the difference between a minor climatic change and a major or nonlocal interference with vegetation by man [the subject of chapter 2], we confront the difficulty that Margaret Davis has emphasized in all her work: the quantitative nature of pollen evidence is illusory if one does know what area or from what distance or from how many plants the pollen came."[113] These problems could not be solved with methods like the quadrat or direct experiences in the field. Instead, as the rest of

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[111] Sears, 8.
[112] Davis, "On the Theory of Pollen Analysis," 901–2.
[113] Edward S. Deevey, "Coaxing History to Conduct Experiments," *BioScience* 19, no. 1 (January 1969): 41.

this chapter illustrates, pollen workers developed strategies unique to working with proxies so that their conclusions would still be seen as authoritative. Although these issues reappear throughout the history of pollen analysis, this chapter focuses mainly on discussions of these issues in the first half of the twentieth century when pollen workers first grappled with these problems.

Identifying and Exploiting Local Features

Given questions about how pollen represented the variable of interest, paleoecologists used a number of strategies to make their data easier to interpret. The process of selecting a research site was the first opportunity to make choices that would facilitate the interpretation of thousands of pollen grains and better define the boundaries of their objects of inquiry.

Pollen workers sought sites where pollen production, dispersal, deposition, and preservation were nearly uniform, and where geologic processes were relatively stable. Geological processes might mix pollen produced at different times or from different places, upsetting the chronology and spatial understanding of past vegetation communities and climates. For example, winds could sweep away pollen-containing sediment in a process known as “deflation.” Deflated sites and areas with high erosion were not suitable for sampling because much of the record would be missing or mixed to the point of being incomprehensible. By avoiding these kinds of sites, paleoecologists were making their task of interpreting pollen analysis slightly easier.

In order to find research sites where processes like deflation and erosion were least prevalent, pollen workers went into the field. They also relied on the field experience of others to identify locations where sedimentation and fossilization processes were most uniform and least destructive of the evidence. For example, Yale PhD student Edward Smith Deevey, whose 1938 dissertation used pollen analysis to study the development of lakes, had consulted with geologists before undertaking his pollen work. These geologists had helped him identify areas where winds were less frequent and where the water was more likely to be stagnant: the less sediment washed away over time or mixed with foreign sediment, the more likely pollen would be preserved in the order and place in which it was produced and deposited.[114] By understanding natural processes, pollen workers like Deevey chose sites whose natural processes were most likely to preserve a column of sediment. Paul Sears did the same. He was eager to spend a year at Harvard, where there were more botanists and geologists "of great skill and judgment" than at Oklahoma, where he had become professor of botany in 1927. These specialists had the skills to give pollen analysis "adequate backing," by identifying geologic processes with the potential to impact his results.[115]

Yet pollen workers could not solely choose sites with favorable geological conditions for their research sites. They took other factors into account when selecting sampling areas. A common consideration was accessibility. Sometimes they found sediments below too much water or they were frozen and inextricable for half the year.[116] Other sites were too far away from transportation routes, especially when sampling required heavy sampling apparatus.[117] Access to other sites might be limited because of wary officials: when Deevey traveled to Mexico on a collecting trip, he asked for a letter from the Mexican government granting permission to collect specimens, which, he said, would "perhaps free me from the charge of

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[114] Edward S. Deevey to W. Armstrong Price, October 18, 1944, Edward S. Deevey Papers, MS 239, box 1, Special and Area Studies Collections, George A. Smathers Library, University of Florida.

[115] Paul B Sears to Edgar B. Howard, February 12, 1935, Paul B. Sears Collection, MS 455, box 1, folder 9, Special Collections, University of Arizona.

[116] Robert Sayles to Paul B. Sears, November 8, 1934, Paul B. Sears Collection, MS 455, box 1, folder 19, Special Collections, University of Arizona; Edward S. Deevey to Paul B. Sears, October 7, 1943, Paul B. Sears Collection, MS 455, box 1, folder 5, Special Collections, University of Arizona.

[117] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 54.

trespassing" because "my presence on some of your lakes with a boat and limnological equipment might arouse the suspicion of local officials and wardens, at least it often does in the United States."[118] There was also the time and expense of traveling to any research site, which might cause some researchers to work at more difficult sites closer to home rather than travel to sites further afield. Sears applied to the National Research Council a number of times throughout the 1920s and 1930s asking for funds to cover travel-related expenses and to support technical assistants collecting and preparing material.[119] In these applications, Sears routinely noted the lack of time to carry out this work, as well as a lack of assistance, but promised an "unremitting" search for pollen in order to reconstruct past climates.[120]

Despite the care about site-selection, local features and geologic processes affected proxies from even the most favorable sites. In order to account for these features in their interpretations of the data, pollen workers carefully documented the features of their sampling sites (for an example field notebook page, see Figure 4). They recorded information on surface vegetation, sediment type, water source, prevailing wind conditions, the presence of beaver dams, and much, much more. All of these features might influence the production, dispersal, deposition, and preservation of pollen, either making it harder or easier to interpret pollen data depending on the extent to which these processes obscured the connection between fossil pollen and its parental vegetation formation, and the relation to climate.

Paleoecologists also recorded larger geomorphological features of a site. They thought topography particularly affected the dynamics of proxy data. Since the effects could be

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[118] Edward S. Deevey to Fernando Obregon Fernandez, May 9, 1941, Edward S. Deevey Papers, MS 239, box 1, Special and Area Studies Collections, George A. Smathers Library, University of Florida.

[119] Paul B. Sears, "Correspondence National Research Council General 1923-1933" (n.d.), Paul B. Sears Collection, MS 663, box 91, folder 175, Manuscripts and Archives, Yale University.

[120] Paul B. Sears to D. White, April 15, 1930, Paul B. Sears Collection, MS 663, box 94, folder 175, Manuscripts and Archives, Yale University.

pronounced, pollen workers often chose to undertake detailed studies to understand the effects on their data, rather than diving in with paleoecological reconstructions. For example, in the mid-1930s, pollen workers located pollen in arid environments in Arizona where they had not expected to find well-preserved samples. Even though they wanted to use these data to understand how human communities responded to climatic shifts, analysts first tried to understand the region's geomorphological features and how they might influence pollen profiles. Sears, for instance, argued, "no safe conclusions can be drawn from this region on the basis of pollen analysis alone. The erosion history of the entire region must be developed. Terraces and buried soils abound and their relationships must be established as a necessary step in the application of pollen analysis of silt deposits."[121] Without better data on soil dynamism, pollen workers claimed they had "little but conjecture to guide us," which led to a fear that researchers had "carte blanche" to come to any conclusion they wanted about the vegetation, climate, and peoples of the southwest.[122] Only careful research on natural processes would move conclusions based on proxy evidence from conjecture to fact. The first textbook on pollen analysis, published in 1950, thus reminded workers that the aim of field work was to carefully record and understand the local conditions of the deposit; only then would they be able to begin their interpretations.[123]

On the whole, field work was field work "wet and rough" and sometimes incredibly difficult.[124] One of the students Sears sent out to collect samples, complained that his collecting was "proceeding very slowly due to the unusually wet weather… it has rained, sometimes terrifically, every day. Mosquitoes are the worst in the history of the station for this time of the

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[121] Paul B Sears, "Pollen Analysis as an Aid in Dating Cultural Deposits in the United States," in *Early Man,* ed. G.G. MacCurdy (London: J.B. Lippincott Co, 1937), 65.

[122] Sears to Howard, February 12, 1935.

[123] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 50; 84; 94–95.

[124] Erdtman quoted in Sears, "Common Fossil Pollen of the Erie Basin," 95.

year, while blackflies are bothering about as usual."[125] Pollen workers needed to overcome these difficulties in order to understand how environments and their different geologic processes affected pollen samples.

Pollen Dispersal and the Spatial Scale of Pollen Data

Observations in the field also attuned pollen workers to features of pollen that potentially made it a problematic source for reconstructing past environments. Immediately after von Post's first lecture on pollen statistics in 1916, those who studied pollen expressed concern about differential production, dispersal, deposition and preservation of pollen from different tree genera. Through the first half of the twentieth century, these concerns solidified into three broad sources of error, which, critics argued, would limit fossil pollen's ability to represent past communities. As the first textbook on pollen analysis explained in 1950, these problems were "known and feared during the whole history of pollen analysis."[126] The first difficulty resulted from the heavy pollen production of some genera, resulting in over-rerepesentation in pollen data relative to their abundance in vegetation, and the small amount of pollen produced by other genera, resulting in under-representation. A second problem related to the differential destruction of pollen grains. Differential production and preservation had the potential to over- or under-represent some vegetation, impairing climatic reconstructions that relied on the percentages of pollen types found in each sample. Lastly, pollen workers worried about the different dispersal distances of pollen from different types: some was dispersed locally, while others could travel long distances. This last problem made it difficult to know the area that pollen analysts were studying; was their object of inquiry a regional vegetation community or a local formation or a

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[125] Letter from A. Sharp to P.B. Sears, June 26 1928, box 63, folder 854a, coll. MS663, Paul Bigelow Sears Papers, Manuscripts and Archives, Yale University.

[126] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 91.

mixture of both? Since paleoecologists argued that "knowledge of the approximate extent of the area represented in a pollen diagram is indispensable for an ecological understanding of the [pollen] diagram," this was a major problem for understanding pollen data.[127]

This section follows pollen workers as they discussed differential pollen dispersal before turning to their attempts to define the spatial boundaries of proxy data. As they had done when selecting a site, paleoecologists relied heavily on intimate connections to the field to understand how pollen travelled. They tried to understand natural mechanisms and patterns of pollen dispersal, thinking once again that only a proper understanding of natural processes would allow them to interpret proxy data, and determine the extent to which climate drove vegetation and culture.

As they studied pollen, scientists quickly recognized that pollen from different genera traveled different distances, and that these differences had the potential to result in climatic and vegetation reconstructions that had little resemblance to that had once existed. As Erdtman explained in 1943, "pine and spruce pollen is easily carried by the wind and may be scattered over considerable areas outside of the coniferous region. Therefore, it goes without saying that the evidence of pollen grains sometimes must be taken with a grain of salt."[128] Sears has also noted the problem when he introduced the technique in 1930, suggesting that "some species of trees might contribute pollen quite out of proportion to nearness and abundance of the species."[129] This fact might mean that some genera would be found in samples even when the parental vegetation was not found in the formation, or that vegetation not part of the community might be found in the sediment core.

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[127] Henrik Tauber, "Differential Pollen Dispersal and the Interpretation of Pollen Diagrams," *Danmarks Geologiske Undersøgelse*, II, 89 (1965): 10.

[128] Erdtman, *An Introduction to Pollen Analysis*, 2.

[129] Sears, "Common Fossil Pollen of the Erie Basin," 96.

As an example of the ways that differential pollen transport might influence paleoecological reconstructions, consider a sample of pollen that mostly resulted from local vegetation. The climatic tolerences of the genera present in the sample indicate a warm, moist period for several hundred years following the last ice age. Now consider a sample with the same pollen profile, but now assume that most of the pollen was transported over long distances. Now the same data might indicate that the conditions were unfavorable: it was possible that little vegetation grew near the sampling site because of cold or dry conditions; instead pollen from warm and moist-loving pollen was coming from another location, skewing climatic results that relied on this evidence.

Pollen analysts corrected for the differential dispersal of some pollen types by relying on situated knowledge, along with more formal, experimental methods. As an example of the former, pollen workers commonly subtracted long-distance pollen not considered part of the community. Paleoecologists claimed to know which pollen to eliminate because, when they went to the field, they recorded all the vegetation that surrounded their field site. When they counted pollen from their samples, they could discount any pollen from genera that was not found among the vegetation community or that did not progress towards that community. For instance, textbooks instructed pollen workers to subtract pollen from forest species if they were interested in reconstructing the vegetation of a steppe, which was typically composed of grass and shrubs with trees only present around rivers or lakes. Yet there was some possibility that what were now steppe lands had once been forested. To properly reconstruct the historical progression from forest to steppe, paleoecologists needed the “common sense” of botanical training in order to recognize successional patterns among their data, or to know when it would be appropriate to ignore or augment some data to properly capture data about the community.[130]

The advice that Sears offered to one of his students captures this strategy. In a 1932 letter, he told Dorothy Flynn, who was working to reconstruct an upland forest, that “the actual significance of your [pollen] counts… is obscured by the fact that willow is so predominant between 21 and 5 foot intervals” of the sediment core. Willow, in Sears’ experience, was never part of an upland forest community, so he encouraged Flynn to subtract this moisture-loving genus from her samples. When she did, she would find a “striking dry maximum” about fourteen feet down the sediment core. That point corresponded to the dry period roughly 4,200 years ago, which other paleoecologists had identified in their samples.[131] A skilled analyst could thus differentiate the uplands from the swamp by eliminating uncharacteristic pollen. In another case, Sears encouraged a student to ignore the hornbeams because, as he wrote, they “do not in my experience make up a very significant part of the community.”[132] These correction methods were mostly informal, relying on the analyst’s skill as an interpreter of nature’s patterns, rather than hard-and-fast rules about what corrections were appropriate.

Analysts also attempted to use morphological evidence to understand differential pollen transportation. As they developed pollen identification keys, paleoecologists became attuned to differences in the sizes and shapes of pollen grains. They then correlated pollen’s morphology with the transportation differences they had witnessed during pollen rains, noting that some genera had “buoyant pollen grains” while there were other “species with heavy, rapidly

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[130] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*.

[131] Paul B. Sears to Dorothy Flynn, March 9, 1932, Paul B. Sears Collection, MS 455, box 2, folder 7, Special Collections, University of Arizona.

[132] Paul B. Sears to Russell C. Artist, July 20, 1938, Paul B. Sears Collection, MS 455, box 2, folder 1, Special Collections, University of Arizona.

sedimenting grains."[133] More specifically, Sears claimed that the most distinguishing feature of the conifers were its two "air sacks or wings," which likely facilitated their long-distance flight (see Figure 5).[134] Morphology was thus a more formal way that pollen workers could justify the exclusion of some long-distance producers.

Paleoecologists also undertook empirical studies to understand pollen dispersal.[135] For example, in 1937, Gunnar Erdtman was set to sail from Gothenburg to New York City. The popularizer of pollen analysis as a method for reconstructing past environments was always eager to do a little work while on vacation, so he arranged for two vacuum cleaners to be set up on the ship's deck (see Figure 6). He planned to sample airborne pollen and spores on his week-long voyage across the North Atlantic.[136]

On Ertdtman's transatlantic voyage, he exposed seven vacuum cleaner bags to pollen and spores. He found pollen, especially pine, over 650 kilometers from the nearest landmass (see Figure 7). This discovery led him to posit that pine, which was already recognized as a long-distance traveler, might travel over 1000 kilometers over the open ocean, but that a number of other pollen varieties travelled much shorter distances.

These results caused Erdtman to ruminate on how pollen workers could correct for the presence of foreign pollen in certain environments: he suggested that the vast dispersal of pine

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[133] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 85.

[134] Sears, "Common Fossil Pollen of the Erie Basin," 101.

[135] Phyllis Draper (now Newcomb), who had published the first pollen diagram in North America in 1928, similarly traced the distance pollen travelled, finding pine pollen on slides over 450 miles from the source. See, "Pine Pollen Carried 450 Miles By Wind," *The Leaf-Chronicle*, February 13, 1950. Scientists also sampled the vertical height at which pollen could be found in the atmosphere using airplanes. This work was often for hay-fever studies, but help with pollen analysis as well. See, Gregg Mitman, *Breathing Space: How Allergies Shape Our Lives and Landscapes* (New Haven, CT: Yale University Press, 2007), 75–79; O.C. Durham, "The Pollen Content of the Air in North America," *The Journal of Allergy* 6 (1935): 128–49; "Upper Air Over Atlantic Will Be Searched for Pollens," *Science Newsletter*, September 2, 1939.

[136] Gunnar Erdtman, "Pollen Grains Recovered from the Atmosphere over the Atlantic," *Meddlelanden Fran Goteborgs Botaniska Tradgard* 12 (1937): 185–96; Edlund and Winthrop, "Sharing What He Saw."

meant that its presence in peat in the tundra and other unforested areas was not local, but came from distant lands.[137] The knowledgeable pollen analyst working in these areas would thus omit pine from their final pollen tallies. Others, working in regions with the potential for long-distance producers, would need to decide whether pollen from certain genera should be omitted as a foreign traveler or included as representative for the pollen profile of that place. He could offer no hard-and-fast rules because there were too many site-specific variables.

Some analysts were unhappy with experiments like Erdtman's because they did not think that his sampling methods corresponded to conditions in nature. Some critics were concerned about inquiries into pollen dispersal which sucked pollen out of the air or which used pollen that landed on protected surfaces such as microscope slides. These critics thought these methods could not answer questions about dispersal because these studies prevented pollen grains from following their natural dispersal and deposition patterns. Instead, the experiments were artificially stopping the pollen from dispersing. Instead, Knut Faegri and Johannes Iversen, two leading European pollen workers, encouraged others to sample pollen with "techniques corresponding to natural pollen sedimentation."[138] They thought that only "natural experiments" or observations in situ could justify pollen analysts' inferences, otherwise paleoecologists might reconstruct the past in ways that were not justified.

Henrik Tauber undertook these kinds of natural experiments, capturing pollen in carefully designed traps that only minimally interfered with pollen's natural dispersal and deposition. His findings led him to question the accepted model of pollen dispersal, which posited that pollen was released into the atmosphere and carried to high altitudes by thermal convection currents, thus spreading out over large areas, only to fall back down to the surface of lakes and bogs in a

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[137] Erdtman, "Pollen Grains Recovered from the Atmosphere over the Atlantic."

[138] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*, 34.

vertical rain, eventually settling to the bottom of the basin. Most paleoecologists thought that this picture of dispersal allowed them to reconstruct regional vegetation communities and climates. Yet Tauber warned that this description of pollen rain was not correct "in view of evidence from actual measurements of pollen drift."[139] He suggested that the traditional view did not account for wind velocities, wind turbulence, terminal velocities of fall, or the physical structure of the vegetation, all of which hindered or facilitated the movement of pollen, further complicating reconstructions based on proxy data.

In response, pollen analysts continued to claim that properly understanding natural processes would allow paleoecologists to reconstruct the past from proxy data. They would incorporate the factors Tauber suggested in order to properly understand the data. But the phenomena that Tauber was describing were very difficult to understand, especially through time. For example, when Tauber noted that the paleoecologist needed to understand the physical structure of vegetation, he was referring to things like vegetation density and the height of the canopy. In a dense forest with a high canopy, pollen would not travel as far as in a sparsely populated area. But paleoecologists could not know these features to correct their pollen data; they were trying to reconstruct vegetation communities with pollen analysis but, on Tauber's analysis, they needed to know those vegetation communities in order to provide a good reconstruction.

Setting the Boundaries of Proxy Data

Tauber's insight, along with continued uncertainties in understanding natural processes, led paleoecologists to propose a variety of ways to eliminate the interpretation problem. One proposal recognized that, statistically, with enough samples over an area of interest and enough

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[139] Tauber, "Differential Pollen Dispersal and the Interpretation of Pollen Diagrams," 8.

pollen grains counted within those samples, any abnormalities from foreign pollen or highly local conditions were likely to be minimized. This was the solution that Knut Faegri proposed to Deevey. After disparaging Deevey for the way pollen analysis was carried out in North America, Faegri encouraged Deevey to count upwards of 1000 grains of pollen from multiple samples in a region to justify his results about regional vegetation, climate, and their effects on prehistoric man. According to Faegri, there was no way to thoroughly establish vegetation change for an area without multiple samples at different sites within the same vegetation formation, followed by large pollen counts to minimize the effects of differential production, transportation, and preservation. Faegri claimed that reconstructions resulting from a small number of samples with a small pollen count were in an "imperfect state," since they were unlikely to represent a vegetation formation.[140] They were of no interest for climatic conclusions, since any conclusion that based on the climatic tolerances the vegetation community would not hold if paleoecologists reconstructed a vegetation community that did not correspond to one that existed in the past.

During the 1930s, pollen workers began to emphasize multi-site sampling techniques to ensure that they were reconstructing actual communities from pollen data. In a letter to Sears from Russell Artist, a graduate student at the University of Minnesota who was using pollen analysis in his dissertation, Artist was highly critical of work that drew conclusions from a single bog. Artist called such practices "dangerous" because local influences might dominate the sample. He preferred Sears' method of drawing on evidence from multiple bogs to create regional picture of climate.[141] Sears agreed with him, writing that he did not think that anyone "seriously defends, as a method¸ the use of single bogs at wide intervals. It simply represents the

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[140] Knut Faegri to Edward S. Deevey, n.d., Edward S. Deevey Papers, MS 239, box 1, Special and Area Studies Collections, George A. Smathers Library, University of Florida.

[141] Russell C. Artist to Paul B. Sears, June 28, 1938, Paul B. Sears Collection, MS 455, box 1, folder 2, Special Collections, University of Arizona.

first stage of reconnaissance, now happily passing." Sears told Artist that he was trying, in his recent work, "to undo whatever mischief arose from overworking the data on my first report," which used only a single bog. He now feared that he hazarded "a gambling guess on the meaning of some material" because he could not be sure of the local or regional effects on his proxy data.[142]

Even the author of the paper that Artist cited as evidence of the problematic "single-bog plan," Robert Prettyman, recognized that his limited sampling did not allow him to say much about the region. Prettyman called his results "tentative," suggesting that his conclusions about the succession patterns of the surrounding vegetation were "only conjecture" because highly local environmental conditions could influence the pollen deposits. He concluded, in much the same way as Artist, his critic, by calling for "a more extensive and exhaustive investigation of peat deposits in North America," suggesting that "until these have been made, any interpretations or reconstructions of postglacial migration and climate are provisional."[143]

With multi-site sampling, the boundaries of particular places mattered less: data from multiple sites could be aggregated to construct a vegetation type or climatic region. Samples which did not match the formation could be ignored if paleoecologists thought they were influenced by unique, local processes. Or they could eliminate certain pollen types by claiming that they originated from a different vegetation formation.

Despite the potential of multi-site sampling to more firmly establish paleoecology's spatial boundaries, some were unsatisfied with this solution. They thought it did not properly account for the varied local conditions they had witnessed in the field. Paleoecologists thought

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[142] Paul B. Sears to Russell C. Artist, June 1, 1938, Paul B. Sears Collection, MS 455, box 2, folder 1, Special Collections, University of Arizona. Underlining in original.

[143] Robert L. Prettyman, "Fossil Pollen Analysis of Fox Prairie Bog, Hamilton County, Indiana," *Butler University Botanical Studies* 4, no. 1 (1937): 39, 41.

that climatic, edaphic and topographic features all influenced vegetation, but with multi-site sampling, vegetation growing in slightly different soils or at different elevations might not be understood to be part of the same vegetation community or climatic region even if they should be. Splitters might incorrectly eliminate divergent pollen profiles which fell outside the expected range. They would be splitting natural units, as Sears complained to a colleague in 1936. He told them, “we have been too quick to differentiate regions that actually have similar climatic histories because any similarities are obfuscated by their unique glacial histories. Many climatic conclusions are thus unjustified. ”[144]

There was also the opposite fear, not that vegetation communities would be artificially split, but that many different communities were artificially being lumped together. This was Merritt Lyndon Fernald’s greatest concern. The Harvard botanist who was a respected scholar of the taxonomy and phytogeography of the vascular plants of eastern North America warned Sears against going to Scandinavia to learn more pollen analysis, especially if he wished to continue working on North American topics. While Scandinavia remained a center of pollen work, Fernald was “very much afraid that the Swedes are inclined to look upon North America as merely a part of Sweden and not to realize that it has had its independent geological history.” This lumping was inappropriate in Fernald’s eyes, who was “skeptical about much which has been published in regard to the Pleistocene” because his findings “so often seem to contradict the deductions which I read in the studies of others.” He went on to say that:

> [I] feel that there is a very large element of assumption in much of the work as, for instance, when I find Scandinavians coming over here without much knowledge of our geological history or our flora, and assuming that any bog in Nova Scotia or New

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[144] Paul B. Sears to Ernst Antvers, March 3 1936, Paul Bigelow Sears Collection, MS 455, box 1, Special Collections, University of Arizona.

> Brunswick, for instance, is post-Wisconsin; whereas many of the bogs have deposits of infusorial earth up to 75 feet in depth, a depth which certainly cannot have accumulated in post-Wisconsin time… in other words, Pleistocene geology, at least in the Northeast, is an essentially unworked field and it is wholly unsafe to assume that our bogs are all of anything like the same age. Consequently the deductions which I have seen have left me a little bit cool.[145]

Fernald thought that paleoecologists needed to carefully document local conditions, rather than working to create community-sized pictures of vegetation and climate. He argued that pollen workers should undertake more basic research before making climatic deductions.

As pollen analysts tried to balance the tendency to lump dissimilar places into the same climatic regions or to split data from the same ecological region into separate regions, they were speaking of a desire to set the spatial boundaries that captured vegetation formations and climatic regions. Essentially, they were seeking to understand the drivers of vegetation change, especially the role that climate might play on these communities, in much the same way as ecologists, and geologists. But much of this discussion had been, and continued to be, sidelined by larger questions about proxy methods. This chapter has shown that these kinds of questions dominated the field, which became sidelined because its "intuitive style… long made the method difficult to understand and evaluate."[146] Critics were only willing to ignore these problems when they realized the perspective that long-term studies offered to environmental crises, as my next chapter will show.

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[145] Merritt Lyndon Farnald to Paul B. Sears, October 22, 1934, Paul B. Sears Collection, MS 455, box 1, folder 7, Special Collections, University of Arizona.

[146] Margaret B. Davis, "Palynology and Environmental History during the Quaternary Period," *American Scientist* 57, no. 3 (Autumn 1969): 317.

Mediating Place and Scale

This chapter has argued that, although paleoecology and pollen analysis emerged out of a larger tradition concerned with the drivers of environmental change and the scale of natural communities, paleoecology ultimately dealt only slightly with these questions. Proponents of the method had hoped that longer timescales would help deal with these questions, but soon realized that interpretation of longer scales required a lot of work to make the data comprehensible. The central questions for those working with proxy data thus became how to make their proxy into reputable measures of past environmental conditions. That is, they had to solve a data problem before they could solve an interpretation problem.

This argument helps to show the mediating work that long-term studies require in order to be used to generate knowledge. We do not yet have an account of these inferential practices as a way of knowing; instead, the history of ecology is dominated with accounts of knowledge produced in place. These histories often connect the places where scientific knowledge is created with the scientific theories and programs that develop in those spaces, as well as related questions of authority and expertise.[147] As they argue, place becomes a site for making, experiencing, and generating knowledge.[148] This work on place initially focused on the universalizing and "placeless" knowledge produced in the lab, but moved to examine other sites

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[147] Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life*, 1st Princeton pbk. print. with corrections (Princeton, N.J: Princeton University Press, 1989); Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts*, 2nd ed. (Princeton, NJ: Princeton University Press, 1986); Steven Shapin, "Placing the View from Nowhere: Historical and Sociological Problems in the Location of Science," *Transactions of the Institute of British Geographers* 23, no. 1 (1998): 5–12; Diarmid A. Finnegan, "The Spatial Turn: Geographical Approaches in the History of Science," *Journal of the History of Biology* 41, no. 2 (2008): 369–88; Christopher J. Ries, "Armchairs, Dogsleds, Ships, and Airplanes: Field Access, Scientific Credibility, and Geological Mapping in Northern and North-Eastern Greenland 1900-1939," in *Scientists and Scolars in the Field: Studies in the History of Fieldwork and Expeditions*, ed. Kristian H. Nielsen, Michael Harbsmeier, and Christoper J. Ries (Aarhus: Aarhus University Press, 2012), 329–62.

[148] In one example, Mitman argues that place shapes knowledge about illness and health. His book is also a useful reminder of the interrelationship between people, knowledge and the environment, all of which are constantly being made and remade. See, Mitman, *Breathing Space: How Allergies Shape Our Lives and Landscapes*.

of knowledge production, such as museums, pubs, field sites, and scientific research stations.[149] For research done in the field, historians have noted scientists' strategies for interpreting the vagaries of nature at sites where direct control or continuous observations are rarely possible, as well as how scientists established and maintained authority when they worked in these sites.[150] This large body of literature emphasizes slightly different ways that knowledge and authority are gained and cultivated in situ, with historians framing their work around local knowledge, situated knowledge, bodily knowledge, and knowledge of nature through labor or play.[151] My account adds to this literature by demonstrating how pollen workers interpreted the natural environment when direct access to the natural world was not available. Instead, they only had indirect evidence of natural processes, which presented unique challenges to scientists hoping to understand this evidence.

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[149] Jane Maienschein, "History of American Marine Laboratories: Why Do Research At the Seashore?," *American Zoologist* 28, no. 1 (1988): 15–25; Anne Secord, "Science in the Pub: Artisan Botanists in Early Nineteenth-Century Lancashire," *History of Science* 32, no. 3 (1994): 269–315; Henrika Kuklick and Robert E. Kohler, eds., "Science in the Field," *Osiris* 11 (1996): 1–255; Lynn K. Nyhart, *Modern Nature* (Chicago: The University Of Chicago Press, 2009); Jeremy Vetter, "Rocky Mountain High Science Teaching, Research, and Nature at Field Stations," in *Knowing Global Environments: New Historical Perspectives on the Field Sciences* (New Brunswick, N.J.: Rutgers University Press, 2011), 108–34; Stephen Bocking, "Situated yet Mobile: Examining the Environmental History of Arctic Ecological Science," in *New Natures: Joining Environmental History with Science and Technology Studies*, ed. Dolly Jørgensen, Finn Arne Jørgensen, and Sara B. Pritchard (Pittsburgh: University of Pittsburgh Press, 2013), 164–78; Raf De Bont, *Stations in the Field* (Chicago: The University of Chicago Press, 2014); Samantha K. Muka, "The Right Tool and the Right Place for the Job: The Importance of the Field in Experimental Neurophysiology, 1880-1945," *History and Philosophy of the Life Sciences* 38, no. 7 (September 2016): 1–28; Megan Raby, "Ark and Archive: Making a Place for Long-Term Research on Barro Colorado Island, Panama," *Isis; an International Review Devoted to the History of Science and Its Cultural Influences* 106, no. 4 (2015): 798–824.

[150] Henke and Gieryn characterize four waves of thought about whether and how place matters in the sciences, See, Christopher Henke and Thomas F. Gieryn, "Sites of Scientific Practice: The Enduring Importance of Place," in *The Handbook of Science and Technology Studies*, ed. Edward J. Hackett et al., 3rd edition (Cambridge, MA: MIT Press, 2008), 353–76; Robert E. Kohler, "Place and Practice in Field Biology," *History of Science* 40, no. 2 (2002): 189–210; Kohler, *Landscapes and Labscapes*.

[151] White, *The Organic Machine*; Murphy, *Sick Building Syndrome and the Problem of Uncertainty*; Nash, *Inescapable Ecologies*; Parr, *Sensing Changes*.

A second prong of the literature on the places of scientific inquiry focuses on how research sites themselves have shaped research programs.[152] Scholarship on field sites and experimental stations fit within this tradition, with historians emphasizing the power of place in the development of specific theories and questions.[153] In the history of ecology, historians have used the material reality of particular places to explain associated concepts of nature.[154] For example, the shifting sands of the Indiana Dunes likely pushed Cowles to theorize about succession, since this dynamic landscape-in-the-making made change-over-time very visible. As historians have documented, ecologists in more stable landscapes were less likely to study vegetation dynamics, and instead examined issues more pertinent to those landscapes, such as the effects of rainfall on desert communities.[155] In this analysis, the material reality of field sites shapes scientific facts or influences scientific questions, and may explain why certain ideas might not travel to other spaces: in landscapes with different characteristics, the conclusions might not seem as applicable.[156]

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[152] For general works on the role of specific locations in the production of knowledge, see Jan Golinski, "The Place of Production," in *Making Natural Knowledge: Constructivism and the History of Science* (Chicago: The University of Chicago Press, 1998), 79–102; Jon Agar and Crosbie Smith, eds., *Making Space for Science - Territorial Themes in the Shaping of Knowledge* (New York: Palgrave Macmillan, 1998); Kohler, "Place and Practice in Field Biology"; David N. Livingstone, *Putting Science in Its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2010); Gregg Mitman, Michelle Murphy, and Christopher Sellers, eds., "Landscapes of Exposure: Knowledge and Illness in Modern Environments," *Osiris* 19 (2004): 1–288; Finnegan, "The Spatial Turn"; Sharon E. Kingsland, "The Role of Place in the History of Ecology," in *The Ecology of Place: Contributions of Place-Based Ecological Research to Ecological Understanding*, ed. Ian Billick and Mary V. Price (Chicago: The University of Chicago Press, 2010), 15–39. Thomas Gieryn has introduced the concept of 'truth-spot" to describe places that lend credibility to beliefs and claims about the natural and social reality. See, Thomas F. Gieryn, *Truth-Spots: How Places Make People Believe* (Chicago: The University of Chicago Press, 2018).

[153] Raby, "Ark and Archive"; Muka, "The Right Tool and the Right Place for the Job."

[154] Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1880-1900*; Cittadino, "A 'Marvelous Cosmopolitan Preserve': The Dunes, Chicago, and the Dynamic Ecology of Henry Cowles"; Daniel W. Schneider, "Local Knowledge, Environmental Politics, and the Founding of Ecology in the United States: Stephen Forbes and 'The Lake as a Microcosm' (1987)," *Isis* 91, no. 4 (2000): 681–705; Kingsland, "The Role of Place in the History of Ecology"; Raf De Bont and Jens Lachmund, eds., *Spatializing the History of Ecology: Sites, Journeys, Mappings* (New York: Routledge, 2017).

[155] Kingsland, "The Role of Place in the History of Ecology."

[156] Daniel P. Todes, "Darwin's Malthusian Metaphor and Russian Evolutionary Thought, 1859-1917," *Isis* 78, no. 4 (December 1, 1987): 537–51.

While scientists interpret pollen results in laboratories, this chapter has shown that pollen analysis is primarily a field science, with many of the situated qualities of the field sciences noted by other historians.[157] Yet, because pollen workers were accessing the past through indirect methods, the material reality of the natural world was mediated by proxy evidence, where a whole host of factors could play a role in shaping the data. As I'll argue in chapter 4, the complicated mediation led to a particular interpretation of climate among pollen workers. For now though, I hope to have shown that questions about the proper ways to interpret proxy evidence were at the heart of paleoecology in the first half of the twentieth century, and that generating knowledge by proxy was quite different than other ways of knowing in ecology.

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[157] Kohler did not discuss paleoecology in *Landscapes and Labscapes* because it was "more exclusive of the field" and did not illustrate the theme of cultural interaction between lab and field as strongly. See, Kohler, *Landscapes and Labscapes*, 20.

Chapter 2 – Dust Bowl Advocacy:
Prediction, Prophecy, and the Direction of History

While paleoecologists focused on their methods, they were not blind to the environmental disasters taking place around them. These problems, this chapter contends, encouraged paleoecologists to put aside their concerns about their methods and claim that their knowledge of long-term environmental change offered the broad perspective necessary to interpret the disaster at hand and to forecast likely outcomes. For these forecasts, paleoecologists did not need carefully interpreted data that closely cohered to past conditions, which they required when they focused on method, as the last chapter describes. Instead, the "broad knowledge and deep understanding" of shifts in both the environment and human societies that they had obtained as they sought to interpret paleoecological data became knowledge that they could apply to environmental problems.[158]

Paleoecologists first claimed this special perspective during the Dust Bowl of the 1930s. During the 1930s, the Great Plains, where many paleoecologists were working, experienced a severe drought. The resulting dust clouds blew as far as Chicago and New York City. These so-called "black blizzards" severely damaged the region's agriculture, forcing over 3 million people to abandon their farms and migrate west.[159]

Paleoecologists like Sears and Clements soon became involved in the disaster, offering forecasts about the likely outcome of desertification if humans did not change their actions. They both worked for government agencies applying their visions, and the popular press often discussed the importance of their forecasts. For example, as one review of Sears' 1935 popular book on desertification, *Deserts on the March,* wrote, "prophets of doom are rarely popular, but

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[158] "[Review of] Deserts on the March," *Scientific Book Club Review* 6, no. 11 (November 1935): 3.
[159] Worster, *Dust Bowl*.

in times like these they deserve an audience, especially if they couple with their gloomy pictures an adequate and appropriate suggestion concerning the way to escape the doom. Mr. Sears has accepted the rôle with dignity, restraint, and keen judgment."[160]

In this review, we see common claim, promoted by paleoecologists like Sears and Clements and in reviews of their work: that ecology could be a future-oriented science, and ecologists could be prognosticators of the future. This understanding of ecology calls into question historians' claims that early ecology was a descriptive science primarily concerned with vegetation dynamics. In the standard view, ecology was not future-oriented.[161] The historiography also only describes a brief period in the early twentieth century when ecology focused on issues affecting human livelihoods.[162] In contrast, I argue that several environmental crises led to various discussions of the rise and fall of human populations, and their relation to environmental change. The Dust Bowl of the 1930s was the first of these moments, with ecologists expressing a desire to use the past to shape the future.

Engaging with the likely course of desertification encouraged these ecologists to set aside questions about pollen as data in order to focus on larger questions about the direction of historical change common in this period. After the First World War, leading thinkers like Oswald Spengler, A.J. Toynbee, V. Gordon Childe, Lewis Mumford, and H.G. Wells began "theoretical inquiry into the meaning of history as understood in terms of patterns of historical change such

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[160] "[Review of] Deserts on the March," 2.

[161] As Robert McIntosh explains, ecologists themselves helped to promote this vision of their field: in the 1970s, as community ecology developed, which paid greater attention to quantification and theory, ecologists asserted or intimated, "that what had gone on before was descriptive, old-fashioned, immature, static, soft, atheoretical, and done by natural historians who did not know what science is all about." See, McIntosh, *The Background of Ecology: Concept and Theory*, 245–46.

[162] Kingsland, *The Evolution of American Ecology, 1890-2000,* 129–55; Eugene Cittadino, "The Failed Promise of Human Ecology," in *Science and Nature: Essays in the History of the Environmental Sciences*, ed. Michael Shortland (Oxford: British Society for the History of Science, 1993), 251–83.

as cycles, flux, providence, and progress."[163] Ecologists modified this line of inquiry, asking if there were any regularities in the human and ecological past. Their answers provided the basis for ecologists' forecasts, but, as I argue, their different answers to this question about the nature and direction of historical change resulted in different interpretations of whether paleoecologists could predict or prophesize. Two views dominated. One vision – associated with German cultural pessimist Oswald Spengler, geographer Ellsworth Huntington, and ecologist Frederic E. Clements – stressed the cyclical nature of history. The regularities of history would allow paleoecologists to make predictions, quantified forecasts indicating the likely outcome of climatic and human-caused environmental shifts. The opposing vision, emphasized by H.G. Wells and Sears, described a more linear history. History was less regular on this view, but, since the direction was known, ecologists could still use the past to determine the future. In this case, their forecasts would be more prophetic than predictive because they aimed to be broad descriptions of possible change which aimed to encourage individuals and governments to intervene in order to halt those changes. This chapter follows these conflicting visions as ecologists debated possible solutions to desertification given their different interpretations of the past.

Strengthening the Links between Human and Ecological History

Paleoecologists' ideas about the direction of history stemmed from their engagement with the environmental influence idea discussed in chapter 1. The once fashionable view of environmental determinism, especially the idea that the environment influenced the rise and fall of civilizations, with so called "primitive cultures" understood as more dependent on climate and vegetation, led ecologists to engage with archeology and broader discussions about the direction

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[163] Gilbert LaFreniere, *The Decline of Nature: Environmental History and the Western Worldview* (Bethesda, MD: Academica Press, 2007), 23.

of history.[164] Both Clements and Sears expressed these deterministic views at various points. Clements, for example, maintained, "primitive man must have been particularly dependent upon climate, vegetation, and animal life"[165] and Sears posited that climatic change could explain migrations among Native Americans who were less independent of their "immediate environment than those of more civilized people" and likely followed the climatic conditions that favored them.[166]

Yale geographer Ellsworth Huntington, who was influenced by European human geography, advanced a particularly strong form of climate determinism in which climatic conditions influenced the health, efficiency, and even the mental processes, of people.[167] He first reached these conclusions during field work in Central Asia in 1903 and 1905-1906, where he used evidence of past vegetation alongside geomorphic and cultural evidence to make inferences about the climatic factors that led to a civilization's decline. While in Asia, he had noted recently dead poplar trees, trees which always grew close to water but were now far from shore, and old tamarisk mounds, both of which indicated desertification. Huntington supported this claim by drawing on evidence from abandoned canal systems as well as oral histories, all of which suggested declining water levels. The data led him to claim that the region's drying climate had negatively impacted human inhabitants.[168] In subsequent work, Huntington applied these insights to other civilizations, adding evidence from the tree rings of giant sequoias to sunspot and

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[164] Von Post, "The Prospect for Pollen Analysis in the Study of the Earth's Climatic History," 217.
[165] Clements, *Plant Succession: An Analysis of the Development of Vegetation*, 319.
[166] Sears, "The Archaeology of Environment in Eastern North America," 612.
[167] Ellsworth Huntington, *Civilization and Climate* (New Haven, CT: Yale University Press, 1915); McGregor, "Huntington and Lovelock: Climatic Determinism in the 20th Century"; Fleming, "The Climatic Determinism of Ellsworth Huntington."
[168] Ellsworth Huntington, *The Pulse of Asia* (Boston: Houghton, Mifflin, 1907).

glaciation theories to strengthen the connection between climatic history and the rise and fall of civilizations.[169]

While not adopting the strict determinism of Huntington, a number of ecologists were quite struck by the connections he made between vegetation, climate and populations. Clements, who met Huntington in Arizona in the 1910s, regularly cited his work. The connection with Sears is less direct, but Sears undertook studies along the lines suggested by Huntington. For example, he used pollen analysis to trace why the Hopewell tradition, a maize-growing culture that had flourished along waterways in the Northeast and Midwest from 200 BCE to 500 CE, had thrived east of the present-day corn-belt. Sears posited that the culture had prospered in the east because the climatic conditions in that area had once favored maize agriculture. Yet the climate had shifted, forcing migrations and leading to decline (see Figure 8).[170]

Huntington, Sears, and Clements' moves to examine archeology in concert with the environment was relatively novel in ecology. Huntington's experience in the Ecological Society of America (ESA) helps to capture this novelty. Huntington was initially quite excited about ecology because he thought it could provide a grand synthesis of history, geography and biology. In the 1910s, he helped form the Ecological Society of America (ESA). He actively recruited geographers to join the society in order to advance his broad program of human ecology, which also encompassed public health, race, and culture. By the mid-1920s, as historians Sharon Kingsland and Eugene Cittadino explain, Huntington's vision of an expansive ecology had lost favor and Huntington became disenchanted with the field. Ecology had not proved to be the

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[169] Ellsworth Huntington, "The Secret of the Big Trees," *Harper's Magazine*, July 1912; Ellsworth Huntington, "Temperature and the Fate of Nations," *Harper's Magazine*, August 1928; Ellsworth Huntington, "The Solar Hypothesis of Climatic Changes," *Geological Society of America Bulletin* 25, no. 1 (1914): 477–590; Ellsworth Huntington, *Earth and Sun: An Hypothesis of Weather and Sunspots* (New Haven, CT: Yale University Press, 1923); Huntington, *Civilization and Climate*.

[170] Paul B. Sears, "Recent Climate and Vegetation a Factor in the Mound-Building Cultures?," *Science*, New Series, 73, no. 1902 (June 12, 1931): 641.

inclusive discipline Huntington envisioned; instead scientists studied aspects of human ecology, particularly urban ecology, in sociology departments without much regard to biological influences.[171]

Yet connections between the study of humans and ecology did not end with the demise of Huntington's general program: the historical ecology of Clements and Sears included humans and explored the relationship between humans, vegetation, and climate. In *Plant Succession,* Clements insisted that paleoecology was a "field in which the interrelations of climate, topography, and vegetation, animals and man play the paramount role."[172] He remained interested in questions of human adaptation to a fast-changing land.[173] Sears was also interested in these questions, as he hoped to understand how drivers of change: as one reviewer of his second book wrote, ecology was traditionally defined as the "study of the relations of plants and animals to their environment and to each other," but Sears and other ecologists were now incorporating human relationships into ecology.[174]

As ecology answered questions about human populations in light of the observation that changing climate and vegetation affected human livelihoods, ecologists did more than engage with archeology: they explicitly began to link ecological ideas with various theories of historical change related to the rise and fall of civilizations. After the First World War, which had been represented as a war "to save civilization," influential thinkers such as Oswald Spengler, A.J. Toynbee, V. Gordon Childe, Lewis Mumford, and H.G. Wells, all attempted to understand

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[171] Kingsland, *The Evolution of American Ecology, 1890-2000,* 131–41; Cittadino, "The Failed Promise of Human Ecology," 254–62.
[172] Clements, *Plant Succession: An Analysis of the Development of Vegetation*, 280.
[173] Kingsland, *The Evolution of American Ecology, 1890-2000,* 131.
[174] "Ecological Method Applied to Problems of Human Life."

whether civilizations rose and fell with any discernable pattern.[175] The larger project sought to explain the nature and direction of historical change, especially whether history was chaotic, regular or cyclical. As the Dust Bowl hit, claimed that these theories of history could ground their forecasts.[176]

The main theories of history that ecologists adopted were advanced by German philosopher of history Oswald Spengler and English writer H.G. Wells. In *The Decline of the West* (1918-1922), Spengler rejected the idea of a linear, progressive model of history. Instead, he thought that civilization was trapped in a process of deterioration and inevitable collapse. New civilizations would rise, but they too would follow a path of exhaustion, decline, and replacement. Modern science and technology would not arrest this development. As Spengler put it, "each culture has its own new possibilities of self-expression, which arise, ripen, decay, and never return."[177] Spengler understood the First World War as part of this cycle, it was not "a momentary constellation of casual facts due to national sentiments, personal influences, or economic tendencies… but the type of historical change of phase occurring within a great historical organism of definable compass at the point preordained for it hundreds of years ago."[178] Spengler's views echoed Clements' organicism, discussed in the last chapter. In *Plant Succession*, Clements had argued "as an organism the formation arises, grows, matures and

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[175] Felipe Fernandez-Armesto, *Civilizations: Culture, Ambition, and the Transformation of Nature* (Simon and Schuster, 2001).

[176] In many ways, these ecologists were using the patterns they found in nature, not just those found in history, to make claims about moral and social justice. For a wide-ranging history of these kinds of appeal to moral authority, see Lorraine Daston and Fernando Vidal, eds., *The Moral Authority of Nature* (Chicago: The University of Chicago Press, 2003).

[177] Oswald Spengler, *The Decline of the West*, trans. C.T. Atkinson (New York: Knopf, 1959), 22. The first English translation appeared in 1926, when Atkinson translated a new and revised edition. The publisher also released new English print runs in 1932 and 1939.

[178] Spengler, 46–47.

dies."[179] For both, the organicist metaphor dominated their interpretation of either vegetation formations or civilizations, leading to a unidirectional view of the direction of history.

Spengler advanced his position using the method that ecologists employed: analogies with the past. In Spengler's case, he made inferences about the direction of history by comparing the present to three other civilizations. From these analogies, Spengler made predictions about Western countries over the next 200 years. These organicist ideas about decline and cyclical patterns of history were fairly influential, even in the United States where "Spenglerism spurted from the pens of countless disciples."[180]

While there is little direct evidence that paleoecologists adopted a Spenglerian point of view when they interpreted the Dust Bowl, it is clear that some paleoecologists, particularly Edward Smith Deevey, were eager to apply Spengler's point of view to ecology by the 1940s. Deevey, who had shown the relevance of past climates to modern plant distributions and had applied pollen analysis to the archeology of Mexico, thought that Spengler's ideas about cycles offered the possibility that ecological and historical phenomena were ordered in a way that could yield predictions. Given the regularity of history on this view, Deevey thought that he could create a universal science, which he called "general ecology." In a popular article entitled "Pollen Analysis and History," published in 1944, Deevey advanced the idea, positing that Spenglerism and pollen analysis offered a way of integrating the sciences and the humanities. Devey himself would broker this merger, as he wrote:

> I shall rest when I have fused, at least to my own satisfaction, the broadest of the natural sciences, ecology, with the broadest of the humanities, history... History has long been thought to deal with the special case, the irrevocable past – 'we learn from history only

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[179] Clements, *Plant Succession: An Analysis of the Development of Vegetation*, 3.
[180] "Patterns in Chaos," *Time* 12, no. 24 (December 10, 1928): 63.

> that we learn nothing from history' – whereas science has to do with the general case, the predictable future. But great historians, like Spengler, have exercised their powers in generalizing history, and have shown that historical phenomena are as orderly in their way as scientific. Civilizations grow, flourish, and decay according to the same curve that ecology discovers in the growth of organisms and populations.[181]

For Deevey, the regularity of history under a Spenglerian point-of-view provided a framework for the sciences to come together with history, in order to create a truly future-oriented general ecology.

In many ways, this was the program that Clements had already adopted when he explained that ecology and human history were influenced by sunspot cycles. Between 1909 and the 1930s, Huntington and astronomer Andrew Ellicott Douglass had developed the sunspot theory to explain climate. They posited that the alternation of dry and rainy climatic phases could be attributed to fluctuations of the sun, visible in dark spots on the sun, which affected the energy available on earth and thus its climate. Douglass supported these claims using the nascent science of dendrochronology, the study of tree rings, with which he demonstrated that the climate's periodicity mirrored that of the sun over geological time periods.[182] During the 1920s and 1930s, scientists explained everything from the lifecycles of animals to parasites, agriculture and financial panics, as well as solar, lunar and metrological in terms of cycles, especially sunspot cycles. For example, in 1922 and 1928, Clements participated in colloquia organized by paleontologist John C. Merriam. The gatherings considered the application of cycle theory in geography, economy, paleontology, and archeology, with the larger aim of evaluating whether

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[181] Edward S. Deevey, "Pollen Analysis and History," *American Scientist* 32, no. 1 (1944): 39.

[182] Donald J. McGraw, "Andrew Ellicott Douglass and the Big Trees," *American Scientist* 88, no. 5 (October 2000): 440–47.

cycle theories could aid in scientific prediction in these disciplines. A number of the presentations – including those of Huntington, economist Henry L. Moore, and botanist Daniel T. MacDougal – used the sunspot theory to explain the periodicity of various phenomena and promote prediction.[183] The 1931 Matamek Conference on Biological Cycles also reflects these aims. The Conference, which was funded by American businessman Copley Armory in the hopes that scientific knowledge of population cycles in nature might be applied to business cycles, summarized the wide-ranging literature on cycles and their applications to the future.[184]

Clements' work in the conference proceedings indicates that, as early as 1923, he was already thinking of using the power of cycles to predict. In Clements' introduction to the 1923 volume that followed the first of Merriam's colloquia, Clements wrote a definition of the scientific term "cycle" arguing that scientists use the word to denote "a recurrence of different phases… which are often susceptible of exact measurement."[185] He went on to say that there were many types of cycles, including "physical, biological, and demial, the last term denoting those that appear in the social processes of human communities." He gave several examples of each type of cycle, writing that the "chief demial cycles are those of production, transport, prices, wages, employment, etc., which find their expression in such phases as good times and hard times, progress and reaction, war and peace."[186] Despite theories posting many different types of cycles, Clements thought that the sun-spot cycle and its relation to terrestrial climate and weather cycles was the best proven: he said "there can be no question of the existence of climatic cycles,"

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[183] Christophe Masutti, "Frederic Clements, Climatology, and Conservation in the 1930s," *Historical Studies in the Physical and Biological Sciences* 37, no. 1 (September 2006): 35.
[184] Ellsworth Huntington, "The Matamek Conference on Biological Cycles, 1931," *Science* 74, no. 1914 (1931): 229–35.
[185] Frederic E. Clements, "Nature of the Problem of the Cycle," *The Geographical Review* XIII, no. 4 Special Supplement (October 1923): 657.
[186] Clements, 658.

in large part because of Huntington and Douglass' work.[187] The last paragraphs of his introductory remarks discussed the "enormous advantage derived from the method of cycles is that of prediction in the broad sense of the term."[188] He then suggested that various predictions might be made, and the ways that, by modifying agricultural practices in years that the sun-spot cycles predicted would be dry, he could "practically eliminate the disastrous effects of drought."[189] Clements would repeat similar phrases during the Dust Bowl.

His contribution after the second of Merriam's colloquia, entitled "Climatic Cycles and Changes of Vegetation" and published in 1929, made the connections between prediction in ecology and sunspots even clearer. Clements began by arguing that

> ecology deals constantly with cycles and with climatic cycles in particular. The sun-spot cycle is taken as the typical example of cycles in general and its variability in duration, intensity and phase is thought to be sufficient warrant for using the term to apply to a wide range of continuous recurrences.[190]

After discussing sunspot maxima and minima in more detail, Clements claimed that "the initial interest of the ecologist in cycles was an outcome of the study of succession, itself a cycle, but it was much stimulated by the investigations of Douglass and Huntington." Clements cited their 1909 and 1914 papers for strengthening the connections between succession and sunspot cycles, claiming that this work "lent support to the working hypothesis that the sun-spot cycle might be employed for anticipating changes in rainfall and hence in vegetation."[191]

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[187] Clements, 658.
[188] Clements, 659.
[189] Clements, "Climatic Cycles and Changes of Vegetation," 659.
[190] Clements, 64.
[191] Clements, 65. See, also, A.E. Douglass, "Weather Cycles in the Growth of Big Trees," *Monthly Weather Review*, June 1909, 225–37; A.E. Douglass, "A Method of Estimating Rainfall by Growth of Tree," in *The Climatic Factor as Illustrated in Arid America*, ed. Ellsworth Huntington, Carnegie Institution of Washington, Publication No. 192 (Washington, D.C.: Carnegie Institution of Washington, 1914), 139–56; Ellsworth Huntington, *The Climatic Factor*

Clements also made connections between sunspots cycles and paleoecology, claiming that "the application of the methods of the climatic and biotic cycle to the past has led to the point of view embodied in paleo-ecology."[192] He then talked about the ways in which the formation and the climatic climax "have supplied the guiding principles of paleo-ecology and contain the promise of a changed outlook upon paleo-climatology" because the regularity of cycles would allow scientists to both reconstruct the past climates and predict future ones.[193]

Given this belief in sunspots and climate cycles, Clements produced his own graphs to show the correlation of sunspots and rainfall when he discussed the causes of the Dust Bowl (see Figure 9).[194] For popular audiences during the Dust Bowl, he also linked sunspots with human populations, arguing that human migrations were heavily based on these cycles.[195] He supported these claims by looking to one of the oldest sources of human history, the Bible, drawing on the series of lean and fat years described in the Book of Genesis to show that these cycles had influenced people for thousands of years. He found similar periods of abundance and want in more recent history, as crops failed with regular periods of drought and rain. Even the growth, maturity and decay of plant formations was based on these cycles; in the words of historian Christophe Masutti, Clements, "insisted on embedding succession within a history determined by the theoretical cycles that drove climatic development."[196] For Clements, once prediction became the imperative during the Dust Bowl, sunspot cycles offered a shape to history – natural and human alike – from which he could make his predictions.

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as Illustrated in Arid America, Carnegie Institution of Washington, Publication No. 192 (Washington, D.C.: Carnegie Institution of Washington, 1914).

[192] Clements, "Climatic Cycles and Changes of Vegetation," 69.

[193] Clements, 69.

[194] Frederic E. Clements, "Drouth Periods and Climatic Cycles," *Ecology* 2, no. 3 (July 1921): 181–88. *Plant Succession* has thirty-six references to sunspots.

[195] Frederic E. Clements, "Climatic Cycles and Human Populations in the Great Plains," *The Scientific Monthly* 47, no. 3 (September 1, 1938): 197.

[196] Masutti, "Frederic Clements, Climatology, and Conservation in the 1930s," 32.

While Deevey spoke with excitement about the ways that Spengler was able to force "wondrous patterns on the chaos of history" and Clements insisted that the cyclical nature of sunspots formed the basis of history, others were not as prepared to see the past as a series of relatively regular cycles.[197] English novelist and political commentator H.G. Wells, who also collaborated on a biology textbook with large sections on ecology, developed one of the most influential competing theories of history. For Wells, history was a "race between education and catastrophe," but the shape of history was much more linear and progressive, rather than cyclical, because science was able to prevent decline.[198] Wells advanced this progressive vision of history in his best-selling historical works, *The Outline of History* (1920) and *A Short History of the World* (1922).

There were important differences between Spengler's and Wells' approaches to history. Spengler denied science's capacity to provide for humans, and instead saw decline as inevitable. Wells believed that science would solve humanities' problems. These differences meant that, for Spengler history was a series of predictable cycles, but for Wells it was a progressive march forward. The contrast between these approaches to history was so great that Wells and Spengler were asked to debate the "philosophy of history" in front of a Cambridge, MA audience in 1926.[199] While the lecture never came to fruition, comparisons between Spenglerian pessimism and Wellsian optimism about human will and intellect were often discussed, with both of their ideas permeating popular discourse.

Sears was particularly struck by Wells' philosophy of history. Like Deevey, Sears

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[197] "Patterns in Chaos."

[198] H.G. Wells, *The Outline of History: Being a Plain History of Life and Mankind*, 3rd edition (New York: MacMillan, 1921), chap. 41; William T. Ross, *H.G. Wells's World Reborn: The Outline of History and Its Companions* (London: Associated University Presses, 2002); H.G. Wells, *Text-Book of Biology* (London: W.B. Clive & Co., 1893).

[199] John Farrenkopf, *Prophet of Decline: Spengler on World History and Politics* (Baton Rouge, LA: Louisiana State University Press, 2001), 161.

connected ecology with theories of history, especially when he spoke to popular audiences. On radio broadcasts, Sears often reflected broadly about history, ecology and the need for the technical expertise of ecologists, invoking Wells as he made these claims. Before Wells, Sears said, history was "a chronicle of the deeds of kings and warriors quite satisfying until you look into it." But in the hands of Wells, history was "made by many more people" and included environmental factors, which, Sears thought, carved out a place for scientists, particularly ecologists, in public affairs. After all, from Wells' perspective scientists would be the ones to ensure that civilizations would flourish. For Sears, this meant that "biology, history, and human ecology" should be at the core of any educational enterprise; these fields would teach the next generation how to use the past to understand the future.[200] As Sears reflected on his understanding of ecology in the 1930s in a *Science* article published in 1954, "ecology is concerned not only with the present but with the past… since the present and future of any community are expressions of its past, we may expect the study of archeology and history to have a practical bearing on the critical question of man's future."[201] For Sears, the world was complex and required ecological expertise because ecologists could properly interpret the past and plan for the future.

Sears was also struck by Wells' imperative to prophesize. In 1902, Wells claimed that "prophecy has always been inseparably associated with the idea of scientific research."[202] According to Wells, the scientific process analyzed facts, resulting in ordered knowledge that yielded confident, prophetic forecasts. Essentially, once "one assimilated the broad conceptions

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[200] "Looking into the Future"; Paul B. Sears, "Utility, Science, and Education," 1938; Paul B. Sears, "A Blind Spot in the Curricula," 1937, Box 124, Folder 123, Coll MS663, Manuscripts and Archives, Yale University.

[201] Paul B. Sears, "Human Ecology: A Problem in Synthesis," *Science*, New Series, 120, no. 3128 (December 10, 1954): 960.

[202] H.G. Wells, *The Discovery of the Future* (London: T. Fisher Unwin, 1902), 53.

of science…. The future was just as fixed and determinate, just as possible a matter of knowledge as the past.”[203]

Sears drew heavily on Wells’ idea of science as prophecy. Sears had been reading Wells’ stories from the time he was a “small lad” and Wells remained an inspiration as Sears contemplated the purpose of science later in his career.[204] In particular, Sears came to repeat Wells’ view that modern science could bridge the gap between past, present, and future during the Dust Bowl of the 1930s.[205] Sears ended *Deserts on the March* saying that ecology “has been called by H.G. Wells, who cannot be accused of a failure to anticipate events, the science of prophecy.”[206]

In sum, in the first half of the twentieth century, historians and popular writers had advanced a number of different theories of history. As the Dust Bowl struck, paleoecologists actively engaged with these theories of history, not in an unconscious and passive way, as Donald Worster has claimed.[207] Instead, Clements, and Sears actively engaged philosophies of history in order to carve out a place for paleo-perspectives and forecasts on the unfolding disaster. As my next section shows, these ecologists interpreted the problem based on their competing theories of history, with Clements espousing prediction and Sears promoting prophecy.

The Historical Character of the Dust Bowl

During the Dust Bowl, ecologists like Sears and Clements began to use insights from paleoecology and theories of history in order to understand the unfolding disaster. Clements,

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[203] “The Discovery of the Future,” *The Times*, January 12, 1902, 12.
[204] “Looking into the Future”; Sears, “Utility, Science, and Education.”
[205] Sears, “The Goals of Paleoecological Reconstruction,” 4.
[206] Paul B. Sears, *Deserts on the March*, 2nd ed., (London: Routledge and Kegan Paul, [1935], 1949): 177
[207] Worster, *Nature’s Economy: A History of Ecological Ideas*, 218–19.

who was working alongside the US Soil Conservation Service, predicted the likely outcome of the drought by drawing on his cyclical understanding of history: sunspots were responsible for the arid period, but the drought had been exacerbated by land-use practices which had destroyed the soil and vegetation. As Clements wrote, "of all the forces that act upon plant cover to destroy its protective power, those released by man are by far the most potent."[208] His solutions aimed to restore damaged land and to conserve unscathed landscapes, since he thought that climax communities were the most stable. Many of these climax communities comprised perennials able to repopulate the land after drought, whereas crops were usually annuals, easily "subject to the whims of a hot summer."[209]

Given these insights, Clements encouraged landowners to return the native vegetation, allowing it to progress towards a stable complex without being threatened by grazing or deep plowing. Yet, to maintain the agricultural productivity of the region, he recognized that it was not possible to fully restore the prairie. Instead, he thought that ecologists could classify the land to determine which areas were appropriate for grazing, crops, or forest. He claimed that ecologists were especially needed for this task, noting that there was "nothing mysterious about the way in which plants act to hold particles of soil in place, but the combination of different plant covers, soils, climatic conditions and human disturbances are innumerable."[210] The complexity of the matter meant that the "task of the ecologist as a student of environment is chiefly to analyze vegetation in the process of protection and recovery, in order to obtain a properly balanced control for the future."[211]

The future-oriented nature of ecology came from changing the land classification given

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[208] Clements, "Climatic Cycles and Human Populations in the Great Plains," 202.
[209] Clements, 198.
[210] Clements, 200.
[211] Clements, 200.

climatic forecasts. Knowing sunspots' cyclical pattern, which made "droughts recurrent and inevitable," Clements thought he could predict climatic conditions in much the same way that the Weather Bureau was able to make short-term predictions about the week's weather.[212] As he put it, agriculture would become a science of using moisture forecasts "anticipated year after year by means of long-range indexes" that relied on sunspot data.[213] From this information, farmers could appropriately adjust their crops, since state and federal agricultural experiment stations had already experimentally determined the ideal conditions for each crop: in wet years corn could be planted, while in dry years, winter wheat, which required less water, could be sown. Clements' program thus relied on the statistical likelihood of particular conditions to make recommendations about appropriate actions in response to the Dust Bowl, ones that relied heavily on theories about cycles.[214] He thought that these methods would be "adequate to eliminate" the major effects of drought.[215] In making this claim, Clements was arguing that ecology could be an applied science of prediction.

Clements even went so far as to test his predictions: in a draft paper co-authored with his wife, Edith, in the 1940s, the Clements' linked droughts to sunspot maxima. They also had predicted that once the number of sunspots began to drop, "good rains… and a precipitation above normal" would result. By following three years' worth of data as the Dust Bowl ended, the Clements' found that these predictions had been correct; there was a close relation between the monthly sunspot mean, and rainfall.[216] With this conclusion, Clements was even more convinced that ecology could be a predictive science.

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[212] F.E. Clements and E.S. Clements, "Climate, Climax and Conservation," n.d., 8, Box 29, Folder 20, Coll 01678, Edith S. and Frederic E. Clements Papers, University of Wyoming American Heritage Center.
[213] Clements, "Climatic Cycles and Human Populations in the Great Plains," 209.
[214] Tobey, *Saving the Prairies: The Life Cycle of the Founding School of American Plant Ecology, 1895-1955*, Chapter 7.
[215] Clements and Clements, "Climate, Climax and Conservation," 8.
[216] Clements and Clements, 7.

Sears also weighed in on the Dust Bowl, but he did so by drawing on Wellsian philosophy rather than an adherence to cycles. As the drought struck, Sears was working at the University of Oklahoma, which was in one of the hardest hit areas. The resulting dust clouds were so conspicuous that Sears remembered, "I could not, in honor, shut my eyes. When drouth [*sic*] and dust and economic disaster hit, the administration sent around to find out what scientists could do about it. And since the basic story was clear enough to any student of landscape, I offered to tell it, as my contribution."[217] His contribution was *Deserts on the March* (1935), a book that aimed to teach the general public about the growing menace of soil erosion as humans "upset the balance under which wind and water were beneficial agents."[218] This work translated the symbols, numbers, and charts from Sears' scientific pollen work into prose that evoked a sense of crisis, and explained the origins of the drought by drawing on historical cases of civilizations from around the world that had similarly degraded the soil. It also offered possible solutions, as Sears highlighted past practices that had replenished the soil. *Deserts on the March* also helped make a name for Sears, to the point that Donald Worster has called the volume "the most important popular ecological work" of the 1930s.[219] More importantly, *Deserts* serves as an example of ways that ecologists, particularly those who studied change over geologic timescales, convinced others that they had a broad, scientific basis for their claims.[220] As one review of *Deserts on the March* claimed, Sears is "generally recognized as one of America's foremost experts in the application of pollen analysis… In this book he demonstrates the fact that a

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[217] Sears to Patten Jr.
[218] Sears, *Deserts on the March*, 1935, 3.
[219] Worster, *Dust Bowl*, 25th Anniversary Edition, 200.
[220] As historian Peder Anker has noted, as "ecology embraced an enlarged order of nature, knowledge, and society," ecologists fashioned themselves as "new masters and interpreters of this world." See, Peder Anker, *Imperial Ecology: Environment Order in the British Empire, 1895-1945* (Cambridge, MA: Harvard University Press, 2001), 237.

specialist in some small area of science may also have broad knowledge and deep understanding of related fields."[221]

Sears' interpretation of the Dust Bowl was colored by Wells' philosophy of history, which Sears continued to cite as an inspiration.[222] Sears' pollen analysis and witness tree work led him to believe that Americans had accelerated erosion processes: where it had once taken over 4000 years to erode a foot of soil, now only a decade was needed.[223] Sears thought that current erosion rates resulted from land-clearing and settlement practices, and compared the rapid erosion to the actions that had led to the decline of civilizations like the Maya and Romans. Yet the past was not full of examples of doom; Sears also described techniques that restored the soil, such as the Chinese application of night-soil which had ensured the productivity of many ancient dynasties. Sears used these historical examples as a guide for future management. As chair of Oklahoma's legislative advisory committee that oversaw the drafting of the state's first soil conservation laws, Sears recommended the immediate adoption of mandatory soil conservation methods similar to the ones most effective in the past.

Sears also used findings from paleoecology to suggest that, post-glaciation and pre-settlement, the prairies had been covered with grasses that had survived changes within the climate, including other dry periods. He therefore counselled farmers to plant wheat fields with cover crops and native grasses, which his paleoecological work had shown were more resilient to change. Yet, because the conditions in each county were slightly different, Sears also thought each community needed a resident ecologist, someone who was able to use science to provide

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[221] "[Review of] Deserts on the March," 3.

[222] "Looking into the Future"; Sears, "Utility, Science, and Education"; Paul B Sears, "The Black Blizzards," in *America in Crisis*, ed. Daniel Aaron (New York: Knopf, 1952), 287–302; Paul B. Sears, *Deserts on the March*, 3rd Edition, Revised (Norman, OK: University of Oklahoma Press, 1959).

[223] "Soil and Sociology," *Science Service* (Columbia Broadcasting System, October 13, 1936), Box 120,Folder 29, Coll MS663, Manuscripts and Archives, Yale University.

place-specific guidance for the future.[224] With Wellsian optimism that science and knowledge of the past would provide guidance for a prosperous future, Sears provided a popular, optimistic vision for how ecologists would restore the Great Plains.[225]

These two solutions to the Dust Bowl at first look quite similar: Clements and Sears both blamed human land-use practices for the severe wind-erosion that severely worsened the drought. Both advocated for the restoration of historical vegetation, which held the soil in place and helped it retain moisture. Both claimed that ecologists should guide restoration efforts, using local knowledge of the geologic past to achieve the most favorable outcome in different locations.

There were, however, differences between these two interpretations of the Dust Bowl, in part because the two disagreed about the nature of the historical record, resulting in different senses of the extent to which the future could be predicted. Clements believed in cycles, fitting the Dust Bowl within the regular periods of drought he observed. Sears, in contrast, bemoaned ecologists who were fixated on cycles because he thought they gave “the misleading idea of regularity – recurrence is a better word than cycle.”[226] Sears furthered this idea in the *Pollen Analysis Circular*, which he initiated during the Second World War to facilitate communication among pollen workers. In the 1943 circular, Edward R. Dewey, the founder of the Foundation

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[224] Sears’ more local ideas, which included a resident ecologist in every county were also part of his conservative outlook. He had been “born and raised in the tradition of conservatism,” so shied away from solutions that promoted big government or larger changes to the social order. Sears, quoted in Worster, 208. See also, Eugene Cittadino, “Paul Sears and the Plowshare Advisory Committee: ‘Subversive’ Ecologist Endorses Nuclear Excavation?,” *Historical Studies in the Natural Sciences* 45, no. 3 (June 2015): 399.

[225] In his review of *Deserts,* Arthur Tansley critiqued this advice. He agreed that ecological advice was vital to the proper utilization of the land, but questioned “who would take the ecologist’s advice about the utilization and treatment of land if he though it conflicted with his immediate interests.” See, A. G. T[ansley], “The Destructiveness of the Human Animal,” *Journal of Ecology* 24, no. 1 (1936): 296–97.

[226] Sears, “The Black Blizzards,” 289. Sears had been carefully reading about periodicity in biological phenomena; he was able to provide Ellsworth Huntington with a number of recommendations when asked for sources, including Charles Elton’s work on voles, mice and lemmings, and Raymond H. Wheeler’s work on weather cycles and their influence on human behavior. See, Paul B. Sears to Ellsworth Huntington, February 4, 1944, Series III, Box 93, Fol. 3883, Ellsworth Huntington Papers, MS 1, Archives and Special Collections, Yale University.

for the Study of Cycles and who would go on to write *Cycles: the Science of Prediction* (1947), had encouraged “every worker in pollen analysis” to keep in touch with the foundation and read its brochure on rhythmic behavior. Sears seems to have used his editorial power to add a note in response to Dewey’s request.[227] Sears wrote:

> Speaking of Cycles, the biologist, accustomed to working with material of great complexity, may feel more comfortable to use the expression ‘Recurrent Phenomena.’ This avoids the implication of regularity of interval and intensity which is often so troublesome before the facts are all in.[228]

Sears’ distaste for cycles meant that the future could not be predicted with the same statistical regularity that Clements promoted. At best, past vegetation and land-use practices could strongly inform ecological-minded planning, which would give a sense of where the climate and vegetation were heading. In Sears’ opinion this meant that ecology was best described as the science of prophecy.

Sears made these commitments to a non-cyclical prophetic science explicit when the science editor for *Literary Digest* wrote in 1936 asking “if you can say from your studies whether there seems to be any cyclical changes in climate and whether from that, it is possible to prognosticate.”[229] Sears responded that prognostication was possible, but not because there were cyclical changes. Instead, Sears described his pollen analysis work. His studies had traced climatic changes over the last 3,000 years alongside human land use practices.[230] The knowledge he had gained studying the deep past allowed him, an ecologist, to identify the “continuity,

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[227] Edward R. Dewey, “Foundation for the Study of Cycles,” ed. Paul B. Sears, *Pollen Analysis Circular* 3 (September 15, 1943): 1.

[228] Paul B. Sears, “Speaking of Cycles,” ed. Paul B. Sears, *Pollen Analysis Circular* 3 (September 15, 1943): 1.

[229] G. Edward Pendray to Paul B. Sears, July 21, 1936, Box 1, Folder 17, Coll MS455, Special Collections, University of Arizona.

[230] Paul B. Sears to G. Edward Pendray, July 26, 1936, Box 1, Folder 17, Coll MS455, Special Collections, University of Arizona.

integration, and destiny" of vegetation communities and human populations. This knowledge was "indispensable to management and control," which is why Sears advocated for a resident ecologist in any sizeable community: they too would be able to read the past and prophesize.[231]

Thus, while Sears was making very broad and coarse claims about the direction of the future with his prophecies, he thought that only scientific training grounded his prophecies. As he said during a 1937 radio broadcast, "prophecy is a dangerous business but like most dangerous pursuits it is tempting and exciting. It has often occurred to me that we need more of it, particularly from people who have their technical basis for what they say."[232] Given that the Dust Bowl was an ecological disaster, ecologists like him had the technical basis for prophecies. As he said, "the branch of science that deals most directly with the problem [of desertification] is ecology – the science of living things, plants, animals, and man, in relation to their environment. The ecologist sees these in perspective and in that sense can look ahead for the future."[233] These claims led the radio broadcaster to claim that Sears was a "scientific prophet."[234]

The respect Sears gained as a scientific prophet was not just because he "wail[ed] a jeremiad," even one based in scientific fact.[235] He also provided ways out of the gloomy future, in part because he did "not enjoy the role of the gloomy prophet."[236] For instance, towards the end of a radio broadcast which outlined Oklahoma's problems during the Dust Bowl, Sears spent some time looking "ahead into the Oklahoma of the future. In so doing," he said,

> I shall depart from the usual role of a prophet in one respect. A prophet generally talks about what will happen if things go on as they are at present. If I did this for the soil and

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[231] Sears, *Deserts on the March*, 1935, 226–27.
[232] "Looking into the Future."
[233] "Soil and Sociology."
[234] "Soil and Sociology."
[235] Donald Culross Peattie, "A Grass-Roots Pessimism," *New York Herald Tribune (1926-1962),* April 19, 1936.
[236] Paul B. Sears, "The Black Duskers," n.d., Box 122, Box 74, Coll. 663, Paul Bigelow Sears Papers, Yale University Special Collections.

> vegetation of Oklahoma, it would not be particularly cheerful reading. Being somewhat of an optimist, I should like to talk about what Oklahoma can be like if we really mean business.[237]

As reviews explained, getting down to business meant restoring the grasslands and forests. In this sense another review claimed that Sears "is no mere Jeremiah, prophesying only inescapable doom. Ways out can be found by serious application… [which will allow] human society… [to] achieve a stable culture pattern, comparable to the stability that slowly develops in organic communities in the subhuman world."[238]

Clements also believed that scientifically trained ecologists were the proper interpreters of change. In his view, these ecologists would decipher the drought within the larger cycles in which they were a part, which would allow them to restore the communities most-able to survive given the likely conditions. For Clements, this meant that ecology would be predictive because it could mathematically understand cycles within history, and ecologists could then use this periodicity to provide direction for the future. His claims, perhaps because they were more quantified and he only occasionally wrote for popular audience, spoke in a different affective register. I have found no instances where Clements regarded himself as prophet, or where his contemporary referred to him or his science as prophetic. Instead, his was a predictive science.

Sears and Clements' interpretations of the Dust Bowl and ecologists' predictive power thus indicate the ways in which various ideas about history shaped these interpretations and the limits of ecological forecasts. They were grappling with questions of whether the natural world,

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[237] "Looking into the Future."
[238] "Ecological Method Applied to Problems of Human Life," 9.

especially the climate, exhibited regular patterns that could be interpreted and predicted.[239] This question about climate and how to interpret it, especially in light of anthropogenic environmental change, also arose as paleoecologists interpreted extinction, as my next chapter will show. It shows how, as paleoecologists tried to understand what had caused the extinction of large mammals at the end of the last Ice Age, they too grappled with larger social questions, this time about human nature rather than the course of history. Contested ideas about whether humans were an inherently violent species, which paleoecologists built into the predictive models, called their forecasts into question, not for their gloomy outlook, but for their potentially racist conclusions. It thus shows that although the quantitative predictions of the sort promoted by Clements were becoming the imperative during the 1960s and 1970s, not all predictions viewed favorably.

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[239] There were even more theories of climate during this period: for example, the first professor of climatology in the United States, Robert DeCourcy Ward, stressed climatic constancy R.D. Ward, *Climate: Considered Especially in Relation to Man*, 2nd edition (New York: G.P. Putnam's Sons, 1915).

Chapter 3 – Questioning Overkill: Ways of Knowing the Past

In December 1960, the United States began construction of Titan II missile sites south of Tucson, Arizona. Once operational, the Strategic Air Command would be able to launch nine megaton thermonuclear warheads able to reach targets over 10,000 kilometers away in just thirty minutes. Residents of Tucson protested the construction of these missile silos, concerned that a nearby intercontinental ballistic missile (ICBM) facility would be a likely target of Cold War aggression, which would put the city at risk of hazardous “downwind fallout of a magnitude unknown in the bombing of Hiroshima.”[240] Their protests also reflected a larger concern about nuclear proliferation and humankind’s ability to destroy the earth many times over.

University of Arizona paleoecologist Paul S. Martin attended many of the protests against the Titan missile sites and enumerated his objections in the op-ed pages of local newspapers.[241] The self-described lapsed Quaker remembers discussing his scientific work with a lawyer who was also present at one of these protests. A few years earlier, Martin had used paleoecological techniques, including pollen analysis and radiocarbon dating, to demonstrate a synchronicity between the extinction of North America’s megafauna and the arrival of humans at the end of the Pleistocene, roughly 11,700 years ago.[242] After hearing about Martin’s work, the lawyer is said to have exclaimed “you study ‘overkill.’” Martin, who had “no objection to the shock value of words,” agreed that this was an apt description of his work.[243] At the time, major newspapers

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[240] Paul S. Martin and Cornelius Steelink, “The Titanizing of Tucson,” *Bulletin of the Atomic Scientists* 17, no. 4 (1961): 167–68.

[241] See, Paul S. Martin Papers, Series IV - Newspaper Clippings, MS442, Special Collections, University of Arizona.

[242] Paul S. Martin, “Pleistocene Ecology and Biogeography of North America,” in *Zoogeography*, ed. Carl L. Hubbs, The American Association for the Advancement of Science 51 (Baltimore, MD: Horn-Shafer Company, 1958), 375–420.

[243] Paul S. Martin to David Burney, January 11, 1988, Box 2, Folder 2, Coll MS442, Special Collections, University of Arizona Library.

were using “overkill” to describe nuclear weapons’ capacity to kill and destroy in excess of any strategic requirements.[244] Martin’s hypothesis, which also came to be known as the “blitzkrieg” theory of extinction because of the rapid pace at which hunters were thought to have wiped out the megafauna, drew on concerns about nuclear annihilation, and became a popular way to discuss the causes of the Pleistocene extinctions, with “‘American Blitzkrieg’ and ‘Slaughter of Mastodons Caused Their Extinction’” defining headlines in the popular press.[245]

Cold War concerns about nuclear annihilation, and the related concern about extinction, represented another moment when paleoecologists intervened in discussions about an environmental disaster at hand. Like Paul Sears and Frederic Clements, who had used their skills interpreting the past to project the future of the Great Plains during the Dust Bowl of the 1930s, In their studies of desertification and extinction, paleoecologists worked with proxies, pollen in the 1930s, and fossil dung and debris piles in Martin’s case. Paleoecologists used proxy evidence to understand past environments, and then applied their insights from the past to the future. In the 1970s, however, Martin took the overkill hypothesis a step further than the forecasts of the Dust Bowl: he began making predictive models. These mathematical models made hypothetico-deductive predictions about what scientists would find in the fossil record that would support the theory.[246]

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[244] Seymour Melman, a Columbia economics professor who taught industrial engineering, popularized the term “overkill” to describe the stockpiling of nuclear weapons in excess of any strategic advantage. As he wrote in a letter to *The New York Times* in 1964, “Isn’t 1,250 times overkill enough? Since the Soviets by similar calculation can overkill the United States only 145 times, are we to believe that any advantage exists here for either side?” See, Jennifer Bayot, “Seymour Melman, 86, Dies; Spurred Antiwar Movement,” *The New York Times*, December 18, 2004, sec. Obituaries. For more on the popularity of overkill in the 1960s, see Benjamin DeMott, “The Age of Overkill,” *The New York Times* (May 19, 1968): SM104.

[245] Krech III, *The Ecological Indian: Myth and History,* 29.

[246] I In this chapter I deal less with predictions about the future, but with predictions about the evidence that scientists would find in the fossil record. Martin was making explicit claims about the future in 2005 when he talked about Pleistocene rewilding, which aimed to reintroduce the descendants of the Pleistocene megafauna and their close ecological relatives. The final chapter in *Twilight of the Mammoths* is entitled “Resurrection: The Past is Future.” From the 1960s onwards, though, Martin was making implicit clams about the future: as Robert L. Kelly

As other historians have argued, these kinds of predictive models were becoming increasingly common in the earth and atmospheric sciences in the 1970s.[247] Before then, the geologic record was too poorly understood for these kind of quantitative predictions. For instance, although Clements had held out hope that the best-known cycle, the sun-spot cycle would allow predictions, he had to admit that "it is at present impossible to predict" the sun-spot maxima or minima, and that it was even more difficult to arrive at "definite prediction[s] of the year of maximum drought or wetness in any particular climate cycle."[248] By the 1970s, Martin thought that he had enough evidence to understand the interactions of the relevant factors that had led to the extinction of the megafauna.

Policymakers found Martin's theory convincing, so, in much the same way Dust Bowl ecological theories about succession had been used to advocate for land-use policies, in the 1990s policymakers began to suggest that overkill offered insights into who was capable of governing land. Once again, the past was being used to guide the future. In the overkill case, rather than suggesting that ecologists could manage the land, as Sears and Clements had done during the Dust Bowl, those who gained perspective from overkill suggested that Native Americans, who were the descendants of those who had wiped out the megafauna, should not be given the right to manage their lands.

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and Mary M. Prasciunas explain, many ecologists used the late-Pleistocene extinctions as examples of what humans did in the past and what they are capable of doing in the future. See, Robert L. Kelly and Mary M. Prasciunas, "Did the Ancestors of Native Americans Cause Animal Extinctions in Late-Pleistocene North America?," in *Native Americans and the Environment: Perspectives on the Ecological Indian*, ed. Michael E. Harkin and David Rich Lewis (Lincoln, NB: University of Nebraska Press, 2007), 95–96. On Pleistocene rewilding, see C. Josh Donlan et al., "Pleistocene Rewilding: An Optimistic Agenda for Twenty-First Century Conservation," *The American Naturalist* 168, no. 5 (November 2006): 660–81; Paul S. Martin, *Twilight of the Mammoths: Ice Age Extinctions and the Rewilding of America* (Berkeley, CA: University of California Press, 2005), 200–212. For Martin's first work on modelling, see Paul S. Martin, "The Discovery of America," *Science* 179, no. 4077 (1973): 969–74.

[247] Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970"; Oreskes, "From Scaling to Simulation: Changing Meanings and Ambitions of Models in Geology"; Oreskes, "Why Predict? Historical Perspectives on Prediction in Earth Science."

[248] Clements, "Nature of the Problem of the Cycle," 659.

In this chapter, I show how these debates about land management centered on what constituted appropriate scientific evidence as paleoecology moved from prophecy to prediction. While many found the new proxy techniques and models compelling, especially because they overcame some of the problems of working with proxies discussed in the first chapter, Martin was also building claims about human nature into his predictive models. Proxy evidence could provide evidence of past climates and vegetation, but it was not a useful technique for overcoming the large debates about human nature and the status of Native Americans that were implicated in the overkill theory. As I show, overkill came to be challenged on all sides, which provides useful insights into how far paleoecologists, in an era of prediction, could move beyond vegetation and climate to human concerns before they went beyond their evidence. I argue that anthropologists and archeologists, who worked with the macrofossil record, were concerned that Martin overstepped the evidence, thereby delegitimizing their field. I also argue that Native American activists were concerned about the authority granted to scientific theories, regardless of the paucity of their evidence. They were frustrated that scientific theories like Martin's held sway even though their oral traditions produced plausible explanations about the causes of the Pleistocene extinctions.

This episode about Pleistocene extinctions thus reveals several aspects of the history of forecasting and proxy work: first, it exposes the shift from prophecy towards hypothetico-deductive predictions that was taking place in the middle of the twentieth century. Second, it reveals how scientists could overcome some of the problems with proxies discussed in chapter 1, to generate authoritative conclusions about past climates. Third, it shows that predictions often moved into a grey area of uncertainty beyond the realm of what could be known through scientific evidence. As I show, these kinds of forecasts could then become the subject of discussions about whether scientists had the authority to predict.

Early Explanations for the Pleistocene Extinctions

The overkill theory responded to many explanations about extinction that had developed since the early-nineteenth-century when naturalists unearthed the bones of large and strange creatures with no apparent modern counterparts. Prior to the widespread acceptance of extinction in the nineteenth century, naturalists did not believe that these creatures were extinct, especially because an omniscient and benevolent God would not have allowed His perfectly ordered creation to vanish. Doubts about the impossibility of extinction gave way in the early-nineteenth-century due French comparative anatomist Georges Cuvier's evidence that fossil bones did not correspond to any living species.[249] With this evidence, leading naturalists stopped debating the possibility of extinction and instead focused on the causes.[250] In 1822, English theologian and geologist William Buckland invoked the biblical flood as cause, which he argued had swept away species that no longer existed.[251] Geologist and zoologist Louis Agassiz cited widespread glaciation and rapid freezing as the basis of these extinctions in 1866, writing of that a "sudden intense winter" that lasted for ages "fell upon our globe; it spread over the very countries where these tropical animals had their homes, and so suddenly did it come upon them that they were embalmed beneath masses of snow and ice."[252] Noted geologist Charles Lyell and evolutionist Charles Darwin preferred more gradualist explanations, with extinction resulting from slower

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[249] Georges Cuvier, *Essay on the Theory of the Earth*, trans. Robert Kerr (Edinburgh: William Blackwood, 1813), http://archive.org/details/theoryofeessayon00cuvirich.
[250] Mark V. Barrow Jr., *Nature's Ghosts: Confronting Extinction from the Age of Jefferson to the Age of Ecology* (Chicago: The University Of Chicago Press, 2009).
[251] Mott T. Greene, "Genesis and Geology Revisited: The Order of Nature and the Nature of Order in Nineteenth-Century Britain," in *When Science and Christianity Meet*, ed. David C. Lindberg and Ronald L. Numbers (Chicago: University of Chicago Press, 2003), 139–60.
[252] Louis Agassiz, *Geological Sketches* (Boston: Ticknor and Fields, 1866), 208.

climatic and ecological changes.[253] Lyell laid out his theories in the *Principles of Geology*, published in three volumes between 1830 and 1833, while Darwin presented his in the *Origin of Species,* published in 1859. These nineteenth-century naturalists helped set the theoretical and empirical foundations for twentieth-century accounts of extinction, including the Pleistocene extinctions.

The extinction of North American fauna was of particular interest in the eighteenth and nineteenth centuries; many early colonists wondered why North America's fauna were so small compared to those found elsewhere. Were the New World's fauna – and its people – inferior? The unearthing of giant bones suggested that the Western Hemisphere similarly had its giants, but where were they gone? Glaciation, which geologists thought had been particularly influential in carving out the North American landscape, offered one explanation.[254] Since naturalists thought that the extinction of North America's megafauna coincided with the cold period at the end of the last Ice Age, climatic explanations for the extinctions became the most popular in the nineteenth century. The simple synchronicity between climatic change and extinction suggested a causal connection between climate and extinction.

In the early twentieth century, once glaciation theory became even more widely accepted, scientists proposed various mechanisms to explain how climate affected species' survival: some naturalists stressed the direct role of cold temperatures while others invoked the indirect consequences of advancing ice sheets, increasing precipitation, and decreasing temperatures.[255] A few scientists thought that extinctions occurred alongside retreating ice sheets, meaning that

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[253] Donald K. Grayson, "Nineteenth-Century Explanations of Pleistocene Extinctions: A Review and Analysis," in *Quaternary Extinctions: A Prehistoric Revolution*, ed. Paul S. Martin (Tucson: The University of Arizona Press, 1984), 5–39.

[254] Rudwick, *Earth's Deep History: How It Was Discovered and Why It Matters*, 174–80.

[255] For an overview, see Kevin James Francis, "'Death Enveloped All Nature in a Shroud': The Extinction of Pleistocene Mammals and the Persistence of Scientific Generalists" (PhD Dissertation, University of Minnesota, 2002).

warm and dry conditions were responsible for the extinctions. While climatic explanations dominated, a few paleontologists argued that overspecialization, understood as extreme adaptation to a limited set of environmental conditions, resulted in species unable to cope with change.[256] Still others argued that extinction was merely the result of old age; whole species died out in the same ways as individuals.[257] These different explanations coalesced into a comprehensive theory of extinction, published in 1906 by American vertebrate paleontologist Henry Fairfield Osborn, which brought together climate, disease, and other external factors as the causal mechanisms for the Pleistocene extinctions.[258]

Although scientists were willing to hypothesize about multiple causes for the Pleistocene extinctions, prior to the first quarter of the twentieth century, most rejected anthropogenic causes. Scientists dismissed human causes in part because they thought that the arrival of humans in North America postdated the extinctions. In the early twentieth century, physical anthropologist and Smithsonian curator Aleš Hrdlička claimed that American Indians migrated from Asia to North America about 3,000 years ago while the extinctions occurred much earlier.[259] In 1927, the Royal Anthropological institute awared Hrdlička the Huxley Memorial Medal, its highest honor, for these ideas. Hrdlička continued to support these ideas through the 1920s and 1930s with research trips to Alaska and the Yukon in search of evidence that the first Americans had recently entered the New World by way of Asia.

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[256] Henry Fairfield Osborn, "The Present Problems of Paleontology," *Congress of Arts and Science, Universal Exposition, St. Louis* 4 (1904): 1–20; Frederic Lucas, *Animals Before Man in North America* (New York: Appleton, 1902).

[257] A. Smith Woodward, "Address of the President to the Geological Section of the British Association for the Advancement of Science," *Science* 30 (1909): 321–31.

[258] Henry Fairfield Osborn, "The Causes of Extinction of Mammalia," *The American Naturalist* 40, no. 479 (November 1906); Henry Fairfield Osborn, "The Causes of Extinction of Mammalia (Concluded)," *The American Naturalist* 40, no. 480 (December 1906): 829–59.

[259] Aleš Hrdlička, "Neanderthal Phase of Man," *Journal of the Royal Anthropological Institute of Great Britain and Ireland* 57 (1927): 249–74; Aleš Hrdlička, *The Skeletal Remains of Early Man*, Publication 3033, vol. 83, Smithsonian Miscellaneous Collections (Washington, DC: The Smithsonian Institution, 1930).

Even prior to the widespread acceptance of an earlier human arrival in the New World, scientists like Osborn, who believed that humans had arrived in the Americas during the glacial period, were reluctant to consider primitive hunting as a possible cause of the Pleistocene extinctions. Osborn thought that Native Americans were not agents of destruction but "endangered races" in need of their own protection. In 1900, Osborn claimed that, like the "Mastodon, Buffalo, the Elk, Moose, Deer, and Beaver" who first occupied the continent after the last ice sheets retreated, the Indian was threatened by the next wave of colonizers, "our Dutch and English ancestors" who were "enemies and exterminators of all."[260] According to Osborn, prehistoric humans were primitive and weak, they had only "killed with clubs, dug pits, rolled down rocks" and were "probably less destructive than most of the large predatory animals."[261]

Osborn's view reflected the popular vision of the "vanishing Indian," an idea that took root in the nineteenth century after colonists had defeated many of the tribes of the eastern United States. The vanishing Indian ideology posited that Native Americans were a conquered, helpless people who would be swept away as civilization displaced savagery.[262] In Osborn's interpretation, Indians' lack of technological sophistication meant that colonizers were able to cause a wave of extinctions in the more recent period, which threatened Native peoples and animals alike.

In the 1920s and 1930s, several discoveries began to point towards human causes. First, in 1927 paleontologists discovered flint "arrowheads" alongside the bones of the extinct

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[260] Henry Fairfield Osborn, "Address of Welcome at the Opening of the New York Zoological Park," Fourth Annual Report of the New York Zoological Society, 1900, 77.

[261] Henry Fairfield Osborn and H.E. Anthony, "Close of the Age of Mammals," *Journal of Mammology* 3, no. 4 (1922): 219–37.

[262] Brian W. Dippie, *The Vanishing American: White Attitudes and US Indian Policy* (Middletown, CT: Wesleyan University Press, 1982); Berry Brewton, "The Myth of the Vanishing Indian," *Phylon* 21 (1960): 51–57; Martin Barker and Roger Sabin, *The Lasting Mohicans: History of an American Myth* (Jackson, MS: University of Mississippi Press, 1995).

mammoth in Folsom, New Mexico. This discovery caused them to argue that humans had occupied North America for periods longer than most scientists, especially anthropologists adopting Hrdlička's dogma, assumed.[263] The discovery of the Clovis site in New Mexico in 1932, where archeologists unearthed mammoth bones alongside fluted spear points, reinforced the plausibility of anthropogenic causes. Other archeological excavations in the 1930s and 1940s uncovered Clovis points from sites all over North America, revealing an extensive hunting culture.[264] These excavations began to alter the chronology of human settlement in North America, with the peopling of the continent thought to be contemporaneous with the decline of the megafauna. Prior to this, anthropologists and archeologists used Hrdlička's well-hardened dogma of the relatively recent peopling of the New World to reject any sites that posited early arrival dates.[265] By about 1950, Albert Kroeber, who succeeded Hrdlička as the "Dean of American Anthropologists," pushed the dates of human arrival back to between 12,000 and 15,000 years ago because of the accumulating evidence of the Clovis culture and geologists' chronology of Beringia.[266]

While scientists drew on these discoveries to question the synchronicity of climatic change and the extinctions and to point to human hunting as a possible mechanism for the Pleistocene extinctions, some scientists wondered how hunters would have been effective against the megafauna. Geographer Carl Sauer was one of the first to propose a mechanism by which

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[263] J.D. Figgins, "The Antiquity of Man in America," *Natural History* 27 (1927): 229–39; H.J. Cooke, "New Geological and Paleontological Evidence Bearing on the Antiquity of Mankind in America," *Natural History* 27 (1927): 240–47.

[264] Charles C. Mann, "The Clovis Point and the Discovery of America's First Culture," *Smithsonian Magazine*, November 2013, http://www.smithsonianmag.com/history/the-clovis-point-and-the-discovery-of-americas-first-culture-3825828/.

[265] Alexander Ewen, "Bering Strait Theory, Pt. 2: Racism, Eugenics and When Natives Came to America," *Indian Country Media Network* (blog), June 20, 2014.

[266] David J. Meltzer, *The Great Paleolithic War: How Science Forged an Understanding of America's Ice Age Past* (Chicago: University of Chicago Press, 2015).

hunters would have been able to wipe out the megafauna. In the 1940s, Sauer posited that human hunters had used fire to "frighten, injure, and demoralize" the megafauna and to drive and corral them to spaces where they would be easier targets.[267] Once fire had driven the frightened beasts to easy kill sites, prehistoric hunters would have used Clovis points to finish off their prey.

Some were dissatisfied by Sauer's theory, including American anthropologist Loren Eiseley. Eiseley was wary because Sauer's fire-drive mechanism was unlikely to be effective against some of the megafauna that went extinct, including the giant beaver. Although he accepted that "man was on the scene at the final perishing," Eiseley was also dismissive of Sauer's view because he thought that humans had neither "the appetite nor the capacity for such giant slaughter."[268] Eiseley's main objection, which centered on humans' capacity for wholesale destruction, would soon be taken up by those objecting to the overkill hypothesis, alongside arguments questioning the methods that paleoecologists were using to support overkill.

The Development of the Overkill Theory of Pleistocene Extinctions

Paul Martin's overkill theory responded to, and ultimately rejected, many of these earlier ideas about the causes of the Pleistocene extinctions. His theory is important because, as other historians have argued, Martin's conclusion "was so powerfully framed that its appearance marked the end of routine publication of multi-causal explanations of those extinctions." After Martin, most scientists argued "that either climate or people, but not both, provided the driving force behind the extinctions."[269] I argue that, as Martin developed his theory, it became even

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[267] Carl Sauer, "A Geographic Sketch of Early Man in America," *Geographical Review* 34 (1944): 543.

[268] Loren C. Eiseley, "Archaeological Observations on the Problem of Post-Glacial Extinction," *American Antiquity* 8, no. 3 (1943): 214.

[269] Donald K. Grayson, "Explaining Pleistocene Extinctions: Thoughts on the Structure of a Debate," in *Quaternary Extinctions: A Prehistoric Revolution*, ed. Paul S. Martin and Richard G. Klein (Tucson: The University of Arizona Press, 1984), 810. Martin himself was not willing to take the middle ground. When asked why he did not accept multi-causal explanations, Martin replies "That's a terribly tempting thought… so much of science is politics, and politics is compromise. But it's part of the temple vow of academics to drive plausible theories as far as possible…

more difficult to adjudicate which cause, climate or hunting, was more likely because scientific evidence could not support the predictions that Martin came to make.

Martin's first talk on the Pleistocene extinctions, presented to the Academy for the Advancement of Science in 1957, opened by suggesting that most theories about the causes of these extinctions did "injustice to the temporal and ecological record."[270] Martin then enumerated why each of these early hypotheses was inadequate. He noted that the climate-caused hypothesis failed because the fossil record indicated a differential loss between large and small animals, with the megafauna experiencing a precipitous and near-total decline at the end of the Pleistocene, while smaller genera were virtually unscathed. In his view, these differences invalidated climatic causes, which would have affected species regardless of body size. Further, Martin argued that there was a lack of evidence for a major climatic shift during the extinction period. He also critiqued Sauer's fire-drive hypothesis. Following Eiseley, Martin noted that even the most ardent proponents of fire as an ecological force would be hard-pressed to attribute the extinction of some species to this technique, especially when large African herbivores survived fire drives.[271] Finally, Martin questioned those who claimed that the extinctions were part of the normal course of evolution. His main objection stemmed from the observation that the megafauna disappeared without being replaced. Evolutionary theories required that better-adapted genera supplant their morphologically inferior peers, but, in this case, North America was left impoverished of the "great monsters," as Martin quoted Darwin. Instead, it was filled with "mere pigmies [*sic*.], compared with the antecedent allied races."[272]

That's my mission." Paul S. Martin quoted in Sharman Apt Russell, *When the Land Was Young: Reflections on American Archaeology,* (Lincoln, NB: University of Nebraska Press, 1996): 52.

[270] Martin, "Pleistocene Ecology and Biogeography of North America."

[271] Martin, 412..

[272] C. Darwin, 1855, quoted in Martin, "Pleistocene Ecology and Biogeography of North America," 395.

While Martin's observations did not support these other causes of extinction, he did think that there was a synchronicity between the arrival of humans in North America and these extinctions. Martin concluded that this simultaneity demanded an explanation, as he said "the late Pleistocene environment has some unique features. Man is the only one clearly identified."[273] This 1957 presentation was the first to hint at a mono-causal theory of extinction.

After his initial presentation, Martin worked on the extinction hypothesis over the next 50 years, until his death in 2010. His argument changed slightly over that time: he began by stating that human hunters were present in North America during the Pleistocene extinctions and their presence needed to be explained. By the 1960s, Martin claimed that human hunting represented an adequate cause of the Pleistocene extinctions, including those outside the Americas. For example, in 1966, Martin published what he now called the "overkill" hypothesis in *Nature*, where he explained how the peopling of different land masses at different times explained global extinction patterns.[274] In his view, human hunting accounted for the extinctions in the Western Hemisphere, Australia, Madagascar, and other islands. It also explained why large species survived in Africa; there, these species had evolved alongside hominids, so had adapted to human predation, but in these other locations the fauna would not have developed defense mechanisms. A year later he co-edited a book on the idea, *Pleistocene Extinctions: The search for a cause* (1967) in which he claimed that "extinction closely follows the chronology of prehistoric man's spread and his development as a big-game hunter…The phenomena of overkill alone explains the global extinction pattern."[275] Thus, by the late 1960s, Martin's theory had

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[273] Martin, "Pleistocene Ecology and Biogeography of North America," 413.
[274] Paul S. Martin, "Africa and Pleistocene Overkill," *Nature* 212, no. 5060 (October 22, 1966): 339–42.
[275] Paul S. Martin, "Prehistoric Overkill," in *Pleistocene Extinctions: The Search for a Cause,* ed. Paul S. Martin and Herb E. Wright (New Haven, CT: Yale University Press, 1967), 75.

hardened into a mono-causal view where “man, and man alone, was responsible” for a global extinction event.[276]

Martin largely thought overkill was reinforced by better scientific evidence, particularly new proxy evidence. As he wrote to his mentor Edward Deevey in 1978, “the pattern and its chronology and the environment of extinction are all better known and all still neatly comprehensible it seems to me, under the model of overkill.”[277] Martin had long believed that there was a deadly syncopation between human arrival on various landmasses and the extinctions, but, since the 1950s, he had acquired additional evidence from a variety of proxies. He used this evidence to implicate paleo-hunters and discount climatic causes.[278]

New Paleoecological Techniques and the Evidence for Overkill

Much of Martin’s argument about the role played by anthropogenic causes resulted from a belief that the timing of climatic change and the extinctions was discordant, but that the extinctions coincided with the arrival of human hunters. Convinced that an even tighter chronology would lend further support to overkill, Martin sought to use pollen analysis and radiocarbon dating to “establish relatively exact dates for both the extinctions and the arrival of humans at the places where extinctions occurred.”[279] He learned about many of these techniques during a fellowship at Yale in 1955-1956 where Edward Deevey introduced him to pollen analysis. While there, geologist Richard Foster Flint had also encouraged him to use radiocarbon dating on archeological samples. For the reasons discussed in chapter 1, Martin was initially a bit

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[276] Paul S. Martin, “Pleistocene Overkill,” *Natural History* 76 (December 1967): 36.

[277] Paul S. Martin to Edward S. Deevey, September 15, 1978, Box 2, Folder 11, Coll MS442, Special Collections, University of Arizona.

[278] R.D.E. MacPhee, ed., *Extinctions in Near Time: Causes, Contexts, and Consequences* (New York: Plenum/Kluwer Press, 1999).

[279] Martin, *Twilight of the Mammoths: Ice Age Extinctions and the Rewilding of America*, 49.

wary of pollen analysis as a method, but he was excited to use these techniques in broad, and, as he saw it, less problematic ways. As he explained:

> reconstruction of forest community composition from pollen data is beset with difficulties, for example in evaluating relative pollen rain among different wind-pollinated species and correcting for underrepresentation of insect-pollinated plants. However, it seems possible to determine structure of the simplest type, to distinguish forest, savanna, and grassland biochores and, within the first, coniferous and deciduous formations.[280]

The biochore, introduced to Martin by Pierre Dansereau, Martin's adviser at during a second postdoc at the University of Montreal, interpreted the landscape based on vegetation structure.[281] Individual species were unimportant in the biochore; instead, the theory emphasized large-scale similarities within vegetation. For example, in the boreal forest biochore, a continuous canopy characterized by needle-leaved evergreens dominated, whereas the temperate forest biochore mainly comprised broad-leaved deciduous trees and shrubs. Dansereau argued that the vegetation in each biochore was largely controlled by climate, which meant that biochores became a useful unit for comparing climatic shifts with faunal shifts. Martin found this unit so useful that he spent the first half of his 1957 extinction presentation creating maps of changes within biochores at the end of the Pleistocene based on pollen data.

Martin used these maps and pollen diagrams to discount climatic explanations of the Pleistocene extinctions. He could paint broad pictures of the genera present in the biochore, which could be used to suggest how the climate changed. As Martin recounts:

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[280] Martin, "Pleistocene Ecology and Biogeography of North America," 379.

[281] Pierre Dansereau, *Biogeography: An Ecological and Evolutionary Approach* 4th ed, (Oxford: Blackwell, 1957): 244.

> Magically, the fossil pollen record in sediments cored in New England lakes told of the comings and goings of treeless tundra and of spruce, fir, jack pine, and other trees as the climate warmed, the glaciers melted and on occasion readvanced… It even gave clues to the fate of the animal species during this period. For example, fossil pollen counts plotted in percentages as a diagram associated with bones of mastodons indicated that they vanished around the time that, according to the fossil pollen counts, spruce gave way to pine.[282]

Since the replacement of spruce by pine indicated a warming climate, the synchronicity of extinctions with an improving climate seemed problematic to Martin: why had these species gone extinct when the conditions were becoming more similar to ones that they had once thrived under? Mastodons had also survived similar periods of warming in the past, so Martin wondered why they had gone extinct during this period.

Most of Martin's pollen work in the 1950s took place in New England and Quebec, since this was where he was doing his postdoctoral work. He wanted to work in the American southwest where there were many megafaunal remains and Clovis points, but pollen analysts had initially thought that this region was too arid to preserve enough pollen for their methods.[283] By the mid-1950s, just as Martin was finishing his last postdoc, Paul Sears and his students began to show that it was possible to find pollen in arid environments. Sears, along with his research associate Katherine Clisby, took long sediment cores near Mexico City and in New Mexico, and

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[282] Martin, *Twilight of the Mammoths: Ice Age Extinctions and the Rewilding of America*, 5.

[283] The promise of pollen in arid environments was one of the things that attracted Martin to Arizona. Years later, Martin would have to "admit that arid land fossil pollen research now looks somewhat more formidable than when I knew less about it. I see grand opportunities nevertheless, on such matters as the potential for long cores from the 'right' desert lake." See, Paul S. Martin to Edward S. Deevey, December 30, 1976, Box 2, Folder 11, Coll MS442, Special Collections, University of Arizona. At a mastodon site in Ohio, Sears and Clisby had already found the benefits of putting pollen information in conversation with the remains, but they had few paleontological sites to work with. See, Paul B. Sears and Katherine H. Clisby, "Pollen Spectra Associated with the Orleton Farms Mastodon Site," *Ohio Journal of Science* 52, no. 1 (1952): 9–10.

found enough pollen to reveal climatic changes in the region. They found evidence of a glacial-age spruce forest in New Mexico, where only juniper and pinyon now grew. These differences caused Sears and Clisby to infer that the climate had once been much colder to support the spruce visible in the pollen record.[284] This was one of the first instances that called into question the hypothesis that the lower latitudes had escaped the climatic influences of the last glacial period: prior to the 1950s, scientists thought that unglaciated regions had largely dodged the cold temperatures of the Ice Age. With Sears' work in the mid-1950s, it became clear that the climate had cooled in the unglaciated regions.

When Martin got to the University of Arizona in 1957, he began working with pollen. He agreed with Sears that the climate in Arizona had been affected during the Ice Age, but Martin did not think that climatic changes spelled the end of the region's megafauna.[285] Just as in New England and the Great Lakes region, where pollen revealed an improving climate when the megafauna expired, the pollen record in the southwest at the time of these extinctions demonstrated similar improvements. Once again, Martin used this evidence to question climatic explanations, which did not explain why favorable conditions killed off the megafauna.

While Martin used pollen techniques to question climatic explanations, he also began to use other proxy techniques to advance similar conclusions, especially when he realized that a richer record might be available from fossil dung. For example, Martin and his colleagues

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[284] Katherine H. Clisby and Paul B. Sears, "Palynology in Southern North America. Part III Microfossil Profiles under Mexico City Correlated with the Sedimentary Profiles," *Bulletin of the Geological Society of America* 66, no. 5 (1955): 511–20; Paul B. Sears and Katherine H. Clisby, "Palynology in Southern North America. Part IV Pleistocene Climate in Mexico," *Bulletin of the Geological Society of America* 66, no. 5 (1955): 521–30; Katherine H. Clisby and Paul B. Sears, "San Augustin Plains. Pleistocene Climatic Changes," *Science* 124, no. 3221 (1955): 537–529; F. Foreman, Katherine H. Clisby, and Paul B. Sears, "Plio-Pleistocene Sediments and Climates of the San Augustin Plains, New Mexico," in *Tenth Field Conference, New Mexico Geological Society* (Socorro, NM: New Mexico Geological Society, 1959), 117–20.

[285] Paul S. Martin to David J. DesMarais, April 24, 1975, Box 2, Folder 11, Coll MS442, Special Collections, University of Arizona.

buttressed the overkill theory with organic material found in giant sloth dung.[286] The *Tucson Daily Citizen* reported on this work in 1959, describing how the "seven-foot beast… ate just about any type of plant, bush, or tree leaf he could find." Given that sloth's dung was found in layers in caves, Martin and his colleague Dick Shutler Jr., a geochronologist, could use the vegetation remnants found at various depths in the dung to infer changes in the climate at different moments of time. Paleoecologists like Martin were turning excrement into a promising proxy.

From these inferences, which were aided by radiocarbon dating in order to create a chronology of the sloth's changing diet, Martin and Shutler concluded that the climate was improving at the end of the Pleistocene and was becoming more similar to conditions under which the sloth had once thrived. Instead, the sloth had gone extinct. This caused Martin to posit that climatic explanations were inadequate, but that human-caused extinctions were more likely. As the newspaper explained, "The sloth survived changes in climate, but he couldn't escape the dinner table of man."[287]

By the 1970s, Martin and his colleagues were using yet another proxy for historical ecology: packrat middens.[288] Like sloth dung, the debris piles constructed by woodrats in the genus *Neotoma* contained stratified bits of fossilized plants that could be used to reconstruct the climate and vegetation over the last 50,000 years. The organic materials in middens were an improvement over those found in sediment layers or in sloth dung: there were often well-preserved macrofossils in middens, which could be used to determine the vegetation to the

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[286] Martin, Sabels, and Shutler, "Rampart Cave Coprolite and Ecology of the Shasta Ground Sloth."

[287] "Hungry Man, Not Harsh Climate, Killed Sloth," *Tucson Daily Citizen*, January 15, 1959; "Pollen Menu Dates Prehistoric Sloth," *New York Times,* February 3, 1959.

[288] Wells and Jorgensen, "Pleistocene Wood Rat Middens and Climatic Change in the Mohave Desert: A Record of Juniper Woodlands."

species level, rather than just the genus, the more common level of resolution for pollen data. Packrat middens provided "an amazingly rich source of late Pleistocene plant remains" that allowed ecologists to track how plant communities of the southwest had responded after the last ice age. These data supported Martin's overkill hypothesis by demonstrating the inadequacy of climatic explanations.[289] Martin was so excited by their potential that he wrote to a friend that "fossil woodrat middens rich in seeds, leaves, bones and of course pollen, are much more interesting to me" than sediments cores. As a result, he largely abandoned sediments to focus on middens and dung.[290]

With these new techniques, Martin recalled that his team was "equipped with several crucial new tools for exploring the late Quaternary." These tools "provided rich opportunities to see what some of the large-animal extinctions looked like up close.[291] When Martin scrutinized the Pleistocene extinctions with the level of detail these new tools provided, he continued to conclude that climatic explanations were invalid. As he recalled in 1988 "I have not changed my views. Some evidence has, however, changed my approach" because he moved away from sediment analysis towards the records available from dung and middens.[292] These records provided more taxonomic resolution and very local spatial resolution, situating the data in place in the ways that pollen did not. Dung and middens also offered data for areas where lake sediment deposits were rare, but where macrofossils from extinct species were common. The dung and midden records, however, were not very detailed in terms of temporal precision, so

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[289] Paul S. Martin to Robert T. Clausen, August 25, 1981, Box 2, Folder 6, Coll MS442, Special Collections, University of Arizona.

[290] Paul S. Martin to Lincoln Constance, August 28, 1979, Box 2, Folder 6, Coll MS442, Special Collections, University of Arizona.

[291] Martin, *Twilight of the Mammoths: Ice Age Extinctions and the Rewilding of America*, 75.

[292] Martin to Burney, January 11, 1988.

Martin turned to pollen analysis, then radiocarbon dating, so that data from dung and middens could be used to support his chronologically dependent argument.

In the 1940s, as pollen workers began to understand the annual deposition rates of pollen and sediment, they began to use these sedimentation rates as a chronometer. By the 1960s, when paleontologists found mammal fossils in a pollen sequence in the American southwest, analysts could calculate approximately when the fossil had been deposited. In that region, about a centimeter of sediment corresponded to about a decade of time. By measuring the depth of the megafaunal remains, analysts could estimate to within a ten-year period when the fossil had been deposited. Regionally, where there was less knowledge about sediment rates, "events could not be ordered much more closely than to the nearest century," which was still an "incredibly precise" chronology for specimens more than 10,000 years old.[293] However, because of the regional variation in sedimentation rates, sedimentation proved to be a difficult-to-calibrate chronometer.

As University of Chicago physical chemist Willard F. Libby provided evidence that radiocarbon could be used to date organic materials in the late 1940s and 1950s, Martin changed his approach once again. By the late 1960s, he began sending off samples of dung and bone for radiocarbon dating, rather than using sedimentation rates to estimate when the species went extinct. Radiocarbon dating was particularly helpful for the overkill hypothesis, which gained its force through chronological arguments that found a synchronicity between human arrival on unpopulated landmasses and the extinction of the megafauna. With radiocarbon dating, Martin thought he could compare the wave of human migration with various extinction events, determining whether the synchronicity held as paleo-hunters arrived in new parts of the globe.

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[293] Edward S. Deevey, "Introduction," in *Pleistocene Extinctions: The Search for a Cause*, ed. Paul S. Martin and Herb E. Wright (New Haven, CT: Yale University Press, 1967), 66.

The principles of radiocarbon dating were relatively simple. After initial research predicted that all living matter absorbed small amounts of radioactive carbon-14 from carbon dioxide, Libby and colleagues explained that radiocarbon could be used for dating.[294] Their short 1949 article in *Science* explained how, given known estimates of the half-life of carbon-14, "one can calculate the specific activity to be expected" from an organic sample after an organism dies.[295] After death, living beings no longer absorb radiocarbon, meaning that any radiocarbon incorporated by the organisms would continue to decay at a known rate, without any additional radiocarbon added. Using a Geiger counter to detect the energy that is emitted by each radiocarbon atom as it disintegrates, Libby could then compare the energy with estimates of original carbon content, given carbon-14's half-life. The older the sample, the less energy would be detected because more carbon-14 would have decayed. By the end of 1949, a second article appeared in *Science* which dated five samples. Around the same time, Libby began to provide dates to archeologists, paleontologists, and geologists who sent samples to his University of Chicago laboratory.[296]

The radiocarbon dates on the bones of Pleistocene megafauna added precision to the timeline of Pleistocene events. As historian Kevin Francis has argued, radiocarbon dating moved the timeline of prehistoric cultures backward in time, such that scientists came to believe that human arrival coincided with the extinction of the megafauna.[297] As radiocarbon dating

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[294] W. F. Libby, "Atmospheric Helium Three and Radiocarbon from Cosmic Radiation," *Physical Review* 69 (1946): 671–72.

[295] W. F. Libby, E. C. Anderson, and J. R. Arnold, "Age Determination by Radiocarbon Content: World-Wide Assay of Natural Radiocarbon," *Science* 109, no. 2827 (1949): 228.

[296] J. R. Arnold and W. F. Libby, "Age Determination by Radiocarbon Content: Checks with Samples of Known Age," *Science* 110 (1949): 678-80.

[297] Francis, "'Death Enveloped All Nature in a Shroud': The Extinction of Pleistocene Mammals and the Persistence of Scientific Generalists," 319.

confirmed this synchronicity, even critics had to admit that "the coincidence in time of the arrival of man with these extinctions" was largely convincing.[298]

Although radiocarbon dating helped to establish the synchronicity of human arrival in North America and the extinction of Pleistocene herbivores, it would be wrong to assume, as some scientists and historians have, that radiocarbon dating or the conclusions drawn from proxy evidence were free of problems. Like many proxies, radiocarbon was a helpful tool for studying the past, but there were still uncertainties. For example, radiocarbon dating initially required fairly large samples of organic material. Often enough material was not available, so Martin's chronological argument did not have a particularly strong chronology to support it, particularly across continents. Even by the 1980s, when a number of refinements in mass spectrometry allowed smaller samples to be dated, the record was still quite patchy.[299] Some critics were also concerned that the dates offered by radiocarbon dating were rather strange. In some cases, radiocarbon dating identified samples that were thousands of years younger than all other samples for that species. Was this a problem with the method or had a small number of individuals of a species survived? In other cases, the date of the dung did not match dates of tissue or peat samples, which led to questions about the causes of these discrepancies.[300] At times, scientists could invoke contamination or geological processes that mixed sediments to explain these anomalies, harkening back to the need to ground data in the field, discussed in chapter 1. For example, in 1973, Martin prepared a research proposal for the National Science

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[298] Grover S. Krantz, "Human Activities and Megafaunal Extinctions: Man's Modification of the Environment May Have Caused the Demise of Some Large Pleistocene Mammals," *American Scientist* 58, no. 2 (1970): 164.

[299] "Accelerator Facility for Radioisotope Analysis - Information for Users" (University of Arizona and National Science Foundation, June 1988), Box 17, Folder 2, Coll MS442, Special Collections, University of Arizona; Paul S. Martin to Paul Damon, April 7, 1978, Box 17, Folder 2, Coll MS442, Special Collections, University of Arizona.

[300] Weston Blake, Jr. to David S. Christie, July 10, 1972, Box 18, Folder 1, Coll MS442, Special Collections, University of Arizona; Douglas R. Grant to Douglas Byers, September 26, 1972, Box 18, Folder 1, Coll MS442, Special Collections, University of Arizona.

Foundation (NSF) in which he sought to use radiocarbon dating to retest certain samples of sloth dung from arid caves, worried that the activities of postglacial woodrats had contaminated the dates.[301] As late as 1985, Martin was still arguing that the blitzkrieg chronology required “a very tight chronological relationship between arrival of human colonizers and extinction of the megafauna,” and thus any chronological outliers could be used to question the theory. Martin thus sought to limit those questions with further support from the NSF.[302]

Overall, from the 1950s through the 1980s, Martin put together evidence from a variety of proxy sources to create a fuller picture of what occurred at the end of the Pleistocene. This evidence pointed to newly arrived human hunters as the cause of the North America extinctions, but, by 1973, Martin began to test the hypothesis using mathematical models.

Models and Predictions

In the 1970s, Martin was beginning to create mathematical models that made predictions about what paleoecologists would find in the fossil record. By modelling how a small human population would have moved across the Americas at the end of the Pleistocene, the model would describe when extinctions took place in different parts of the continent. These descriptions were predictions about how the geological record would reflect the events at the end of the Pleistocene: it modelled when scientists would find the first evidence of human hunters in a particular place, and when they would find the last remains for an extinct species (see Figure 10).

Martin’s models were based on numerical estimates of factors that Martin deemed most relevant to the composition of North America’s fauna before the arrival of humans, as well as factors that would affect the demographic patterns of the migrants. For example, in the 1973

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[301] Paul S. Martin, “Research Proposal to the National Science Foundation: Pleistocene Environments and Extinction,” 1973, Box 18, Folder 1, Coll MS442, Special Collections, University of Arizona.

[302] Paul S. Martin, “Proposal to the National Science Foundation,” June 1985, Box 18, Folder 1, Coll MS442, Special Collections, University of Arizona.

Science article which first introduced a model, Martin estimated the size of the megafauna population by looking at game preserves in Africa and livestock density in North America. He also estimated the size, fecundity, and migration capacity of the first human arrivals in the Americas, as well as the number of megafauna human hunters could kill each year, from various demographic estimations.[303]

From these estimates, this model posited that one hundred Paleoindians arrived near Edmonton, Alberta approximately 12,000 years ago, after crossing the Bering Strait. Martin then assumed that the hunters moved south, at a rate of about 20 miles a year, and killed one animal per person as they travelled. As they migrated and hunted, they also reproduced, doubling their population every twenty years. Martin justified this high reproductive rate by looking at other cases, and by claiming that any population "would unavoidably explode" when entering a new and favorable habitat. He argued that "the environment of the New World should have been particularly favorable. The hunters who conquered the frozen tundra of eastern Siberia and western Alaska must have been delighted when they first detected milder climates as their route turned southward." Further, without predation and hominid diseases common in the tropics, Martin expected hunters to have multiplied rapidly.[304] With those estimates, in just three hundred years, the paleo-hunters would have killed over ninety million megafauna and the human population would have grown to 100,000 people spread over a thousand miles. As he said in the *Science* article:

> a very large biomass, even the 2.3 x 10^8 metric tons of domestic animals now ranging the continent, could be overkilled within 1000 years by a hunter population never exceeding 10^6. We need only assume that a relatively innocent prey was suddenly exposed to a new

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[303] Martin, "The Discovery of America," 970.
[304] Martin, 970.

> and thoroughly superior predator, a hunter who preferred killing and persisted in killing animals as long as they were available.[305]

On this model, the megafauna would have disappeared from North and Central America in about six hundred years and from South America about 500 years later.

In other simulations, Martin's assumptions were more conservative, partially in response to critics who worried that the paleo-hunters were "too successful" on Martin's first model because he assumed "a fantastic scenario since the postulated reproduction rate is unknown in human history."[306] Most of Martin and colleagues' more conservative estimates still resulted in Paleoindians reaching Tierra del Fuego at the tip of South America a thousand years after they arrived in the Americas, with the megafauna hunted to extinction as they migrated.[307]

Given that these models posited an advancing front of hunters, the overkill hypothesis made predictions about what would be found in the fossil record. Martin thought that humans had arrived in South America approximately 500 years after crossing the Bering Strait. Given this migration route, he posited that the extinctions in South America should occur after those in North America. As Martin explained, overkill "turns on precision of dates and dating of archeological material… as well as extinct faunal material."[308] Given that, by the 1970s, dating techniques offered century-level precision for dating bones and artifacts, a number of people sought datable materials throughout the Americas. Much of this material initially supported the prediction of overkill, lending support to Martin's predictions and the overkill hypothesis.

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[305] Martin, 972.

[306] Vine Deloria, Jr., "The Speculations of Krech," *Worldviews: Global Religions, Culture, and Ecology* 4 (2000): 286; Krech III, *The Ecological Indian: Myth and History*, 34; Paul S. Martin to Marjolaine Boutin-Sweet, September 7, 1982, Box 2, Folder 1, Coll MS442, Special Collections, University of Arizona.

[307] James E. Mosimann and Paul S. Martin, "Simulating Overkill by Paleoindians: Did Man Hunt the Giant Mammals of the New World to Extinction? Mathematical Models Show That the Hypothesis Is Feasible," *American Scientist* 63, no. 3 (1975): 304–13.

[308] Paul S. Martin, "Draft – The University of Arizona Accelerator Group," n.d., 1, Box 17, Folder 2, Coll MS442, Special Collections, University of Arizona.

Human Aggression and the Overkill Hypothesis

While Martin was making testable predictions using computer models, critics felt that some of the content built into these models was suspect and untestable. Critics particularly objected to Martin's claims about human nature: by saying that humans "preferred killing," Martin was treading into a debate about human aggression, which caused some scientists and the public to distrust the overkill hypothesis. Critics could ask whether Ice-Age humans – and our species more generally as some projected to *Homo sapiens* from this case – were technologically and morally capable of such large-scale destruction. Even Martin was aware of how uncomfortable this suggestion was. As he explained, "the thought that prehistoric hunters ten to fifteen thousand years ago… exterminated far more large animals than has modern man with modern weapons and advanced technology is certainly provocative and perhaps even deeply disturbing."[309] Yet, as I argue, it was not just that critics were disconcerted by Martin's claims; they were also disturbed by the way that Martin's predictions were overstepping the evidence by treading into questions of human nature. Questions about human nature were fraught with uncertainty during the Cold War.

Some scientists certainly agreed with Martin's assessment that humans were hunters. In the 1960s, anthropologists began more explicitly recognizing humans as hunters. In 1966, the University of Chicago hosted a symposium entitled "Man the Hunter," which resulted in a book of the same name published in 1968.[310] The book brought together recent ethnographic research on hunter-gatherers and argued that human hunting was a crucial stage of human development. It also claimed that human hunting was once a universal way of life. In this view, the earliest

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[309] Martin, "Prehistoric Overkill," 115..

[310] Richard Barry Lee and Irven DeVore, eds., *Man the Hunter* (New Brunswick, N.J.: Aldine Transaction, 1968).

hominids had a "killer instinct," a trait that the first humans in North America shared because, only "careful training" could "hide the natural drives" to chase and kill other animals.[311]

As the aggressiveness hypothesis gained support, views like Loren Eiseley's from the 1940s faded. Eiseley was the anthropologist who had claimed that humans did not have the appetite for slaughter on a massive scale.[312] Yet, in the 1960s and 1970s, the Man the Hunter symposium and book, as well as playwright and anthropologist Robert Ardrey's popularization of man-the-hunter in a number of popular books, lent support to views about humanity's destructive tendencies.[313] These ideas were particularly popular at the height of the Cold War: Martin himself used the ongoing Vietnam War and nuclear proliferation to argue that humans did not exercise restraint and could be inherently violent.[314] For example, in 1969, in a speech on the nature of human aggression at a symposium at the University of Idaho on war and violence, Martin made his views about humans' capacity for violence more explicit.[315] Rather than discuss contemporary problems, Martin used the Pleistocene extinctions as well as evidence of crushed baboon skulls found in alongside the bones of early man to argue that "you can make a case for man being a killer ape."[316] Martin, who appeared to be unaware of the recently published work on man-the-hunter, claimed that this position went against paleontologists and anthropologists

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[311] S.L. Washburn and V. Avis, "Evolution of Human Behavior," in *Behavior and Evolution*, ed. A. Roe and G.G. Simpson (New Haven, CT: Yale University Press, 1958), 433.

[312] Eiseley, "Archaeological Observations on the Problem of Post-Glacial Extinction," 214.

[313] Robert Ardrey, *African Genesis: A Personal Investigation into Animal Origins and Nature of Man* (New York: Atheneum, 1961); Robert Ardrey, *The Territorial Imperative: A Personal Inquiry into the Animal Origins of Property and Nations* (New York: Dell Publishing Co., 1966); Robert Ardrey, *The Hunting Hypothesis: A Personal Conclusion Concerning the Evolutionary Nature of Man* (New York: Antheneum, 1976).

[314] Sharman Apt Russell points to another reason why human aggression could be so compelling for Martin: his wife had once been attacked in their own kitchen and violence in Tucson was on the rise while Martin worked there. See, Sharman Apt Russell, *When the Land Was Young: Reflections on American Archaeology* (Lincoln, NB: University of Nebraska Press, 1966), 40.

[315] "Symposium Available on Television," *Idahonian*, March 13, 1969.

[316] "Martin and Wallrich Advocate Non-Violence," *The University of Idaho*, March 14, 1969.

"who cling to the notion that prehistoric man was an innocent animal, capable of no wrong, with no destructive tendencies."[317] At humanity's root, Martin claimed, was a killer instinct.

But, by the time Martin was modelling, people were calling man-the-hunter hypothesis into question. Part of the distaste about the man-the-hunter theory resulted from the way that sociobiologists were using it to explain human biology and human morality.[318] Sociobiology, especially after the publication of E.O. Wilson's *Sociobiology: The New Synthesis* (1975), quickly became the subject of controversy for the way that sociobiologists reduced aggression and other human behaviors to biology, rather than social environment.[319] Since overkill expressed a strong sense that humans were aggressive by nature, Martin's theory was questioned for many of the same reasons as sociobiological theories. He could not turn to his proxies to overcome this critique.

In the 1970s, as critics questioned the way that overkill and sociobiology were reducing human nature to violence, Martin and others were also reducing human nature to dominion, rather than conservation.[320] This reductionism also had consequences for how scientists responded to migration models, as critiques questioned whether the dominion view truly captured human nature.

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[317] "Martin and Wallrich Advocate Non-Violence."

[318] Robert W. Sussman, "The Myth of Man the Hunter, Man the Killer and the Evolution of Human Morality," *Zygon* 34, no. 3 (September 1999): 453–71.

[319] Erika Lorraine Milam, "The Ascent of Man and the Politics of Humanity's Evolutionary Future," *Endeavour*, Science in the Public Eye, 40, no. 4 (December 1, 2016): 225–37; Myrna Perez, "Evolutionary Activism: Stephen Jay Gould, the New Left and Sociobiology," *Endeavour* 37, no. 2 (2013): 104–11; Neil Jumonville, "The Cultural Politics of the Sociobiology Debate," *Journal of the History of Biology* 35, no. 3 (2002): 569–93; Ullica Segerstrale, *Defenders of the Truth: The Battle for Science in the Sociobiology Debate and Beyond* (New York: Oxford University Press, 2000). See also Milam's forthcoming book, *Creatures of Cain: The Hunt for Human Nature in Cold War America* (forthcoming from Princeton University Press in December 2018).

[320] This view that humans had dominion over the earth was articulated most forcefully by Lynn White, Jr, who argued that Judeo-Christian theology grants this dominion. See, Lynn White, Jr., "The Historical Roots of Our Ecological Crisis," *Journal of the American Scientific Affiliation* 21 (June 1969): 42–47.

Like the aggression claim, some were quick to accept the dominion claim: biologist Steven P. Christman believed that humans were incapable of practicing conservation. In a letter to Martin in the 1985 he said,

> I refuse to believe that prehistoric people practiced conservation of their resources any better than modern man… When man first arrived anywhere, he was immediately a predator to be reckoned with… But as weapons and hunting skills improved, even more species of prey became vulnerable. The first people in Florida clubbed the giant tortoises to extinction, later they wiped out the Carolina parakeet, and they're still working on the sea turtles."[321]

Christman's letter speaks to a growing awareness about anthropogenic extinctions and Martin's strategy of using well-established cases of anthropogenic extinction to lend support to ideas about human dominion. Anthropologist John H. Bodley was convinced by this line of reasoning. He wrote, "if prehistoric people killed out the moas of New Zealand and the giant lemurs of Madagascar, perhaps Paleolithic people could also have destroyed even mammoths and mastodons in America."[322] Human-caused extinction, writ large, was lending support for views about human nature.

While Martin could not go to the proxy record to find support for the dominion hypothesis, he had explanations for why Pleistocene hunters, in particular, would have been unlikely to exercise restraint. He argued that "powerful behavioral reinforcers associated with excitement of the chase," would easily have turned hunters into a "superpredator… a species

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[321] Steven P. Christman to Paul S. Martin, June 18, 1985, Box 2, Folder 7, Coll MS442, Special Collections, University of Arizona.

[322] John H. Bodley to Paul S. Martin, September 20, 1983, Box 2, Folder 1, Coll MS442, Special Collections, University of Arizona.

which kills more than food alone."[323] Further, Martin thought the "'consumer-oriented' rush through the virgin continent" would have resulted in indiscriminate killing.[324] Given humans' inherently destructive nature, fueled by the thrill of a hunt in a new landscape full of possibility, Martin did not give much stock to those who thought that early humans would have acted with restraint or practiced conservation. All of these ideas, however, came to be questioned as racist and deterministic, undermining the theory, and leading to questions about what scientists could predict.

The Falsifiability of Overkill

As Martin made these claims about human nature, he trod into a minefield of criticism about evidence. Part of the problem was that he could use the proxy record to refute the climatic hypothesis and use radiocarbon dating to test his models, but he could not find positive support for overkill in the macrofossil record. Archeologists and anthropologists struggled to find evidence of large-scale human-caused extinction in the paleontological and archeological record. In particular, archeologists and anthropologists worried that scientists had discovered few kill sites, places where extinct megafauna were associated with Clovis points and other human artifacts. As anthropologist Grover Krantz, who developed a multi-causal account of the extinctions where hunting only played a minor role, put it, "the absence, in most cases, of the remains of horse, camel, mastodon, or pronghorn from kill sites and their occurrence in natural deposits hardly support the contention that man killed them off."[325] This absence was even more striking in the record from the Americas because, in Eastern Europe, scientists had found

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[323] Walter Sullivan, "'Overkill of Animals Laid To Huntsmen in 9000 B.C.," *New York Times*, February 13, 1972.
[324] Martin to Boutin-Sweet, September 7, 1982; Martin to Christman, June 25, 1985.
[325] Krantz, "Human Activities and Megafaunal Extinctions," 166.

megafaunal remains with art objects and other cultural artifacts.[326] Scientists wondered why humans in the Americas would not have incorporated these species into their art or household objects if they had lived alongside them. Not only were there no cave paintings, there were few sites that indicated any association between humans and many of the extinct megafauna. At North American sites where scientists found the extinct genera in archeological contexts, scientists found only thirteen of the thirty-five genera that Martin had argued went extinct due to human hunting.[327] The small number of sites, along with the small number of species represented in them, led to a concern that there was only strong evidence for anthropogenic explanations for the mammoth and mastodon extinctions. The evidence, critics claimed, did not support human-caused extinction for the whole host of species implicated in Martin's overkill hypothesis.[328]

Martin, however, did not view the paucity of sites as a reason to call the overkill hypothesis into question. Instead, he used the chronology of human arrival and the disappearance of the megafauna to explain the absence in the fossil record. As he reminded Duke graduate student David Burney, "a very narrow extinction 'window' makes it easy to explain absence of extinct fauna in archaeological contexts"[329] because it would mean that Paleoindians had rapidly killed and butchered the megafauna as they populated new territory. They would not have had time for cave paintings depicting the beasts they found on the new continent, they were too busy hunting, migrating, and reproducing. Further, Martin believed that Paleoindians killed animals as they found them, meaning that kill sites would be scattered and ephemeral. Paleontologists would not see the distinctive remains such as those found at buffalo jumps, the cliffs where later

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[326] Martin, "The Discovery of America."
[327] Kelly and Prasciunas, "Did the Ancestors of Native Americans Cause Animal Extinctions in Late-Pleistocene North America?," 104.
[328] Kelly and Prasciunas, 104–5.
[329] Paul S. Martin to David Burney, December 23, 1986, Box 2, Folder 2, Coll MS442, Special Collections, University of Arizona.

Plains Indians drove bison to their deaths en masse. Further, since Martin posited that the human population was relatively small and not very dense, he believed that paleo hunters would have been unlikely to leave much of a record. Given these assumptions, Martin claimed, "the only remarkable aspect of New World archaeology is that *any* kill sites have been found."[330] This claim that the speed of overkill left little record effectively made part of his hypothesis untestable: paleontologists were not likely to find more evidence to support his views, since it was a lack of evidence that buttressed his claims.

Many anthropologists involved in the debate were concerned about Martin's use of negative evidence to buttress his theory. On their view, Martin was not practicing good science. Anthropologist Stephen Krech III, for example, lamented, "If only there were numerous archaeological sites with associated extinct megafauna to test Martin's thesis of overkill. But there are only fifty or so sites – a mere handful."[331] Anthropologists Donald K. Grayson and David J. Meltzer argued that Martin's move to turn the lack of evidence for overkill into empirical support "removes the hypothesis from the realm of science and places it squarely in the realm of faith."[332] These critics thus found it "amazing" that Martin would continue to be so certain that man alone was responsible for the extinctions in light of a lack of positive evidence. They were worried that Martin's hypothesis could delegitimize their discipline for its lack of rigor or responsiveness to evidence.[333]

Even if Martin's appeal to the paucity of sites effectively rendered part of his hypothesis untestable, Martin argued that his hypothesis could be falsified. As he said, the theory rested on

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[330] Martin, "The Discovery of America," 969. Italics in original.
[331] Krech III, *The Ecological Indian: Myth and History*, 36.
[332] Donald K. Grayson and David J. Meltzer, "A Requiem for North American Overkill," *Journal of Archaeological Science* 30, no. 5 (May 2003): 585.
[333] Krech III, *The Ecological Indian: Myth and History*, 36.

the idea that, as human hunters populated new continents, they wiped out the megafauna, which had little experience dealing with human predation. This assumption was one of the reasons that the synchronicity of events was so important: on Martin's view, humans needed to arrive in North America and rapidly wipe out the megafauna, otherwise the now-extinct beasts would adapt to hunting as they had in Africa. If evidence of earlier human populations arose, the overkill model would likely fail.[334] These predictions about synchronicity could be tested using proxy evidence.

While Martin stated that early human arrivals would invalidate overkill, once some sites were found that indicated that humans were in the Americas before the end of the Pleistocene, he had responses that explained away these early sites.[335] He could question whether the archeological sites in Texas, Chile, and Brazil actually pointed to a pre-Clovis arrival, especially given some concern among archeologists about the dating of the sites.[336] Even when Martin did not discount this evidence outright, he claimed that the early arrivals were likely few in number and were not big game hunters. As put it, there is an "absence of ample evidence for an appreciable Paleoindian population prior to 12,000 years."[337] The small population that could have been in the Western Hemisphere before the Ice Age would not have threatened the megafauna because it was too small and did not have the technology to do so. Only the Clovis hunters would have been able to easily eliminate the megafauna when they arrived in North America. No matter the evidence, Martin had a response, but many of his colleagues in the sciences began to wonder whether these responses were valid.

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[334] Paul S. Martin to John E. Bodley, October 5, 1983, Box 2, Folder 1, Coll MS442, Special Collections, University of Arizona.

[335] For a summary of some of the findings that challenge the Clovis-first model, see Simon Romero, "Discoveries Challenge Beliefs on Humans' Arrival in the Americas," *The New York Times*, March 27, 2014.

[336] Paul S. Martin to C.R. Berger, September 7, 1982, Box 2, Folder 1, Coll MS442, Special Collections, University of Arizona Library.

[337] Martin to Berger.

Native American Responses to Overkill

In the late 1980s and early 1990s, a new group of people began to criticize the foundations of the overkill hypothesis. This group comprised Native American activists, which was led by Vine Deloria Jr. Deloria was a member of the Standing Rock Sioux who had attempted to demythologize how white Americans thought of American Indians since the publication of his first book in 1969.[338] He objected to the way that some people were using the overkill hypothesis – despite all its problems – to suggest that, in the past, Native Americans had mismanaged their lands. By the 1980s, Deloria claimed that Martin's theory had been taken up by policymakers to suggest that present-day Native Americans could not manage their lands because their ancestors had mismanaged the bounty they had found upon arriving in North America. Deloria objected to this view to be sure, but his critique really condemned the hegemony exercised by scientific theories over other forms of knowing and other kinds of evidence.

Deloria only became involved once the argument discounting his ancestors developed. This argument had developed slowly. Martin's first paper had only suggested the humans were likely present during the extinctions. But, over the years, Martin and others began to refer to these people as "Paleoindians," "the first Americans," and "ancestral Indians." From there, some began making connections between present-day Native Americans and their conservation practices.

In 1999, anthropologist Shepard Krech III made the connections between American Indians and Pleistocene extinctions explicit. In *The Ecological Indian: Myth and History* (1999) Krech used the Pleistocene extinctions to show that Native Americans were not the ecological

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[338] Jr Vine Deloria, *Custer Died for Your Sins: An Indian Manifesto* (Norman: University of Oklahoma Press, 1969).

Indians many have made them out to be. Instead, they had a mixed relationship to the environment, at times conserving it, at times exploiting it, but, at the end of the Pleistocene, he quotes Martin repeatedly to show just how destructive Native Americans were.[339]

Krech's findings supported "conservative newspaper columnists, right-wing fanatics, sportsmen's groups, and scholars" who believed that Indians lacked "moral fiber and ethical concern for the Earth."[340] Symptomatic of this perspective were the views of Canadian journalist and former politician, Douglas Fisher. Fisher reviewed Krech's book in a Toronto-based newspaper, arguing:

> Canadian Indians should not be accorded the superior sanction of high-minded environmentalism in negotiations of land settlements [...]. It should also mean much more balance in responding to native demands and needs simply because discussion of them no longer should be burdened with the guilt piled on the whites for devastating a noble people whose societies once lived – and might do so again – in perfect harmony with nature. Indians are neither more noble nor ignoble than other people – in their blood, or in their history.[341]

With Fisher's comments, Deloria's fears about the ways that the overkill hypothesis was delegitimizing Native Americans' land-rights and sovereignty were coming true.

While Deloria was concerned about these kinds of comments, he was particularly angered at the ways in which scientists overstepped the evidence, presenting contested theories as fact. In *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact* (1995), Deloria used overkill as an example of the hegemony of ill-supported scientific theories, despite

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[339] Krech III, *The Ecological Indian: Myth and History*.
[340] Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact*, 112.
[341] Fisher, "The Myth of the Ecological Indian."

other evidence, especially the evidence found in Native American traditions. Deloria claimed "scientists have maintained a stranglehold on the definitions of what respectable and reliable human experiences are. The Indian explanation is always cast aside as a superstition," even though Native Americans had explanations of "their origins, their migrations, their experience with birds, animals, lands, waters, mountains, and other peoples."[342]

Before turning to these Native American explanations, Deloria continued to call out the inadequacy of scientific theory. He quoted Jared Diamond, who had originally trained in physiology but whose work drew on fields ranging from anthropology to ecology to geology to evolutionary biology. In 1987, Diamond had claimed that "it's highly suspicious that the sloths and the goats disappeared just after Clovis hunters reached Arizona. Juries have convicted murderers on less compelling circumstantial evidence."[343] Deloria thought that Diamond's comments were "symptomatic of the manner in which scientists have tried to indict Paleo-Indians for the massive extinctions" because:

> there have been times, particularly in the American South at the end of the last century, when a single accusation against an African American was enough to ensure a conviction. What scholars have done, therefore, is something akin to a southern lynching, since there is little evidence that Paleo-Indians hunters did anything more than occasionally catch a mammoth at a watering hole.[344]

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[342] Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact*, 19.

[343] Jared Diamond, "The American Blitzkrieg: A Mammoth Undertaking," *Discover Magazine*, June 1987, 84.

[344] Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact*, 128–29.

Deloria insisted on more evidence before condemning his ancestors. He knew that other scientists were expressing concerns about overkill so demanded to know, "if you can't test the thesis because there is no evidence, why does it still qualify as a thesis?"[345]

As part of Deloria's larger critique of viewpoints that venerated science as the purveyor of ultimate truth, he devoted a chapter of his book to rejecting Martin's overkill hypothesis, claiming that the research did not support the theory.[346] Deloria claimed that some of the evidence for a land bridge, which Martin posited had facilitated the arrival of the paleo hunters, was relatively unsupported. As Deloria wrote, "many scientists" suspect that the "first Indians had emigrated from Asia… because the Indians of today look so much like contemporary Asians."[347] This resemblance alone, in Deloria's view, was not good science. Instead, there was stronger evidence about the origins of Native peoples in their origin stories. Deloria also showed that archeological findings also challenged Martin's chronology about human arrival.[348] Given the limited archeological evidence and competing Native accounts, many Native activists came to view the adherence to the Bering Strait hypothesis as the "politicization of science," since it became a theory that was used, in Deloria's words, to "deny the fact that we were full, complete, and total owners of this continent."[349]

In Deloria's estimation, Martin also failed to take into account other ways of knowing as he constructed his claims. Not only did Martin fall short in accounting for the archeological

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[345] Deloria, Jr., "The Speculations of Krech," 286. Deloria, Jr., 286.

[346] Other Native thinkers have critiqued the "land bridge theory as a master narrative that for a couple of centuries has served multiple ideological agendas, lasting despite decades of growing evidence that casts doubt on the way the story has been perpetuated in textbooks and popular media." See, Roxanne Dunbar-Ortiz and Dina Gilio-Whitaker, *"All the Real Indians Died Off" and 20 Other Myths about Native Americans* (Boston: Beacon Press, 2016), 14.

[347] Diamond, "The American Blitzkrieg: A Mammoth Undertaking," 82.

[348] Alexander Ewen, "Bering Strait Theory, Pt. 1: How Dogma Trumped Science," *Indian Country Media Network* (blog), June 14, 2014, https://indiancountrymedianetwork.com/news/native-news/bering-strait-theory-pt-1-how-dogma-trumped-science/.

[349] Deloria, Jr., *Red Earth, White Lies: Native Americans and the Myth of Scientific Fact*, 84.

record, he also neglected the rich history found in Native American oral traditions, many of which could find support in the archeological record. Deloria listed many examples of this kind of corroboration, which supported climatic explanations for the extinctions. For instance, in one Native American story discussing the origin of the Chief's Face, a rock formation on Mount Hood, south of the Columbia River in Oregon, an elder commented: "In those days [early times] the Indians were also taller than they are now. They were as tall as the pine and fir trees that cover the hills, and their chief was such a giant that his warriors could walk under his outstretched arms." The mountain exploded, and the people could not live near it for a long time. When they returned to the area, "the children, starved and weak for so long, never became tall and strong as their parents and grandparents had been."[350] Only a great chief, who could conquer the volcano spirit, would allow the people to become strong again. Deloria shows how this tradition appears to describe a condition of malnutrition which might be expected to occur if people were deprived of food for several generations. The same could be true of the megafauna, which, forced out of favorable living conditions as the ice age ended, became smaller and more like the animals that currently roam North America.

Deloria also showed how other Native American traditions invoked climatic explanations for the extinctions. For example, in one story about the bones at Salt Lick on the Ohio River, a Delaware chief recounts:

> In ancient times a herd of these tremendous animals came to the big-bone licks, and began a universal destruction of the bear, deer, elks, buffaloes, and other animals: that the great Man above, looking down and seeing this, was so enraged that he seized his lightning, descended on the earth, seated himself on a neighboring mountain, on a rock of

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[350] Deloria, Jr., 140.

> which his seat and the print of his feet are still to be seen, and hurled his bolts among them until the whole was slaughtered, except the big bull.[351]

Native American commentators took this to mean that the cause of these extinctions was climate, not hunting. Frustrated that their explanations were not often accepted forms of what had gone on in the past, many began to corroborate these myths with the geologic record and scientific findings, locating evidence for these more catastrophic climatic changes in the geologic record.[352] But science, they argued, was not the only way to know, and people should to take Native American ways of knowing seriously without having to test them by Western standards of truth.

As Native Americans critiqued overkill and scientific theories, they were expressing some of the strongest sentiments about the limits of scientific predictions. But Martin did not see their critique in this way. Soon after the publication of *Red Earth, White Lies* Martin responded to an interviewer's question asking if the dispute was primarily the result of a lack of data. Martin disagreed. Instead he argued that Deloria was condemning the overkill hypothesis for political reasons. As Martin explained:

> Vine Deloria's trashing of overkill reflects (in my view) his lifelong war against anything that he believes might reflect badly on Native Americans and/or upset those who do not believe that Native Americans were always here (in the New World). I doubt he was ever

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[351] Deloria, Jr., 128.

[352] George Nicholas, "When Scientists 'Discover' What Indigenous People Have Known For Centuries," *Smithsonian Magazine*, February 21, 2018; Rick Budhwa, "Correlations Between Catastrophic Paleoenvironmental Events and Native Oral Traditions of the Pacific Northwest" (Simon Fraser University, 2002).

> seriously interested in the extinctions during near time or in the problem of what caused them.[353]

This response seemed to result from a frustration from Martin, who thought that all evidence – from models, from proxies, and from the macrofossil and archeological record – all pointed to hunting as a cause of the megafauna extinction. He thought that unqualified people like Deloria, who were not interested in extinctions but only wanted to better the status of his people, perpetuated the debate far longer than necessary.[354]

Martin's continued understanding of the debate as being one about Native American rights meant that he missed the point. This was a debate about the status of scientific theories and the proper evidence used to support scientific predictions. Martin had modelled and thereby made a hypothetico-deductive prediction about what paleoecologists would find in the geologic record. The predictions resulting from models were different from prophecies because they incorporated evidence and used it with additional assumptions in a logical framework to make predictions that could be refuted by new evidence. With new developments in proxy evidence, Martin had initially found support for these predictions. But the models also included claims about human nature that could not be tested with this record and were the subject of much scientific and popular controversy. These problems undermined the overkill theory, especially

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[353] Amos Esty, "An Interview with Paul S. Martin," American Scientist, 2009, https://www.americanscientist.org/bookshelf/pub/an-interview-with-paul-s-martin.

[354] In Martin's 2005 book, he had presented different views. There, he said that blaming particular groups was not helpful. Instead he claimed that "the proposal that near-time extinctions in some critical way involve people, our species, *Homo sapiens*, requires at least a modicum of cultural sensitivity. Certainly no one can pass judgment, from long after the fact, on the peoples who first discovered and inhabited new lands. Their achievements are truly remarkable. It is one thing to note synchronicity of arrival of first pioneering prehistoric people in various corners of the planet and the concurrent extinction of many native animals; it is another to make a judgment. It would be absurd to assign blame to the progeny of Paleolithic Europeans or of the First Americans for the extinction of the Old World or New World mammoths, to Australian Aborigines for the end of the diprotodonts, or to the New Zealand Maoris for eliminating the moa. It is important to remember that the extinctions of near time occurred worldwide. To the extent that responsibility is assigned, it belongs to our species as a whole. This may be an even more disturbing thought for many." Martin, *Twilight of the Mammoths: Ice Age Extinctions and the Rewilding of America*, 54.

because the archeological record and Native American oral traditions posited different causes for the extinctions using different bodies of evidence. Thus, while Martin could make predictions that were compelling for many, he could only go so far until he over-stepped the evidence. Martin had once said that scientists “don’t prove things.” Instead, “what scientists do is test various best guesses or, to be blunt, bedtime stories that might explain the mystery.”[355] But critics were not asking for proof; instead they were wondering about the limits of bedtime stories about the past and future, a theme that continued as scientists began to engage with the issue of anthropogenic climate change, the next of the environmental disasters that encouraged paleoecologists to look to the future.

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[355] Martin quoted in Russell, *When the Land Was Young: Reflections on American Archaeology*, 45.

Chapter 4 – Proxies and Perceptibility: Ecologists' and Geoscientists' Climate Forecasts

In 1969, *Time* ran an article entitled "Ecology: The New Jeremiahs." This was only the third time the magazine had used the term ecology in print and the first time it defined ecology for its readers, even as it acknowledged that many would already be familiar with ecological ideas in an era of increasing ecological consciousness. The article began:

> There has not been a topic for such worried conversation since James Baldwin forecast the fire next time [in his 1963 book on race relations]. Suburban matrons predict the melting of the polar icecaps followed by catastrophic floods. Busy executives and bearded hippies discuss the presence of DDT in the flesh of Antarctic penguins. All sorts of Americans utter new words like ecosystem and eutrophication. Pollution may soon replace the Viet Nam war as the nation's major issue of protest.
>
> It is, in short, the year of ecology, a word derived from the Greek oikos, meaning "house." In modern usage, ecology is the study of nature's house or environment, including man's complex dependence on a bewildering variety of other creatures and life processes.
>
> Because of their grim warnings about man's environmental abuses, the once sheltered ecologists are turning into modern Jeremiahs. …Today's ecologists are scientists who know that all nature is interconnected and that any intervention has far-reaching effects. They are moved to action not only by considerations of beauty and sentiment but also by growing knowledge of the possibly disastrous consequences of unthinking intervention. The need for their expert opinions is being increasingly felt in

> Congress, the regulatory agencies and corporations, giving them an influence that promises to match or surpass that of the outspoken atomic scientists of the '50s.[356]

The article then included excerpts from interviews with prominent ecologists – including Barry Commoner, Eugene Odum, and George Evelyn Hutchinson – about the damage Americans were doing to the environment. Each spoke of the important role that ecologists played in combating environmental threats, even if they differed in the kinds of actions that should be taken.

The *Time* article, and others like it, promoted an understanding of ecology that, I have argued, had been developing since the beginning of the twentieth century: ecology, as it responded to environmental crises, was a future-oriented science that relied on ecologists' skills in interpreting natural processes.[357] Ecologists, the *Time* article claimed, understood the interconnections between natural processes and could predict the results of human intervention. Paleoecologists, in particular, had been making arguments about their ability to read the future through their engagement with proxy evidence and conclusions about the past since the Dust Bowl of the 1930s.

But paleoecology was not the only scientific discipline that gained perspective by relying on proxy evidence about the deep past and applying that evidence to the future. In the 1950s and 1960s, glaciologists, climatologists, and geophysicists began drilling into ice in search of evidence about past climates. Eventually, they were able to measure heavy oxygen isotopes found in layers of ice, which reflected changing climatic conditions over tens of thousands of years. Scientists also found volcanic dust, sea salts and ash from forest fires trapped in the ice, which they used as proxies for a variety of environmental conditions. They were also able to

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[356] "Ecology: The New Jeremiahs," *Time*, August 15, 1969.

[357] For another example of this perspective in the late 1960s and early 1970s, see "Paul Revere of Ecology," *Time*, February 2, 1970.

more directly determine the composition of the atmosphere at particular times in the past by measuring the concentration of greenhouse gases in bubbles found in ice cores. By the 1970s, ice core research had established itself as a useful method to establish past environmental conditions, particularly former climates. As anthropogenic climate change presented itself as threat by the 1980s, ice core researchers also came to use their proxy data in a similar way as paleoecologists: to make pronouncements about the future. In these ways, ice core analysis was a conceptually comparable science to pollen analysis.

In this chapter, I argue that proxy techniques, and the cultures that surround them, can color the way that scientists understand the targets of their studies. Proxies can alter study targets, in this case the climate, by mediating scientists' interactions with that target. To demonstrate this point, I compare paleoecological proxy work to climatological proxy work as anthropogenic climate change became an increasingly pressing concern towards the end of the twentieth century. This comparison reveals the way a target of study is mediated by various objects of study, in this case sediment and ice cores containing proxy data. This process of mediation helps to establish particular "regimes of perceptibility" that often locate the target in disciplinarily familiar ways.[358] In the case I present here, the mediation of the climate via vegetation assemblages and sediment cores in paleoecological proxy work enacts a regime of perceptibility that multiply locates climate in various physical manifestations of climate on earth, whereas climatological proxy work enacts a regime of perceptibility that locates climate primarily in the atmosphere. Moreover, locating climate in these different ways resulted in distinctively different forecasts of the future. Proxy methodology is therefore a relevant factor

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[358] Michelle Murphy's concept of "regions of perceptibility" tracks how disciplinary traditions establish what phenomena become perceptible and what objects are obscured. I use her concept as a framework for discussing how proxy techniques helped make the climate into a different target for different people. See, Murphy, *Sick Building Syndrome and the Problem of Uncertainty*.

when understanding how and why certain conceptions of scientific targets, and future predictions, are adopted by various communities. This chapter focuses on proxy work performed in the second half of the twentieth century in order to explore what conceptions of the future became visible in the particular stuff of science.

In what follows, I will show that some of the key differences in how paleoecologists and geoscientists perceived climate resulted from the ways they obtained and interpreted proxy data. In paleoecology, continued discussions about complexity and pollen methods led to hesitations about making definitive pronouncements concerning climate, whereas ice core researchers were much more willing to present their data as conclusive information on climate. Their proxies also connected them with climate in different ways: for paleoecologists, climate was based on vegetation communities that they had experienced, whereas, for ice-core workers, climate was temperature and composition of the atmosphere, which they may not have experienced directly.[359] Lastly, the process of obtaining ice or sediment cores led to different perceptions about whether engineering solutions or adaptation to changing conditions would best mitigate the problems likely to result from global warming.

Interpreting Proxies

As I showed in the last chapter, by the late 1950s, pollen workers had found a broad way to use pollen analysis to reconstruct past climates. Many paleoecologists were happy with their methods, which were codified in a number of textbooks from the late 1960s to the early 1980s

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[359] While Martin Rudwick has shown that renditions of paleo worlds rendered these environments very different than the present, most paleoecologists worked on the period since the last ice age, which they believed comprised familiar landscapes that had slowly resulted in today's environment. See, Martin J.S. Rudwick, *Scenes from Deep Time: Early Pictorial Representations of the Prehistoric World*, (Chicago: University of Chicago Press, 1992).

that would introduce a new generation to pollen analysis.[360] Yet questions still remained about how fossil pollen reflected past vegetation and climate, even after pollen workers had had done a half-century more work to improve the method. Even in a 2014 survey of paleoecologists who were asked to identify the key questions in paleoecology, over forty percent of the critical questions related to method, even after pollen workers had done more work to improve the method.[361] Beginning in the 1960s, Margaret B. Davis was a key part of efforts to understand how fossil pollen could be used to track past environmental conditions.

In the 1960s, when some of these discussions about methods were taking place, Davis was working in a research position at the University of Michigan. She had established herself as a leader in paleoecology after studying with prominent Scandinavian scientists and publishing an important paper on methods in 1963.[362] Her early methods paper grew out of a concern that the methodological assumptions underpinning pollen analysis were unwarranted. But, by 1969, Davis was arguing that "a new objectivity characterizes recent work [in pollen analysis], replacing the intuitive style that long made the method difficult to understand and evaluate."[363] For example, rather than leaving each pollen worker to determine how to correct for pollen that was overproduced (discussed more thoroughly in chapter 1), she suggested that finding modern pollen assemblages that matched a fossil pollen assemblage would allow paleoecologists to determine environments that gave rise to that pollen assemblage, thereby avoiding subjective

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[360] Faegri and Iversen, *Text-Book of Modern Pollen Analysis*; Erdtman, *An Introduction to Pollen Analysis*; Antoinette M Mannion, *Pollen Analysis : A Technique in Palaeo-Environmental Reconstruction* (Reading: Department of Geography, University of Reading, 1980); Peter D. Moore and J.A. Webb, *An Illustrated Guide to Pollen Analysis* (New York: Wiley, 1978); H.J.B. Birks and A.D. Gordon, *Numerical Methods in Quaternary Pollen Analysis* (London: Academic Press, 1985); Robert H Tschudy, *Aspects of Palynology.* (New York: Wiley-Interscience, 1969).

[361] Seddon et al., "Looking Forward through the Past: Identification of 50 Priority Research Questions in Palaeoecology."

[362] Davis, "On the Theory of Pollen Analysis."

[363] Davis, "Palynology and Environmental History during the Quaternary Period," 317.

correction methods. Yet, even as Davis expressed hope in new methods like these, she still thought that paleoecologists needed to do more to understand how pollen could trace the environmental conditions of the past. Many other pollen workers agreed with this assessment, which led to hesitations about making pronouncements on climate change. Worried about the uncertainty that arose from the various ways the characterization of climate was mediated in pollen work, paleoecologists routinely advocated for a better understanding of their proxy's relationship to climate before predictions could be made.

Davis' 1969 paper was one example of the continued discussions about how proxy work mediated ecological understanding of climate. Entitled "Palynology and Environmental History during the Quaternary Period," it aimed to show how palynology, as pollen analysis was increasingly being called, could provide a detailed record of vegetation and climate change. For the *American Scientist* readership, Davis described both the nature of the pollen record and the techniques that allowed paleoecologists to deduce ancient vegetation and climate from pollen in sediment. Both descriptions help reveal paleoecologists' epistemologies surrounding climate. First, in describing the pollen record, Davis explained how pollen found in sediment was quantitatively related to the vegetation of the surrounding region, but she repeatedly stated that the relationship was "complex." Seventeen years later, her former postdoc, Thompson Webb III, drove home how issues of complexity remained at the heart of the pollen record and its interpretation. As Webb wrote, "1) climate change is complex and its late-Quaternary history is incompletely known, 2) vegetational dynamics are complex and not fully understood, and 3) pollen data provide an imperfect record of vegetational dynamics."[364] Complexity talk often led Davis to propose more basic research that would allow paleoecologists to better understand their

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[364] Thompson Webb III, "Is Vegetation in Equilibrium with Climate? How to Interpret Late-Quaternary Pollen Data," *Vegetatio* 67 (1986): 75–76.

proxy record; she ended her 1969 paper by enumerating all the different areas of research that would help paleoecologists understand the complexity they encountered.[365] As we'll see later, this meant that Davis was much less likely to make predictions about future climates out of fear that paleoecologists did not yet understand the basics; she waited until 1989 to write a paper predicting how vegetation would respond to climate.[366]

The complexity of the mediating relationship between the object of study, pollen, and the target of investigation, vegetation and climate, led to Davis' discussion of the inferences pollen workers needed to make in order to make knowledge claims about the pollen record, the second focus of the 1969 article. She discussed ways that previous generations of pollen workers had understood this relationship and found fault with their methods, which directly interpreted pollen percentages. There were two methods of direct interpretation, each, she claimed, with its own problems. The first approach studied changes in vegetation from the percentages of each genus found in the various stratigraphic layers. Paleoecologists who used this method hoped that an increase or decrease in the percentages corresponded with changes in the abundances of the parent plants, thereby giving reasonable pictures of the changing composition of the forest. Davis, however, showed that pollen production rates were not uniform from year to year in a particular taxon, meaning that changes in fossil pollen percentages from one level to another would not convey a true impression of changes in vegetation.[367] The second approach, partially discussed in chapter 1, corrected for some of the problems in this first method by compensating

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[365] Davis, "Palynology and Environmental History during the Quaternary Period."

[366] Catherine Zabinski and Margaret B. Davis, "Hard Times Ahead for Great Lakes Forests: A Climate Threshold Model Predicts Responses to CO_2-Induced Climate Change," in *The Potential Effects of Global Climate Change on the United States*, (Washington, DC, US EPA, 1989): chapter 5; Margaret B. Davis and Catherine Zabinski, "Changes in Geographical Range Resulting from Greenhouse Warming: Effects on Biodiversity in Forests," in *Global Warming and Biological Diversity*, Robert L. Peters and Thomas E. Lovejoy (eds.), (New Haven, CT: Yale University Press, 1992): 297-308.

[367] Davis, "Palynology and Environmental History during the Quaternary Period."

for differences in production, dispersal, and preservation when paleoecologists built and interpreted pollen diagrams. But Davis warned that this method was also plagued by difficulties: pollen diagrams represent each pollen type in comparison to the other pollen types found in the sample. Pollen workers could apply correction factors that might divide the number of pollen grains of a pollen type by two for a type that was twice as well-represented in the sample. This correction method might not work for the whole diagram because of fluctuations in production, sometimes this type might be twice as well represented compared to its abundance in nature, whereas at other points it might be underrepresented in the fossil pollen. Paleoecologists would need to apply different corrective equations to different parts of their samples, which were themselves difficult to determine without knowing the parental vegetation. This problem was made all the more difficult when paleoecologists moved from pollen to vegetation to climate, where there were "few ways to test the actual relation between changes in pollen deposits and changes in climate."[368]

The problem that Davis was discussing was essentially one about the mediating inferences involved to turn pollen into data about climate. Several steps were required and, at each step, the relationship was complex and difficult to determine. These difficulties caused Daniel A. Livingstone, who had developed a coring device commonly used in pollen analysis, to say that:

> The conclusions of pollen analysis are primarily vegetational ones. Vegetation is a function of climate, to be sure, but climatic conclusions depend on additional inference

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[368] Margaret B. Davis to Dan[iel] [Botkin], November 11, 1982, Box 1, Correspondence, Alphabetical, A-I, 1984-1998, Margaret B. Davis Papers, Archives and Special Collections, University of Minnesota.

> and are correspondingly less secure. This is a serious disadvantage if one is primarily concerned with paleoclimatology.[369]

Livingstone was prepared to draw conclusions about vegetation from pollen analysis, but he was reluctant to move from pollen to climate, since the extra step inferring climate from vegetation assemblages was fraught with additional difficulties.

Researchers who studied the physical and chemical composition of ice cores had very different understandings of climate from pollen analysts, in part because the connection between their physicochemical elements and climate was relatively straightforward, especially compared to all the inferences in pollen analysis. Studying the physicochemical elements found in ice began in the 1950s. For example, in 1959, scientists from five European countries began a small-scale expedition to Greenland called the Expédition Glaciologique Internationale au Groenlande (EGIG).[370] One of the goals of the expedition was to "auger snow and firn [granular snow that has not yet been compressed to ice] cores en route to 10-20 meter depths aiming at physical chemical analyses."[371] Although analyses of the stable isotopes of the cores were not in the original plan, Danish climatologist Willi Dansgaard, who specialized in mass spectrometry, asked for access to the cores. After leaving the Danish weather service, where he had been a weather forecaster during World War II, Dansgaard had begun to use mass spectrometry to examine heavy stable isotopes in biology and medicine. In the early 1950s, he became particularly interested in isotope meteorology, where he examined the concentrations of the heavy water in various samples and found a relationship between temperature and the concentration of heavy isotopes. By the time EGIG was making its plans, Dansgaard had already

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[369] Livingstone, "Speculations on the Climatic History of Mankind," 333.
[370] Richard Finsterwalder, "Expédition Glaciologique Internationale Au Groenland 1957–60 (E.G.I.G.)," *Journal of Glaciology* 3, no. 26 (1959): 542–46.
[371] Willi Dansgaard, *Frozen Annals: Greenland Ice Sheet Research* (Copenhagen: Niels Bohr Institute, 2005), 30.

been to Greenland wanting to see how old water found in icebergs reflected the climate when the water had been formed. He could never be sure of the origins of the icebergs as they floated, so he hoped that data from glaciers would allow him to understand how prevailing meteorological conditions affected a particular place over time. He asked to be given access to the EGIG cores to perform his analyses and was granted permission. The samples turned out to be less finely resolved than Dansgaard had hoped, because the Paris center housing the cores had caught fire before Dansgaard was able to perform his analyses. Dansgaard had little more than meltwater in containers to work with, but he was able to begin to reconstruct past temperatures by measuring the isotopic composition of the water molecules.

To see how Dansgaard was able to estimate temperature from meltwater, a quick chemistry refresher is in order. Water, H_2O, comprises two hydrogen atoms and one oxygen atom. Each of these atoms has several different isotopes, atoms which have the same number of protons and are chemically identical, but which have different numbers of neutrons, which results in different masses. Four isotopes are particularly important to climate studies: ^{16}O (an oxygen atom with 8 protons and 8 neutrons, which makes up 99.76 percent of the oxygen in water), ^{18}O (an oxygen atom with 8 protons and 10 neutrons), ^{1}H (a hydrogen atom with one proton and no neutrons, which comprises 99.985 percent of the hydrogen in water), and ^{2}H (a hydrogen atom with one proton and one neutron, also known as deuterium, D, or heavy hydrogen). All of these isotopes are stable, meaning they do not undergo radioactive decay.

Using mass spectrometers, researchers can measure the ratios of the heavy to light isotopes of hydrogen and oxygen in the samples, and then compare them to a standard. This comparison ratio is called δ, and is measured in parts per thousand (per mil units). Based on the isotopic concentrations (δD and $\delta^{18}O$) relative to the standard, scientists can reconstruct past temperatures because, towards the poles, less ^{18}O and D is precipitated during cold periods compared to warm periods. It takes more energy to evaporate heavy water molecules from the ocean's surface so more heavy isotopes will be removed from the ocean and deposited in the Arctic and Antarctic as precipitation during warm periods, especially because the moist ocean air will be transported poleward and will preferentially lose the heavy water during precipitation. With the help of some of Dansgaard's earlier studies, which had revealed a consistent and linear relationship between δD and $\delta^{18}O$ and the surface temperature at middle and high latitudes, scientists could determine the mean temperature at various depths in their ice cores, inferred to be various moments in time.[372]

In the late 1950s, with these basic principles and five different cores taken during EGIG, some of which went back more than 50 years, Dansgaard reconstructed climatic records like the one in Figure 11. It showed temperature fluctuations through time. While this kind of ice core research was still in its early stages, Dansgaard's graphs show how geoscientists understood changes in the climate: as changes in temperature based on the chemical concentrations of molecules in the atmosphere. This idea was reinforced over the next 35 years as ice core workers realized that they could directly measure bubbles of gases like carbon dioxide and methane to determine the changing concentration of these gases in the atmosphere, and link these to climate.[373] This work helped to situate the climate in the atmosphere.

Dansgaard's work with these early ice cores also reveals a big difference between climate inferences from ice cores and those from pollen: the mediating chain of inference from ice cores to climatic conditions was relatively short. Once climatologists determined the relationship

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[372] Robert Mulvaney, "How Are Past Temperatures Determined from an Ice Core?," *Scientific American*, September 20, 2004.

[373] Dansgaard, *Frozen Annals: Greenland Ice Sheet Research*.

between temperature and the presence of isotopes, it was relatively easy to turn isotopic proxy data into conclusions about climate, especially compared to pollen analysis. For proxy data from ice cores, the relationship between $\delta^{18}O$ and temperature was nearly linear (see Figure 12). Not everything was perfect, with a number of sampling sites (the ones marked with crosses in Figure 12) not adhering to this linear relationship. Scientists had to do some work to understand why there might be outliers and how to correct for them. In this case, scientists came to dismiss the measurements from stations on Greenland's eastern slope, explaining that eliminating these data was appropriate because these stations received snow from the west, which lost heavy isotopes as the precipitation crosses Greenland's high-ice ridge. The snow from the other stations came from water vapor from the east, which would not have experienced this loss.[374] But, compared to pollen, where varied production, dispersal, and preservation could all muddle the relationship between pollen, vegetation, and climate, the relationship between isotopes and temperature was much clearer.

Because the mediating relationships between object and target were clearer, Dansgaard could say that his correlations between proxies and temperature variations might "need some correction, but the trends are undoubtedly correct."[375] Rather than declaring the accuracy of her pollen results, Davis was more likely to say that her new methods were leading to a better understanding of past conditions, but, in order to interpret the pollen record "in a sophisticated way we need to know more."[376] For pollen workers, there was often a sense that their methods needed further refinement before they could definitively interpret their organic proxy, a sentiment not expressed as often by ice core researchers.

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[374] Willi Dansgaard, "Stable Isotopes in Precipitation," *Tellus* XVI, no. 4 (1964): 436–68; Dansgaard, *Frozen Annals: Greenland Ice Sheet Research*, 36.
[375] Dansgaard, *Frozen Annals: Greenland Ice Sheet Research*, 36.
[376] Davis, "Palynology and Environmental History during the Quaternary Period," 331.

Obtaining Proxies

Coring techniques and operations also mediated the relationship between proxies and their target: this section argues that differences in coring techniques and operations between ice core workers and pollen workers solidified different perceptions of climate among geophysicists and paleoecologists. Ice core research was a large-scale undertaking, requiring careful coordination of engineers, scientists, and camp staff to ensure success in harsh conditions. Sediment coring for pollen was often much lower tech and a smaller operation; a team, usually those from the same lab, could spend a field season coring with hand-powered equipment. They gained a tangible sense of particular places and their climatic conditions during their fieldwork, whereas ice core workers gained a sense of mean temperatures and the difficulties of coring in difficult environments.

Soon after Dansgaard showed success interpreting the isotopes in short cores, he became part of a team working on taking a much longer core at Camp Century, an American military base in northwestern Greenland. Coring had started there as part of the 1957-1958 International Geophysical Year (IGY). During the IGY, the American team of researchers drilled several cores 300-400 meters in length, but they aimed to take much longer cores in order to have a longer climatic record. By 1966, scientists had obtained a 1387-meter core with the technical support of military assistants from the nuclear-powered “city under ice.”[377] This core contained ice, firn,

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[377] Martin-Nielsen, “The Deepest and Most Rewarding Hole Ever Drilled: Ice Cores and the Cold War in Greenland”; Martin-Nielsen, “The Other Cold War”; Aant Elzinga, “Some Aspects in the History of Ice Core Drilling and Science from IGY to EPICA,” in *National and Trans-National Agendas in Antarctice Research from the 1950s and Beyond*, ed. Cornelia Lüdecke, Lynn Tipton-Everett, and Lynn Lay, Proceedings of the 3rd Workshop of the SCAR Action Group on the History of Antarctic Research (Columbus, OH: Byrd Polar Research Center, 2011), 86–115; Langway Jr., *The History of Early Polar Ice Cores*; W. Dansgaard et al., “One Thousand Centuries of Climatic Record from Camp Century on the Greenland Ice Sheet,” *Science* 166, no. 3903 (October 17, 1969): 377–80; J. Jouzel, “A Brief History of Ice Core Science of the Last 50 Yr.,” *Climate of the Past* 9, no. 4 (2013): 2525–47; Janet Martin-Nielsen, *Eismitte in the Scientific Imagination: Knowledge and Politics at the Center of Greenland* (New York: Palgrave Macmillan, 2013).

and snow from the surface all the way to the bedrock. It too, comprised water isotopes, which caused Dansgaard and his colleagues to argue in *Science* that the ice core researchers now had "far greater, and far more direct climatological detail than any hitherto known method."[378] Within four years of drilling, scientists transformed the Camp Century core into a detailed database of earth's climatic history, with results published in leading scientific journals like *Science* and *Nature*.

Ice core operations took place in extreme environments. During the EGIG expedition that had yielded Dansgaard's early temperature measurements, two scientists lost their lives when they fell into a crevasse. The biographies of some of the key ice core scientists speak of harrowing storms, encounters with polar bears, and living in sub-zero temperatures with little access to supplies.[379] To work in these conditions, ice core researchers often describe feats of engineering and military coordination, since often only the military had planes that could fly in supplies to the coring locations. For the deep-drilling projects that began in the 1960s, this coordination allowed drillers to drill through the ice for ten or more hours a day, researchers to take initial measurements on the core processing line where temperatures averaged -20 degrees Celsius, camp staff to store the cores to prevent contamination or to feed hungry team-members, and military staff to transport everyone and everything to and from the research sites. The camp for the Greenland Ice Sheet Project 2 (GISP 2), which aimed to take a deep core from atop the summit of an ice sheet in central Greenland, eventually housed over fifty researchers, drillers,

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[378] Dansgaard et al., "One Thousand Centuries of Climatic Record from Camp Century on the Greenland Ice Sheet," 380.

[379] Paul Andrew Mayewski and Paul White, *The Ice Chronicles: The Quest to Understand Global Climate Change* (Hanover, NH: University Press of New England, 2002); Alley, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change, and Our Future*; Dansgaard et al., "One Thousand Centuries of Climatic Record from Camp Century on the Greenland Ice Sheet"; Jean Jouzel, Claude Lorius, and Dominique Raynaud, *The White Planet: The Evolution and Future of Our Frozen World*, trans. Teresa Lavender Fagan (Princeton, NJ: Princeton University Press, 2013).

and camp staff, who cored for the summer. Paul Mayewski, the director of GISP 2, talked about the “polar mindset” required when planning any coring expedition. This mindset would allow members of the team, who Mayewski already claimed were “tougher than the average person,” to determine “how to handle every contingency with what is at hand.”[380]

For the deep drilling projects, a special team of drillers undertook the task. Richard B. Alley, a scientist involved with the GISP 2 project, explained that the drillers’ work involved, “building towers, handling tons of supplies, fine-tuning an instrument big and strong enough to kill someone, and producing core that eventually totals forty tons and is of high enough quality to keep the pickiest scientists happy.”[381] As part of their daily operations, drillers needed to ensure that the weight of the ice did not squeeze the hole closed and that ice chips did not clog the drill. This required “brute force knowledge of mechanics and engineering… [to push] the drill ever downward season after season.”[382] It also required quick thinking to deal with the dangers of Arctic conditions: in high wind, the drill needed to be pulled out of the hole, but it risked blowing around at its own peril and threatened people below. As Alley described it, adding to the heroic persona of the ice core team member, the solution was to have a driller climb above the geodesic dome that housed the drill, strap a line to the drill, and then have the driller use their own bodyweight to counter-balance the weight of the blowing drill.[383] Ice core autobiographers use these kinds of solutions as examples of feats of engineering and ingenuity that allowed ice core work to go on in harsh environments with few supplies.

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[380] Mayewski and White, *The Ice Chronicles: The Quest to Understand Global Climate Change*, 57; 49.
[381] Alley, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change, and Our Future*, 28.
[382] Mayewski and White, *The Ice Chronicles: The Quest to Understand Global Climate Change*, 63.
[383] Alley, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change, and Our Future*, 29.

These narratives of engineering feats continue in other parts of ice core work. For instance, even once the core had been extracted, a "carefully choreographed dance" began.[384] Members of the team needed to store the cores and give them time to relax, sometimes for a year, otherwise the ice would shatter as it was cut into samples for analysis. This meant that the team had to build appropriate storage facilities out of the denuded landscape; as Alley described it:

> Storage was needed for cores waiting to be processed, and for cores waiting to be shipped home after processing. The whole operation had to be protected from the wind and sun – the drifting snow of the first storm would have buried anything left on the surface and the sun's warmth may have heated the ice closer to the melting point than we wanted.[385]

Once again, careful planning and engineering solutions helped to ensure that storage and processing were successful.

The engineering mindset even applied to dividing up the core itself, as researchers with a variety of interests, and different requirements about which pieces of core would provide them with the best results, all needed to be accommodated (see Figure 13). Some samples, for instance, needed to be contamination-free to be of any value. Others, such as those used for studying the electrical conductivity (ECM) of the ice, needed to be taken near the surface. Researchers had to figure out which methods and tools would give the best results across their wide range of interests, and engineer technologies that would accommodate these needs.

While this summary only begins to capture the work that went into taking and preparing an ice core, it serves to give a flavor of how ice core scientists described their work. These were "campaigns" or "expeditions," often in the tradition of the heroic polar expeditions of the past

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[384] Alley, 22.
[385] Alley, 24.

where people overcame the dangers and challenges of working on the ice.[386] Ice core expeditions were coordinated efforts that involved large teams, supported by the military, that sought to control the harsh and unpredictable conditions found on ice sheets and glaciers so that they could obtain good scientific results. While the degree of inferential mediation between an ice core and its target of study was relatively small, the processes of gathering ice cores heavily mediated the relationship between researchers and their objects of study, the consequences of which I'll examine in the next section.

As I alluded earlier, there was very little mediating the relationship between paleoecologists and their proxy objects. Taking a sediment core from a lake or bog was much easier than extracting an ice core. For one, paleoecologists were generally taking much shorter cores, often no longer than about 15 meters, since the vast majority of paleoecologists were interested in the period since the last Ice Age. In contrast, the longest ice cores were over 3 kilometers in length and extended back over 130,000 years. For these short sediment cores, a small team of researchers, who could often trek in all the supplies they needed, could take these cores using a hand-driven device. They could about their cores much more quickly, sometimes in a day or two at each field site.

Paleoecologists' coring tools and technique changed little throughout the twentieth and twenty-first centuries, and had only become more standard after the Livingstone proposed his sampling device in 1955.[387] For sediment cores, coring could be done by standing on "floating platforms the size of a picnic table" or, if "you're one of those winter coring people" by standing

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[386] Dansgaard began his autobiography with a description of the "daring polar explorers of old," and placed himself on a continuum with these early expeditions. Similarly, Mayewski described himself as the "fourth generation of polar explorers." See, Dansgaard, *Frozen Annals: Greenland Ice Sheet Research*; Mayewski and White, *The Ice Chronicles: The Quest to Understand Global Climate Change*, 28. On campaign metaphor, see Jouzel, Lorius, and Raynaud, *The White Planet: The Evolution and Future of Our Frozen World*.

[387] D.A. Livingstone, "A Lightweight Piston Sampler for Lake Deposits," *Ecology* 36 (1955): 137–39.

on the ice.[388] From the platform or frozen surface, University of Wisconsin-Madison graduate student Jacquelyn Gill described how she would bring "up meter after meter of sediment core from the bottoms of the small kettle lakes… watching the sediment transition from dark, organic-rich mud to the lighter silts and dense grey clays as the core segments get deeper – essentially time traveling via mud."[389] The visible layers in the cores immediately connected pollen workers with organic materials found in their samples because the color and quality of the sample could speak to past vegetation and other conditions. Gill also talked about embodied practices related to gyttja, the fine-grained, nutrient-rich organic mud that accumulates at the bottom of lakes and bogs. Putting gyttja on the tip of your tongue and placing it your teeth to feel for grit is a rough field test for silt.[390] Tasting their samples was another way to connect researchers to particular places and the geology, vegetation, and climate of the research site.

This lack of mediation between paleoecologists and the places and spaces of their studies is also evident in the tangible way they situated climate change within particular places. From the analog data at the heart of Davis' 1969 paper discussed above, Davis was able to compare the pollen assemblages found at various points in sediments cored in southern New England to modern pollen assemblages. From these data, she was able to say that, 8,000-9,500 years ago, New England probably had a climate like that of present-day northern Ontario. Twelve thousand years ago, the same spot in New England probably had a climate and vegetation much like that presently found in northern Quebec (see Figure 14).[391] In delivering her findings in this way, Davis was offering a tangible sense of the vegetation and climate and how they had changed; her

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[388] Jacquelyn Gill, "I Still Play in the Mud," *The Contemplative Mammoth* (blog), July 1, 2011, https://contemplativemammoth.com/2011/07/01/i-still-play-in-the-mud/; Jacquelyn Gill, "No Pollen Grains Were Harmed in the Writing of This Dissertation," *The Contemplative Mammoth* (blog), October 21, 2011, https://contemplativemammoth.com/2011/10/21/no-pollen-grains-were-harmed-in-the-writing-of-this-dissertation/.

[389] Gill, "I Still Play in the Mud."

[390] Gill.

[391] Davis, "Palynology and Environmental History during the Quaternary Period."

reconstructions corresponded to actual places people could experience: she was not just offering diagrams, which abstracted away from experiences with vegetation and climate, instead she was situating knowledge in place. This was a common practice in paleoecology; many papers showed how their site was once like some other site that people could visit.[392] In this way, paleoecologists were re-orienting climate away from the graphical and quantitative; instead, they were reconstructing landscapes that people could experience in all their complexity.

Even as paleoecologists gained first-hand knowledge of their targets while coring, coring was not always easy. The first generation of pollen workers had talked about storms, fierce mosquitoes, and challenges with piston corers that jammed or were inappropriate in certain samples.[393] Even almost a century after pollen workers first complained of these problems, some of these difficulties remained. For instance, in the comments on Gill's blog post about coring, someone named "Matt," who said he was trying to extract terrestrial cores without great expense (although great expense is relative here to the millions spent on ice core research), asked Gill to describe her coring techniques in more detail because he was having trouble extruding the collected cores from the core sampler. Gill replied that she generally did not have problems extracting organic sediments from her coring device; the stiff clays and sand layers from her sites could be challenging to remove by hand, but her team had found that they could use a winch for particularly stiff cores. She remembered only one case where various solutions that attempted to

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[392] See, for example, K. Gajewski, "Late Holocene Climate Changes in Eastern North America Estimated from Pollen Data," *Quaternary Research* 29, no. 3 (May 1988): 255–62; Margaret Davis et al., "Holocene Climate in the Western Great Lakes National Parks and Lakeshores: Implications for Future Climate Change," *Conservation Biology* 14, no. 4 (2000): 968–83; Margaret B. Davis, Ray W. Spear, and Linda C. K. Shane, "Holocene Climate of New England," *Quaternary Research* 14, no. 2 (September 1980): 240–50; Ronald B. Davis et al., "Vegetation and Associated Environments during the Past 14,000 Years near Moulton Pond, Maine," *Quaternary Research* 5, no. 3 (September 1975): 435–65; E.C. Grimm, "Chronology and Dynamics of Vegetation Change in the Prairie-Woodland Region of Southern Minnesota, U.S.A.," *New Phytologist* 93 (1983): 311–50.

[393] See, for example, A. Sharp to Paul B. Sears, June 26, 1928, Paul B. Sears Collection, MS 663, box 63, folder 854a, Manuscripts and Archives, Yale University.

extrude a core comprised of carbonate-rich mud, marl, and sand had failed. Gill had to abandon the core and shoot water up the barrel of her coring device with a super-soaker in order to be able to be able to continue to use it.[394]

Thus, while there were challenges coring for pollen, compared to ice core work these were much smaller operations which relied on much simpler technologies, and generally had much simpler work-arounds to get a sediment core. Further, since there was a much longer history of sediment coring and pollen work, as I describe in chapter 1, paleoecologists had developed standard coring devices, which required only minor adaptations to use in their field sites. This meant that when paleoecologists were going to take a core, they talked about doing "field work" or going on a "research trip," rather than going on an "expedition." This was not the daring science of ice core research that required feats of engineering, and a heroic autobiography; it was fieldwork that helped connect paleoecologists to particular places and the climatic conditions there.

Regimes of Perceptibility in Proxy Work

Navigating the various forms of mediation to successfully draw conclusions from different proxies helped bring about certain regimes of perceptibility that reinforced disciplinary perspectives in both ecology and climatology. As I have detailed above, the mediating relationships present in paleoecological proxy work spurred a regime of perceptibility in which climate was multiply-located but uncertainly known. The lack of mediation between scientist and their object of study helped encourage the perceptibility of climate within certain places and spaces, by specifying certain geographical regions as examples of how climate would have been, or will be. It also made climate into an almost tangible thing that could even be tasted, should

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[394] Gill, "I Still Play in the Mud."

one need to find silt in their sediment core. Yet, the large degree of inferential mediation in this proxy work cast doubts over how well climate could be perceived in these locations. Pollen was relatively easy to obtain, but hard to interpret, leading to reduced confidence that clear pronouncements could be made from these data. For paleoecologists, climate could only be broadly perceived by making connections to the climates of relatively familiar places.

In contrast, the means of mediating climate within ice core proxy work supports a different regime of perceptibility. Absent from this regime is much sense of place; the isotopes in ice cores helped situate the climate in the atmosphere by giving information that primarily indicates temperatures in the Northern Hemisphere. The climate, like the environment from which the cores come, is conceived of as somewhat distant and abstract.[395] Yet, given the more straightforward relationship between heavy isotopes and temperature, interpreting data from these cores was much easier: ice core workers made many fewer inferences and introduced far fewer correction factors, which led to more confidence in their results. Climate in this regime is better able to be perceived, at least in certain ways, than it is in the paleoecological regime. And while the regime of perceptibility promoted by ice core work locates the climate primarily in the atmosphere – which would be a familiar location for scientists like Dansgaard, who had training in meteorology – it makes invisible notions of climate that have biological and ecological consequences. As we will see in the next section, these different regimes of perceptibility promoted different interpretations of climate change once both pollen and ice core research came to focus more directly on anthropogenic climate change in the 1980s and 1990s.

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[395] Peter Gould-Rudiak, "'We Have Seen It with Our Own Eyes': Why We Disagree about Climate Change Visibility," *Weather, Climate, and Society* 5 (2013).

Perceptions of Climate Change

In the early 1980s, began to discuss how large-scale climatic change might affect the composition of vegetation communities. Her early findings and their relation to "the CO_2 problem" were initially purged from a report because the "concerns were a bit too new and, hence, shocking."[396] Plus, Davis was cautious about the conclusions and wanted more time to research to know that her results were well-grounded in the proxy evidence.

Davis often repeated her cautiousness, even a decade later when she had spent more time interpreting proxy records in relation to anthropogenic climate change. As evidence, consider this exchange between Davis, Bert Slager, and Sebastian Sprengers. Slager and Sprengers worked at the Institute for Environmental Studies in Amsterdam and wrote to Davis in 1994. By this time, Davis had published several papers trying to understand the effects of climate change on biological communities and had helped set the research priorities of the American contribution to the International Geosphere-Biosphere Programme (IGBP).[397] Slager and Sprengers wrote to Davis because they were hoping to develop a discussion paper which they would present at the Intergovernmental Panel on Climate Change (IPCC) meeting that fall. They were interested in determining the points at which species or ecosystems displayed a critical response to parameters like absolute temperature, temperature change over time, change in rainfall, sea-level rise, carbon dioxide concentration, and frequency of extreme weather events. By knowing these parameters, they hoped to identify ecological standards that would guide

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[396] Elliott A. Norse to Margaret B. Davis, January 8, 1981, Box 3, Correspondence and Information N, c. 1978-1981, Margaret B. Davis Papers, Archives and Special Collections, University of Minnesota.

[397] Margaret B. Davis, "Lags in Vegetation Response to Greenhouse Warming," *Climatic Change* 15, no. 1–2 (October 1989): 75–82; Margaret B. Davis and Daniel B. Botkin, "Sensitivity of Fossil Pollen Records to Short-Term Climatic Change," n.d., Box 3, Correspondence and Information D, c. 1977-1995, Archives and Special Collections, University of Minnesota; Committee on Global Change, *Toward an Understanding of Global Change: Initial Priorities for the U.S. Contributions to the International Geosphere-Biosphere Program* (Washington, DC: National Academy Press, 1988).

responses to anthropogenic climate change.[398] In response, Davis wrote that she believed that climate strongly limited the hardwood forest in northern Michigan, but she did not know "the exact climate factors that are involved. If I did, it would be much easier to predict what will happen in the future and it would be easier to understand what was happening in the past." She applauded their efforts to try to compile such information, but thought that there were very few systems in the United States "that are sufficiently well understood for us to specify climatic restrictions."[399] Davis instead thought that more work was needed to understand the exact relationships between ecological parameters and climate before she was willing to comment on critical turning points.

Yet, what is so interesting about this letter is that Davis identified a group of scientists who "are more optimistic" than her that critical climate parameters have been recognized. For example, she encouraged Slager and Springers to contact investigators at the Tree-Ring Laboratory at the University of Arizona as well as University of Oregon geographer Patrick Bartlein – who worked on environmental modeling and paleoclimatology – and bioclimatologist and vegetation modeller Ronald Nielson.[400] These modellers, Davis claimed, felt more comfortable predicting the future, signalling that she was aware of differences in how scientists might approach climate change, but preferred her own cautious approach.

When Davis was pushed to draw conclusions about the changing climate, she often described how her work revealed plants' adaptation strategies to anthropogenic climate change. For instance, in the mid-1980s, Davis was asked about the application of her research. She

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[398] Bert Slager and Sebastian Sprengers to Margaret B. Davis, May 31, 1994, Box 1, Correspondence, Alphabetical, A-I, 1984-1998, Archives and Special Collections, University of Minnesota.

[399] Margaret B. Davis to Bert Slager and Sebastian Sprengers, July 18, 1994, Box 1, Correspondence, Alphabetical, A-I, 1984-1998, Margaret B. Davis Papers, Archives and Special Collections, University of Minnesota.

[400] Davis to Slager and Sprengers.

explained that “by coming to know how trees, and which trees, survived – or perhaps which ones died away for a while only to be seen in the same place 5,000 or so years later, we can see what effect a change in the earth’s temperature – just 1 to 2 degrees – will have.” She went on to say that, with knowledge of how the climate would change over the next 100 or so years, “we can suggest which trees to cut down now as the earth’s temperature change. Then the young saplings of the species that will grow faster and are better adapted to the warmer temperatures will be able to grow and develop.” With her tree-cutting program, Davis was positing that some trees would not survive in a world modified by people so the best response the best response would be to proactively remove even seemingly healthy trees in order to promote an environment that could thrive under new conditions.[401] While this scheme was interventionist, Davis thought that she was mimicking past conditions and encouraging nature to run its course, rather than introducing new variables into a complicated system. Her perceptions of climate change, in other words, were strongly linked to the ecological systems through which she came to know the climate.

Other paleoecologists echoed this message about the need to adapt. Russell W. Graham at the Illinois State Museum ended one of his papers on paleoecological perspectives on conservation biology by writing “perhaps the most prophetic message paleoecology has to offer conservation biology is that we *must* plan to facilitate climatic change in the future. It is futile to assume that the species associations (“communities”) that we observe today and that we are trying to capture in our reserves will be the same over long spans of time.”[402] Instead, scientists should work to preserve species that could best adapt to these projected changes and put their efforts there, rather than seeking solutions that would preserve the status quo.

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[401] Brooke Porkmenn, [ca. 1985], Box 3, Correspondence and Information P, c. 1978-1983, Archives and Special Collections, University of Minnesota.

[402] Russell W. Graham, “The Role of Climatic Change in the Design of Biological Reserves: The Paleoecological Perspective for Conservation Biology,” *Conservation Biology* 2, no. 4 (December 1988): 393.

These solutions were quite difficult because they required knowledge of so many different facets of the environment and species' responses to change. For example, Davis needed to know the future climate, as well as the responses of trees to different climatic conditions, in order to carry out her solutions. But her solutions encompassed much more than climate futures and vegetation responses: they also included elements like soil, moisture, and politics. After explaining her solution to pre-emptively cut tree species not likely to survive in order to promote those that would thrive under new climatic futures, Davis explained further applications of her work, noting that transformations in ecosystems' climate and vegetation also had political consequences. As she noted:

> by studying the climatic conditions of thousands of years ago, we are able to see a pattern – when conditions are favorable in the United States' grain belt, it is dry in Africa, and vice versa. This must have a bearing on our continued economic and political relationships with other countries since the conditions won't always be as they are now. We should foresee the consequences of the changes.[403]

Basically, Davis, who had studied the effects of disturbance on vegetation communities, was attuned to the fact that climate change would have wide-ranging effects and could be compounded by factors external to climate. Her perception of climate change, informed by the situatedness of climate in ecological communities, would thus take more than just the atmosphere into consideration.

While it appears that someone like Davis was actively working on applying pollen analysis to solve practical problems related to climate change, a mid-1980s article on Davis and her work claimed that" most of the time…[she] is not drawing practical applications to modern

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[403] Porkmenn.

times. Davis is generally carrying out basic research."[404] She only began to publish her results in the late-1980s and 1990s, two decades after she began working on the problem.[405] This cautious attitude was consistent with her approach to pollen, which was difficult to work with, making conclusions and applications difficult.

Ice core autobiographies reveal that their writers were much more willing to think about the future. They routinely have sections entitled something like "an ice-core view of the future" or "future climates."[406] Often their discussions of the future suggested that making predictions was a difficult process, especially given the environmental and social systems involved. Some, however, felt very prepared to make predictions about how the ice would change in a warming world. They explored how the ice records would be lost as glaciers melted and explained how the sea-level might rise and the oceans' currents might shift.[407] They thus engaged with the future through the perspective of ice, especially temperatures and greenhouse gas concentrations. For example, French ice core workers Jean Jouzel, Claude Lorius, and Dominique Raynaud suggested that, "to look into the future, the climatologist needs, in addition to models, to know the way in which the composition of the atmosphere and its greenhouse effect, as well as other radiative forcings, those in particular connected to aerosols, will evolve."[408] This was a view about climate and future, driven mostly by atmospheric gases. Two things seemed to be at play in these kinds of analyses: the perception of climate created through ice core work, which helped

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[404] Porkmenn. As further evidence, in 1983, when asked to give a memorial lecture at Duke, Davis declined in order "to remain at home" to generate new data. She was working on computer simulations of vegetation responses to climate and wanted "to develope [sic.] this new area sufficiently to be able to talk about." See, Margaret B. Davis to Norman L. Christensen Jr., August 19, 1983, Box 3, Correspondence and Information C, c. 1977-1983, Margaret B. Davis Papers, Archives and Special Collections, University of Minnesota.

[405] Zabinski and Davis, "Hard Times Ahead for Great Lakes Forests: A Climate Threshold Model Predicts Responses to CO_2-Induced Climate Change;" Davis and Zabinski, "Changes in Geographical Range Resulting from Greenhouse Warming: Effects on Biodiversity in Forests."

[406] Alley, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change, and Our Future*.

[407] Jouzel, Lorius, and Raynaud, *The White Planet: The Evolution and Future of Our Frozen World*.

[408] Jouzel, Lorius, and Raynaud, 204.

situate the climate in the atmosphere, as well as the fact that early models focused more on atmosphere because these processes were easier to model.[409] With this understanding of climate, Jouzel, Lorius and Raynaud suggested that the struggle against climate warming would be based on controlling global greenhouse gas emissions.[410] This was how the United Nations (UN) Framework Convention on Climate Change, adopted in 1992 at the Earth Summit in Rio de Janeiro, understood climate as well. That document, eventually ratified by all member states of the UN, claimed that the objective is the "stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system."[411] Some IPCC reports also set mean temperatures rises, generally 2 degrees C, which they suggested should not be surpassed.[412] These solutions also understood climate futures in the regime of climatology.

Paul Mayewski, the senior scientist with the GISP 2 ice core team, was unhappy with these kinds of descriptions. He worried that too many people had used ice core work to focus on warming and cooling, and that global policies had been single-mindedly concentrated on factors affecting global temperatures, when there were many more consequences of environmental change.[413] This understanding of ice core research and change is certainly what historians Alessando Antonello and Mark Carey have found. They have indicated that, especially as policymakers and the media engage with ice core research, they adopt a form of climate

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[409] Paul N. Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming* (Cambridge, MA: MIT Press, 2010); Paul N. Edwards, "Representing the Global Atmosphere: Computer Models, Data and Knowledge about Climate Change," in *Changing the Atmosphere: Expert Knowledge and Environmental Governance*, ed. Clark A. Miller and Paul N. Edwards (Cambridge, MA: MIT Press, 2001), 247–86; Jouzel, Lorius, and Raynaud, *The White Planet: The Evolution and Future of Our Frozen World*, 202.

[410] Jouzel, Lorius, and Raynaud, *The White Planet: The Evolution and Future of Our Frozen World*, 273–74.

[411] United Nations, *United Nations Framework Convention on Climate Change*, 1992, 4.

[412] "IPCC, 2014: Climate Change 2014: Synthesis Report. Contribution of Working Groups I, II and III to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change" (Geneva: IPCC, 2014).

[413] Mayewski and White, *The Ice Chronicles: The Quest to Understand Global Climate Change*, 202.

determinism, which suggests that humans must merely maintain the temperature by controlling greenhouse gas concentrations, rather than adopt social and political programs that might shape policy.[414] The climate future, as perceived from ice cores, was firmly rooted in the temperature, especially compared to paleoecologists who found climate manifest in ecological systems, for which moisture extremes and seasonality are at least as important as temperature.

For ice core researchers, mitigating climate change or controlling greenhouse gases resulted from a belief about "human ingenuity" and the power of technology.[415] They certainly admitted that technology would not completely solve the problem, but their belief in technological and engineering solutions seemed similar to their belief that engineering would help them overcome the harsh conditions and allow them to do their scientific research. They perceived climate in the ice, temperatures, and greenhouse gases that were their research objects, and they perceived human capabilities controlling climate in the same way that they had seen these qualities in the processes that mediated ice core research.

Historians have argued that the earth sciences have disproportionate influence over how we understand nature and the climate.[416] This influence means that ice corers' regime of climate perceptibility, reinforced by climate modellers with a similar outlook, has had particular sway. But some of the literature on why we care about climate change, or why we don't, suggests that scenarios based on temperature and greenhouse gas concentrations are hard to picture and hard to care about.[417] They are too removed from the everyday to be tangible or worth doing something

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[414] Antonello and Carey, "Ice Cores and the Temporalities of the Global Environment."

[415] Mayewski and White, *The Ice Chronicles: The Quest to Understand Global Climate Change*, 210.

[416] Hulme, "Reducing the Future to Climate: A Story of Climate Determinism and Reductionism"; Clive Hamilton and Jacques Grinevald, "Was the Anthropocene Anticipated?," *The Anthropocene Review* 2, no. 1 (2015): 59–72; Ronald E. Doel, "Constituting the Postwar Earth Sciences The Military's Influence on the Environmental Sciences in the USA after 1945," *Social Studies of Science* 33, no. 5 (October 2003): 635–66.

[417] George Lakoff, "Why It Matters How We Frame the Environment," *Environmental Communication* 4, no. 1 (March 2010): 70–81.

about. Perhaps paleoecologists' regime of climate could resituate discussions of climate back in the tangible places that people know and love, in order to perceive what is needed to deal with the problem of anthropogenic climate change.

Chapter 5 – Prediction, Prophecy and the No-Analog Situation: "Unprecedented" Change and Paleoecological Methods

In 1979, Margaret Davis received a letter from her former post-doc Thompson Webb III. Webb was asking for help formulating a research plan to understand and combat the effects of global warming. Webb hoped to insert his vision for paleoclimate research into a report commissioned by the Department of Energy (DOE), which had recently become concerned about rising greenhouse gas concentrations and was willing to fund new projects to explore the impacts of these changes. In particular, Webb wanted to help Charles Cooper, a plant ecologist charged with writing the report's section on the natural biosphere, specify "how paleoclimatic data can be used as a predictive tool" for assessing the impacts of global warming. Webb thought that the report should "designate areas ripe for research in a 10- to 20-year research program" and highlight the ways that various proxies could provide information about diverse climatic and biological conditions in the past.[418] But Webb wasn't just interested in past conditions; he also wanted to use the warm periods of the past or previous periods of rapid climate change as analogs for the likely changes that would result from greenhouse warming. By knowing the past, Webb hoped to be able to predict the future.[419]

Webb's suggestions for how pollen analysis could be used to predict climate change, particularly his desire to use analogs from the past to predict what would happen in the future, demonstrated the ways that paleoecology had changed since its early days of prophecy. No longer would paleoecologists make subjective claims about the future using the past. Now, Webb

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[418] Webb had some success funding these projects. From 1979-1985, Webb received a grant from the Department of Energy Carbon Dioxide Research Program for a project on "Global Temperature Patterns 6,000 years ago." Along with W.L. Prell and J.E. Kutzbach, Webb also received funding for a project entitled "Model Validation Research: Comparison of Simulated and Observed Climatic Patterns for the Past 18,000 Years." The five grants for this project obtained between 1985 and 1998 gave this team an average of $440,000 a year to conduct this research.

[419] Tom Webb to Margaret [Davis], "Opportunity to Tell DOE about Paleoclimatic and Paleoecological Research That You May Be Later Funded to Do!," Memorandum, December 14, 1979, Box 4, Correspondence and Information, Tom Webb, 1973-1983, Archives and Special Collections, University of Minnesota.

was beginning to describe methods that would statistically determine whether particular vegetation assemblages or environmental properties from the past and future were similar. If they passed a quantifiable similarity threshold, scientists called the two assemblages “analogs” and used the better-known assemblage to determine the properties and dynamics of the lesser-known assemblage since they believed these analogs shared many features.[420]

By examining these analog techniques more closely, this chapter builds off the previous one. Rather than showing how pollen analysis reinforced certain conceptions of climate and climate change, this chapter characterizes the hopes and recognized limits of looking to the ecological past as a guide to the future. It begins in the 1980s, when pollen workers came to recognize that the climate was changing and that human actions were a contributor. Paleoecologists hoped to use the modern analog technique to anticipate these changes, but I argue that the technique was threatened by the very problem it had hoped to solve. As scientists recognized more clearly by the 1980s and 1990s, greenhouse gas concentrations were expected to be higher than those found during most historical periods. Scientists also predicted that greenhouse gas concentrations would lead to climatic shifts of a magnitude greater than many of the past.[421] Since vegetation communities were highly influenced by greenhouse gas conditions and climate, paleoecologists thought that they would encounter “no-analog situations,” moments

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[420] For an overview of some of the ways that analogs could predict the future, see T. Webb III et al., “Group Report: Use of Paleoclimatic Data as Analogs for Understanding Future Global Changes,” in *Global Changes in the Perspective of the Past*, ed. J.A. Eddy and H. Oeschger, Dahlem Workshop Reports, Envrionmental Sciences (Book 4) (Chichester, West Sussex, UK: John Wiley & Sons Ltd, 1993), 50–71.

[421] The Intergovernmental Panel on Climate Change (IPCC) published its first report in 1990. It noted that, if humans were changing the composition of the atmosphere, “several degrees” of warming might be expected by the mid-2000s. See J.T. Houghton, G.J. Jenkins, and J.J. Ephraums, eds., *Climate Change: The IPCC Scientific Assessment* (Cambridge, MA: Cambridge University Press, 1990). On the rise of awareness about anthropogenic climate change, see James Rodger Fleming, *Historical Perspectives on Climate Change* (New York: Oxford University Press, 1998); Spencer R. Weart, *The Discovery of Global Warming* (Cambridge, MA: Harvard University Press, 2008); Tim Flannery, *The Weather Makers: How Man Is Changing the Climate and What It Means for Life on Earth* (New York: Grove Press, 2005); Tim Flannery and Sally M. Walker, *We Are the Weather Makers: The History of Climate Change* (Somerville: Candlewick, 2009).

when the projected future was not expected to look anything like the changes or conditions experienced before.[422] No-analog situations threatened paleoecologists' predictions because knowledge of the past would no longer help scientists forecast the future to be different than anything ever experienced. This worry about the value of paleoecology in an unprecedented future was part of an ongoing debate about the role history could play in documenting what was to come.[423]

Paleoecologists responded to questions about the value of their field by reconfirming the role that proxy evidence and paleoecological reconstructions could play in environmental forecasts. Following discussions of no-analogs, some thought that proxies were better suited to validate and develop computer simulations, an emerging research agenda in the earth sciences in its own right by the 1970s. Since the 1970s, scientists had validated some of their computer simulations of past climates using climatic data from pollen, lake levels, and marine plankton, illustrating the emerging connections between paleoecologists and computer modelers.[424]

The COHMAP (Cooperative Holocene Mapping Project) is one of the best examples of these collaborations.[425] COHMAP was a multi-institutional effort led by John Kutzbach,

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[422] For a widely accepted identification of past no-analog situations, see J. T. Overpeck, T. Webb, and I. C. Prentice, "Quantitative Interpretation of Fossil Pollen Spectra: Dissimilarity Coefficients and the Method of Modern Analogs," *Quaternary Research* 23, no. 1 (January 1985): 87–108. For an early example of future no-analogs, see Webb III et al., "Group Report: Use of Paleoclimatic Data as Analogs for Understanding Future Global Changes."

[423] Gregg Mitman has explored some of these issues with the Anthropocene concept. See, Gregg Mitman, "Hubris or Humility? Genealogies of the Anthropocene," in *Future Remains*, ed. Gregg Mitman, Marco Armiero, and Robert S. Emmett (Chicago: The University of Chicago Press, 2018), 59–68. Other historians have written about how the Anthropocene and climate change have altered how they narrate history and engage with the future, see Dipesh Chakrabarty, "The Climate of History: Four Theses," *Critical Inquiry* 35, no. 2 (Winter 2009): 197–222; Libby Robin, "Histories for Changing Times: Entering the Anthropocene?," *Australian Historical Studies* 44 (2013): 329–40; Libby Robin and Will Steffen, "History for the Anthropocene," *History Compass* 5, no. 5 (August 1, 2007): 1694–1719.

[424] For an overview of development of some of these projects, see H. John B. Birks, "Holocene Cliamte Research - Progress, Paradigms, and Problems," in *Natural Climate Variability and Global Warming: A Holocene Perspective*, ed. Richard W. Battarbee and Heather A. Binney (Oxford: Blackwell Publishing Ltd., 2008), 7–57.

[425] CLIMAP (Climate: Long range Investigation, Mapping, and Prediction) was an earlier example of attempts to map climatic conditions during the last glacial period. The research project was funded by the National Science

Thompson Webb III, and Herb Wright Jr.. It brought together researchers at Brown, Columbia, Minnesota, Oregon, and Wisconsin, who would stimulate past climates at 3,000 year intervals over the past 18,000 years and compare the model simulations with paleoclimate data, particularly terrestrial pollen, lake-levels, pack-rat midden, and marine plankton data. The project began in 1977 as Climates of the Holocene – Mapping based on Pollen Data, and published numerous papers through the 1980s. The 1988 *Science* paper, "Climatic Changes of the Last 18,000 Years: Observations and Model Simulations" and 1993 book, *Global Climates since the Last Glacial Maximum*, were the most important.[426]

A number of paleoecologists, concerned that no-analogs situations limited their predictive capacity, thought that modeling projects might provide more information about the past and future than the analog method. The 1985 DOE report that Webb contributed to highlighted the importance of computer models, rather than analogs, as a way to understand the past because of a worry that the available modern analogs were not similar enough.[427] With more climatic changes expected in the future, as well as the increasing application of computer models in policy, scientists of all stripes thought that computer models, despite their problems, would provide better predictors of the future.

Foundation in the 1970s and 1980s as part of the International Decade on Ocean Exploration, which took place in the 1970s, It examined conditions across the oceans 18,000 years ago using a large number of sediment cores. While it primarily mapped ocean conditions, the CLIMAP project also produced maps of vegetative zones across the continents. See, CLIMAP, "Seasonal reconstructions of the Earth's surface at the last glacial maximum" in *Map Series, Technical Report MC-36,* (Boulder, CO: Geological Society of America, 1981).

[426] COHMAP Members, "Climatic Changes of the Last 18,000 Years: Observations and Model Simulations," *Science* 241, no. 4869 (August 26, 1988): 1043–52; Herb E. Wright Jr. et al., eds., *Global Climates since the Last Glacial Maximum* (Minneapolis: University of Minnesota Press, 1993).

[427] Michael C. MacCracken, "Carbon Dioxide and Climate Change: Background and Overview," in *Projecting the Climatic Effects of Increasing Carbon Dioxide*, ed. Michael C. MacCracken and Frederick M. Luther, DOE/ER-0237 (Washington, DC: United States Department of Energy, 1985), 2–23.

Most models, however, did not capture as many of the interrelated elements of ecosystems as the analog reconstruction methods.[428] Due to computational limitations, lack of data availability, and poor understanding of many properties of the earth system, most computer models could only capture some properties, usually climatic and atmospheric ones. Models thus helped, in the words of Mike Hulme, to reduce the future to climate, moving away from paleoecologists' vision of climate, discussed in chapter four.[429]

A second way that paleoecologists established the value of their field given the no-analog situation was by claiming that their field, in spite of major changes in the future, could offer "cautionary tales" about forecasted changes.[430] They could use historical data to warn about the future in two ways. First, the geological record became a reference point to demarcate the uniqueness of the current situation and anticipated future.[431] This sense of rupture was meant as a cautionary tale in its own right, warning against human modifications to the Earth in ways that made it unrecognizable, unpredictable, and potentially dangerous. They claimed that large-scale modifications might threaten ecosystem stability and industries reliant on vegetation and climate, and perhaps accelerate environmental and social problems. Second, paleoecologists could use history as a broad guide to the future. To do this, paleoecologists like Stephen Jackson and Jonathan Overpeck abandoned the sense that they could provide quantified predictions about the

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[428] For an example of those speaking of the deficiencies of modelling compared to reconstructions, see Martin I. Hoffert and Curt Covey, "Deriving Global Climate Sensitivity from Palaeoclimate Reconstructions," *Nature* 360, no. 6404 (December 10, 1992): 573–76.

[429] On this form of reductionism, see Hulme, "Reducing the Future to Climate: A Story of Climate Determinism and Reductionism."

[430] On the unprecedented nature of change and its effects on paleoecology, see Jonathan T. Overpeck, Patrick J. Bartlein, and Thompson III Webb, "Potential Magnitude of Future Vegetation Change in Eastern North America: Comparisons with the Past," *Science* 254, no. 5032 (November 1, 1991): 692–95; John W. Williams, Stephen T. Jackson, and John E. Kutzbach, "Projected Distributions of Novel and Disappearing Climates by 2100 AD," *Proceedings of the National Academy of Sciences* 104, no. 14 (April 3, 2007): 5738–42.

[431] Matthias Dörries has made a similar argument about how Paul Crutzen, one of the originators of the concept of the Anthropocene, used the deep past to show the uniqueness of the present and warn about the rapid rate at which humans were changing the face of the earth in comparison to slow geological change. See Dörries, "Politics, Geological Past, and the Future of the Earth," 33.

magnitude of climatic changes by relying on statistical similarities with the past, as the modern analog aimed to do. Instead, they began to claim that "history is better suited to providing cautionary tales rather than specific images of future climate and vegetation change."[432] By this they meant that history could be used to paint comprehensive future scenarios by relying on trends discerned from the past.

This chapter follows the reconfiguration of paleoecology in the 1980s, 1990s and 2000s in order to examine the changing uses of historical scientific data at the turn of the twenty-first century. Doing so helps to explain the purpose and limitations of the historical sciences as scientists applied these data to discussions of anthropogenic climate change. Tracing discussions about the application of paleoecological data also brings the story of the imperative for prediction in the earth and atmospheric sciences up to the present. Historians usually describe prediction as a dominant mode in the earth and environmental sciences by the 1970s, ending their story in the early 1980s when quantitative predictions reigned.[433] Historians Paul Warde and Sverker Sörlin depict this period as a time when scientists maintained "that the rates of change were not only knowable but predictable, and indeed, collectively so for many, many different things." Warde and Sörlin also go on to claim that "the possession of numbers became an integral part of a futures narrative, and one's plausibility in pronouncing it."[434] The search for modern analogs was a part of this tradition, with scientists expressing a strong desire to quantify the degree of similarity between sites in order to make quantified statements about change. But the no-analog situation made this kind of quantification and prediction impossible, suggesting

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[432] Jackson and Overpeck, "Responses of Plant Populations and Communities to Environmental Changes of the Late Quaternary."

[433] This literature includes includes Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970"; Oreskes, "Why Predict? Historical Perspectives on Prediction in Earth Science."

[434] Paul Warde and Sverker Sörlin, "Expertise for the Future: The Emergence of Environmental Prediction c. 1920-1970," 41.

that scientists' discussions of the future, and the ways their forecasts gained plausibility, needed to be framed differently in an era that scientists increasingly described as "novel" and "unprecedented."

This account also helps to explain why climate models rose to prominence. In the period that I examine, models had a number of problems, meaning that many scientists initially thought the modern analog technique was an equally viable way to forecast the future. That began to change in the mid-1980s. As historian Paul Edwards has argued, during that time, computer simulations were becoming more acceptable guides to the future, especially as they demonstrated their value for policy-relevant information on issues like acid rain, nuclear testing, and the formation of the ozone hole.[435] But, as I argue, the failure of other techniques to predict the future also helped elevate computer models to the point that the future had largely been reduced to computer simulations, rather than scenarios built with analogs.[436]

The Modern Analog Technique and the Past No-Analog Situation

Paleoecologists first developed the modern analog technique as a way of understanding the properties and dynamics of fossil vegetation communities, including their vegetation structure, carbon storage properties, and the effects of external forces. These properties were difficult to know from the proxy data alone, but scientists thought that, by resorting "to a form of inference in which two entities that are alike in some respects are assumed to be similar in other respects as well."[437] In this case they would make claims about fossil communities by relying on

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[435] Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*; Edwards, "Representing the Global Atmosphere: Computer Models, Data and Knowledge about Climate Change." As Clark A. Miller has argued, computer models also fit within the trend to globalize the environment in this period, see Clark A. Miller, "Climate Science and the Making of a Global Political Order," in *States of Knowledge: The Co-Production of Science and Social Order*, ed. Sheila Jasanoff (New York: Routledge, 2004), 46–66.

[436] Hulme, "Reducing the Future to Climate: A Story of Climate Determinism and Reductionism."

[437] Stephen T. Jackson and John W. Williams, "Modern Analogs in Quaternary Paleoecology: Here Today, Gone Yesterday, Gone Tomorrow?," *Annual Review of Earth and Planetary Sciences* 21, no. 32 (May 2004): 497.

their similarities to communities. The technique relied on finding a close match between a fossil pollen assemblage and a modern pollen assemblage whose dynamics were well understood. When paleoecologists found a match, they used the modern dynamics to infer the dynamics of the past assemblage, which, they posited, would closely mimic its analog.

While pollen workers had been making comparisons between present and past since von Post developed pollen analysis in 1916, the analog method really only became robust in the middle of the twentieth century. By then, paleoecologists had characterized modern pollen assemblages from a wide variety of environments in Europe and North America. They could use these assemblages as points of comparison for a wide variety of fossil pollen assemblages. Margaret Davis’ 1969 publication on palynological techniques, discussed in the last chapter, was part of this trend.[438]

With enough assemblages described to identify analogs, paleoecologists began using a form of the analog method in the 1960s, but, over about two decades, their practices changed. Initially, paleoecologists used an informal and subjective way of indicating that past and present were similar. In the 1960s, after establishing that some properties were similar, paleoecologists might just eye-ball two pollen assemblages to know that they were matches. Davis, for instance, did not offer many insights into why past assemblages from southern New England were analogs with an assemblage from present-day Ontario, rather than some other assemblage.[439] By the 1970s, concerned that their methods were too subjective to be reproducible, paleoecologists began to apply a variety of statistical methods to determine whether two profiles were analogous. They were aided in these efforts by the introduction of computerized algorithms for multivariate analysis, which scientists developed in the 1970s. These computer algorithms meant that, by the

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[438] Davis, “Palynology and Environmental History during the Quaternary Period.”

[439] Davis.

early 1980s, pollen workers had formal, numerical matching tools to statistically determine the similarity of fossil and modern pollen assemblages.[440]

Paleoecologists widely cite the 1985 paper, "Quantitative Interpretation of Fossil Pollen Spectra," as the first publication to show the power of what was increasingly called the modern analog technique. In this article, Jonathan Overpeck, along with his PhD advisor Thompson Webb III and British paleoecologist Iain Colin Prentice, published a comprehensive study of 1618 modern pollen assemblages in eastern North America. With this large data set, they performed numerical comparisons between pollen assemblages within and among various vegetation formations, such as tundra, boreal forest, mixed forest, and deciduous forest. These comparisons formed the basis of empirical thresholds that would indicate when sites were analogs. They called these thresholds dissimilarity coefficients, which offered paleoecologists numerical values that the coefficient must not exceed if the two assemblages were analogs. That is, Overpeck and his colleagues provided a "quantitative aid" to determine when the past vegetation could be said to resemble the vegetation associated with a modern sample.[441]

Paleoecologists' motivation for the analog method was to determine the properties of past communities that were difficult to determine through other means. These properties might include the vegetation structure and its composition. As Overpeck and his colleagues claimed, the modern analog technique allowed scientists to "reconstruct" the past with more detail than ever before.[442]

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[440] Jackson and Williams, "Modern Analogs in Quaternary Paleoecology: Here Today, Gone Yesterday, Gone Tomorrow?," 500. Other subfields were also developing a version of this method in the 1980s. In paleoceanography, W.H. Hutson developed the "analysis of modern faunal analogs" which reconstructed past sea-surface temperature from the remains of marine unicellular microplankton such as foraminifera, radiolarian, diatoms, and dinoflagellates. See, W.H. Hutson, "The Agulhas Current during the Late Pleistocene: Analysis of Modern Faunal Analogs," *Science* 207, no. 4426 (1980): 64–66.

[441] Overpeck, Webb, and Prentice, "Quantitative Interpretation of Fossil Pollen Spectra."

[442] Overpeck, Webb, and Prentice, 88.

While the analog technique could provide details about the past, Overpeck, Webb, and Prentice's 1985 paper also introduced the concept of "no modern analog."[443] No modern analogs were past communities that did not have a comparable modern assemblage because the genera present in the fossil community were arranged in unique ways.[444] The 1985 paper identified two no-analog situations. Both of these profiles, one from Minnesota 9,300 years before present and one from a part of Michigan 11,000 years before present, occurred during periods of change at the end of the last glacial period.

No-analog assemblages presented a problem for reconstructions. Since these communities did not have a modern counterpart, paleoecologists struggled to fill in the environmental conditions of these communities. The vast majority of the fossil record that Overpeck and colleagues examined, however, had analogs between past and present, meaning that the environmental reconstructions that paleoecologists hoped to undertake using the analog technique were possible. Given its promise, paleoecologists soon used this method to reconstruct paleoclimates and paleovegetation at regional and continental scales.[445]

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[443] In 1964 in the United Kingdom, R.G. West used macrofossils to suggest there were no modern analogs for some assemblages, see R. G. West, "Inter-Relations of Ecology and Quaternary Palaeobotany," *Journal of Animal Ecology* 33 (1964): 47–57.

[444] Scientists dismissed evolutionary phenomena as a cause of the differences between past and modern communities. Instead, paleoecologists claimed that the constituent species existing in no-analog communities could still be found today, but that these species had been reconfigured into communities not presently found. See, John W. Williams and Stephen T. Jackson, "Novel Climates, No-Analog Communities, and Ecological Surprises," *Frontiers in Ecology and the Environment* 5, no. 9 (November 2007): 475–82.

[445] See, for example, Rachid Cheddadi et al., "Holocene Climatic Change in Morocco: A Quantitative Reconstruction from Pollen Data," *Climate Dynamics* 14 (October 1998): 883–90; Davis et al., "Holocene Climate in the Western Great Lakes National Parks and Lakeshores"; Owen K. Davis and David S. Shafer, "A Holocene Climatic Record for the Sonoran Desert from Pollen Analysis of Montezuma Well, Arizona, USA," *Palaeogeography, Palaeoclimatology, Palaeoecology* 92, no. 1–2 (March 1992): 107–19; Gunnar Digerfeldt et al., "Reconstruction and Paleoclimatic Interpretation of Holocene Lake-Level Changes in Lac de Saint-Léger, Haute-Provence, Southeast France," *Palaeogeography, Palaeoclimatology, Palaeoecology* 136, no. 1–4 (December 15, 1997): 231–58; Michel Magny, Joël Guiot, and Patrick Schoellammer, "Quantitative Reconstruction of Younger Dryas to Mid-Holocene Paleoclimates at Le Locle, Swiss Jura, Using Pollen and Lake-Level Data," *Quaternary Research* 56, no. 2 (September 1, 2001): 170–80; Scott A. Mensing, "Late-Glacial and Early Holocene Vegetation and Climate Change near Owens Lake, Eastern California," *Quaternary Research* 55, no. 1 (January 2001): 57–65;

While paleoecologists had considerable success reconstructing past environmental conditions using Overpeck and his colleagues' analog method, they soon identified many pollen assemblages that lacked modern analogs. Scientists particularly found no-analog assemblages during the late glacial period, 17,000-10,000 years before the present, in eastern North America (see Figure 15). This area had well-documented modern fossil and pollen assemblages, meaning that the appearance of no-analog situations was unlikely to result from incomplete records.

At first, paleoecologists were distrustful of these no-analog situations. Harkening back to some of the issues discussed in chapter 1, paleoecologists initially thought that the no-analog communities identified in 1985 resulted from the mixing of pollen of different ages in sediments or the presence of pollen transported long distances. They were primed to dismiss no analogs as a problem with their technique rather than a natural phenomenon because they had initially thought that vegetation communities, especially those that had existed in the largely stable period since the last Ice Age, responded to environmental changes in concert; communities might shift to a new location in response to new climatic conditions, but they would remain a part of the same community. With that assumption as a basis, paleoecologists thought that they would likely find analogs; modern communities were just past communities whose distribution had changed slightly in response to environmental factors.[446]

By the 1980s, paleoecologists began to question their assumptions about species responses to environmental change. They started to claim that genera responded individualistically to change, which as I showed in chapter 4, made climatic reconstructions even

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Takeshi Nakagawa et al., "Quantitative Pollen-Based Climate Reconstruction in Central Japan: Application to Surface and Late Quaternary Spectra," *Quaternary Science Reviews* 21, no. 18 (October 2002): 2099–2113.

[446] Margaret Bryan Davis, "Address of the Past President: Insights from Paleoecology on Global Change," *Bulletin of the Ecological Society of America* 70, no. 4 (December 1989): 222–28. As we saw in chapter 1, Paul Sears worked in this way as he talked about the corn-belt shifting as a community. See, Sears, "Recent Climate and Vegetation a Factor in the Mound-Building Cultures?"

more difficult. The mounting sense of differential responses and lags led to a growing appreciation that no-analog situations were likely, and paleoecologists came to find more and more of these communities in the geologic record.[447] As they identified these past no-analog situations, scientists used a variety of words to describe these assemblages. They called no-analog fossil assemblages "compositionally unalike," "disharmonious," "mixed," "intermingled," "mosaic," or "extraprovincial" assemblages.[448] These words all emphasized the ways that various species had assembled and reassembled themselves in the past in ways that were different from the present. Yet, because scientists increasingly recognized that, over the past 18,000 years and longer, the climate and vegetation had gone through "continuous patterns of change," they viewed this reshuffling as part of a normal process of change.[449] These shifts were so common through geological time, that some scientists called the term climate change "redundant" because the climate was always shifting. Thus past no-analog communities, although compositionally unlike to modern communities, represented a normal environmental process. As my next section will show, this was not how scientists viewed the future no-analog situation.

The Future No-Analog as Rupture

In the 1980s and early 1990s, around the same time that paleoecologists began to describe many no-analog situations in the past, climate models began predicting that, due to

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[447] Davis, "Lags in Vegetation Response to Greenhouse Warming"; Margaret B. Davis, "Climatic Instability, Time Lags, and Community Disequilibrium," in *Community Ecology*, ed. Jared M. Diamond and Ted J. Case (New York: Harper & Row, 1984), 269–84.

[448] Williams and Jackson, "Novel Climates, No-Analog Communities, and Ecological Surprises," 477; Jackson and Williams, "Modern Analogs in Quaternary Paleoecology: Here Today, Gone Yesterday, Gone Tomorrow?," 501.

[449] Jonathan T. Overpeck, Robert S. Webb, and Thompson Webb III, "Mapping Eastern North American Vegetation Change of the Past 18 Ka: No-Analogs and the Future," *Geology* 20, no. 12 (December 1992): 1071.

human influence, the earth’s temperature was expected to rise by a few degrees Celsius.[450] With anticipated climate shifts and a sense that climatic factors were responsible for many no-analog communities of the past, scientists began to speculate that future no-analog communities would arise in these new climatic states.

At first, paleoecologists were excited about the prospect of using the past to forecast the future in a climatically altered world.[451] They had two approaches. One method relied on computer models, ably discussed by historian Paul Edwards, which would model the dynamics of key physical properties and verify the models using historical data.[452] The other method relied on analogs, of which there is no historical account. I focus there.

The analog method, as applied to the future, looked similar to the past analog technique, with a few variations. Since scientists were predicting that temperature changes and elevated greenhouse gas concentrations would dominate in the future, scientists looked for assemblages in the past that existed under the climatic and atmospheric conditions that they anticipated. For example, they could use the communities that resulted from higher temperatures during the middle Holocene warm period, 9,000 to 5,000 years before present, as an analog for communities that might result in a warmer world. Or they could use other periods of rapid warming, such as those experienced at the end of the Younger Dryas, the glacial period that lasted from about 20,000 to 11,700 years before present, as an analog for the rapid warming

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[450] See, for example, the first IPCC report, Houghton, Jenkins, and Ephraums, *Climate Change: The IPCC Scientific Assessment*. On the history of global warming, see Fleming, *Historical Perspectives on Climate Change*; Weart, *The Discovery of Global Warming*.

[451] For examples of this desire for prediction, see James S. Clark et al., “Ecological Forecasts: An Emerging Imperative,” *Science* 293, no. 5530 (2001): 657–60; Gretchen C. Daily, “Ecological Forecasts,” *Nature* 411 (May 17, 2001): 245.

[452] On the development of computer models, see Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*; Edwards, “Representing the Global Atmosphere: Computer Models, Data and Knowledge about Climate Change.”

anticipated due to global warming.[453] Or they could use the high carbon dioxide concentrations during the early Pliocene, about 5 million years ago, as an analog for forecast atmospheric conditions. No matter which analog they selected, these scientists thought that the analog technique was a promising method for determining the future since it could capture properties of the variables expected.

Some even thought that the analog method was an improvement over mathematical models that used equations to capture the fundamental physical principles governing earth systems. Critics of modeling thought that the models could not accurately predict regional patterns of future change.[454] There were too many uncertainties because of scientists' incomplete understanding of climatic mechanisms and because of their limited ability to represent the various earth system processes in computer models. Modelers had trouble representing things like clouds, precipitation, and surface hydrology, and they struggled to determine how these processes responded to changing climatic variables.[455] Given the limitations of models, some paleoecologists began to suggest that the analog technique provided a viable, alternative source for developing realistic scenarios of the future.

Yet, even as they were proposing ways to use analogs to study the future, these scientists began to question whether analogs could be predictively useful in any way. In the DOE report

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[453] Webb III et al., "Group Report: Use of Paleoclimatic Data as Analogs for Understanding Future Global Changes."

[454] Webb III et al., 52.

[455] For discussions of the limitations of climate models in contrast to the analog technique, see Michael C. MacCracken and Frederick M. Luther, eds., "Executive Summary," in *Projecting the Climatic Effects of Increasing Carbon Dioxide*, DOE/ER-0237 (Washington, DC: United States Department of Energy, 1985), xvii–xxv; W. Lawrence Gates, "Modeling as a Means of Studying the Climate System," in *Projecting the Climatic Effects of Increasing Carbon Dioxide*, ed. Michael C. MacCracken and Frederick M. Luther, DOE/ER-0237 (Washington, DC: United States Department of Energy, 1985), 57–80; Matthew C.G. Hall, "Estimating the Reliability of Climate Model Projections - Steps Toward a Solution," in *Projecting the Climatic Effects of Increasing Carbon Dioxide*, ed. Michael C. MacCracken and Frederick M. Luther, DOE/ER-0237 (Washington, DC: United States Department of Energy, 1985), 337–64. For a discussion of the history of climate models and their limitations, see Edwards, *A Vast Machine: Computer Models, Climate Data, and the Politics of Global Warming*.

that summarized state-of-the-art findings from a number of fields interested in climate and future change, including Webb's work which began the chapter, the editor of the report dismissed the analog technique. The editor, Michael C. MacCracken, a climate modeler at the Lawrence Livermore National Laboratory, dismissed analogs "because of limitations in data bases, incomplete understanding of the causes of past climatic changes, and an unprecedented rate of change of atmospheric CO_2 and trace gas concentrations." These problems meant that "no perfectly suitable analog is available," and the imperfect partial analogs differed enough that they could say little about the future. Given the lack of appropriate analogs, MacCracken argued that "theoretically based estimates will have to be the primary means for looking ahead."[456] For MacCracken, this meant that mathematical models were the way forward, not the analog method.

In the same report Webb and his collaborator, climate modeler Tom W.L. Wigley, were not as quick to dismiss the analog method. They were aware of the limitations of the analog method, namely that there were enough differences between the past and future that made finding true analogs difficult. These differences meant that the analog method could not

> reliably predict the details of future climate, which is why we deliberately use the word scenario here. Scenarios are not meant to be predictions of future climate; rather they are intended to be internally consistent pictures of a plausible future climate that provides a basis for other workers to evaluate the possible range of impacts of climate change on society."[457]

By speaking of scenario-building rather than prediction, Webb and Wigley were indicating that they thought that "past climate data was an analog for the future" because scenarios provided a description of possible future situations based on the analysis and understanding of current and

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[456] MacCracken, "Carbon Dioxide and Climate Change: Background and Overview," 12.
[457] Webb III and Wigley, "What Past Climates Can Indicate About a Warmer World," 243.

historic trends.[458] Yet, since they were talking about multiple scenarios, each plausible but for which it was difficult to know which was more likely, they were already signaling, by 1985, that the analog method might not be as powerful a predictive tool as they had hoped. [459]

This sense of difference between past, present and anticipated future became even more acute as climate modelers described anthropogenic greenhouse concentrations and climates as “geologically unprecedented.”[460] As MacCracken had stated in the 1985 report, “the combustion of large amounts of fossil fuel and emission of CO_2 into the atmosphere is an unprecedented geophysical event. We cannot expect the past record to provide an exact replication of the recent and projected climate change.”[461] By 1990, the Intergovernmental Panel on Climate Change (IPCC) had issued a similar pronouncement, adding to the perception that the future would have little in common with the past.[462]

With this language of rupture, scientists began to express the view that no-analog communities were very likely in the future. These future no-analog situations, and the future more widely, could only be partially understood, since paleoecologists would no longer have recourse to evidence from the past in which to ground the future. Margaret Davis made some of the problems of using the past to predict the future clear in her Past Presidential Address to the Ecological Society of America (ESA). Her speech aimed to determine “what insights have been gained from studies of the past that are useful to ecologists predicting the future?”[463] She drew four conclusions. Her first insight was that species responded to climate individualistically,

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[458] Webb III and Wigley, 241.

[459] Many climate modelers also came to suggest that they were scenario building as they recognized the complexity of earth systems and the limitations of their models. Examining their responses to how the emerging sense of future novelty affected their sense of the predictive capacity of their modeling techniques is beyond the scope of this paper.

[460] Williams, Jackson, and Kutzbach, “Projected Distributions of Novel and Disappearing Climates by 2100 AD.”

[461] MacCracken, “Carbon Dioxide and Climate Change: Background and Overview,” 17.

[462] Houghton, Jenkins, and Ephraums, *Climate Change: The IPCC Scientific Assessment*.

[463] Margaret B. Davis, “Insights from Paleoecology on Global Change,” *Bulletin of the Ecological Society of America* 70:4 (December 1989): 222.

rather than as a community, one of Gleason's critiques of Clements' organismic formation, discussed in chapter 1. This conclusion meant that the practice of predicting "the future by shifting existing communities or biomes around on the surface of the globe" was inappropriate because new and different ecosystems were likely to come into existence given individualistic responses to climatic changes. The second lesson was that biological responses to climatic change often occur with time lags, and, since there are differences in species' capacity to respond to the extraordinary climate change predicted, new communities would likely result as some species pull ahead while others lag behind. Third, "disturbance regimes are an aspect of climate that changes as climate changes," potentially amplifying the disturbance caused by global warming and making future predictions more difficult. Lastly, Davis claimed that multiple impacts were likely and that responses to climate would differ from those observable today. From this conclusion, she argued that:

> The message from paleoecology is clear. We cannot rely on analogy with the present to predict the future. To predict the future we need *functional* understanding of the responses of individual species to multiple impacts. Larger scale models will have to build on the reactions of individual species to make accurate predictions of communities, ecosystems, and finally landscape-scale biological systems.[464]

In making this argument, Davis was questioning the value of both analogs and models, as well as expressing a preference for more basic research before scientists moved to think about the future.

While Davis was making a larger argument about the predictive capacity of both modelling and analog techniques, from the 1980s to today, a number of people have come to

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[464] Davis, "Address of the Past President: Insights from Paleoecology on Global Change," 225.

specifically dismiss the analog technique, especially because of the no-analog situation.[465] Thomas Crowley was part of this contingent. In a 1990 article entitled "Are there any Satisfactory Geologic Analogs for a Future Green Warming?" Crowley, who had studied marine cores while he earned a PhD from Brown University and who became an expert in the analysis of past climates using data from a number of different proxies, answered his title question in the negative: past climates could not provide satisfactory analogs for future climates. To come to this conclusion, Crowley evaluated attempts to compare the predicted climate with various warm periods in the past, including the Holocene warm period, the last interglacial (120,000 years before present), as well as pre-Pleistocene warm periods such as the Pliocene (about 3-4 million years ago), the Eocene (about 50 million years ago), or the mid-Cretaceous (about 100 million years ago). Crowley thought that there were too many differences in the causes and manifestations of these geological warm periods and future ones to make them comparable. Scientists thought that the past warm periods were regional and seasonal, whereas they predicted that future ones would be global and would cause sustained rises in mean annual temperatures. For the older warm periods, there were also substantial differences between the earth's geography in these periods, which would influence important climate drivers like atmospheric and ocean circulation. Crowley also suggested that the high rate of temperature increase predicted in an anthropogenically forced world would lead to a "unique combination of warm atmospheres and polar ice sheets," conditions which would be very different from past warm periods. These differences caused Crowley to argue that the continued use of the term "analog"

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[465] Some scientists were suggesting that this problem would affect both the analog method and climate modeling, see Matthew C. Fitzpatrick et al., "How Will Climate Novelty Influence Ecological Forecasts? Using the Quaternary to Assess Future Reliability," *Global Change Biology*, 2018, 1–12.

for past warm periods and the future should be abandoned.[466] The so-called analogs would not actually be analogous with future conditions. This conclusion meant that, for Crowley and other who shared this position, the analog method could not say much about the future.

Paleoecologists described future no-analog assemblages differently than they described past no-analog situations. Future ones were called "emerging" and "novel," words which signaled discontinuity from that which came before.[467] Paleoecologists adopted this language in part due the perception that future climate change had unique and unprecedented anthropogenic causes. While change was constant and "kaleidoscopic" as multiple factors interacted to reconfigure earth systems, scientists increasingly recognized that human actions would strongly impact climate, playing a larger role than natural drivers.[468] This difference in the cause of change added to the sense that the future was distinct from the past. Paul Crutzen and Eugene Stoermer's proposal of a new geological epoch driven by human forcing, which they called the Anthropocene, reflects this growing sense of novelty.[469] As Gregg Mitman has argued, the Anthropocene traded in the language of rupture, leaving little place for history.[470] Since the analog method was a historical method, there was less and less of a place for this kind of historical science in a brave new world thought to be completely different from its past.

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[466] Thomas J. Crowley, "Are There Any Satisfactory Geologic Analogs for a Future Greenhouse Warming?," *Journal of Climate* 3, no. 11 (November 1990): 1282–92.
[467] Williams and Jackson, "Novel Climates, No-Analog Communities, and Ecological Surprises," 477.
[468] Jackson and Williams, "Modern Analogs in Quaternary Paleoecology: Here Today, Gone Yesterday, Gone Tomorrow?," 527.
[469] Paul J Crutzen and Eugene F Stoermer, "The 'Anthropocene,'" *Global Change Newsletter* 41 (May 2000): 17–18.
[470] Mitman, "Hubris or Humility? Genealogies of the Anthropocene."

The Place of History in Moments of Rupture

As scientists came to reject the analog method for determining the "unprecedented" future, they began to rethink the role of paleoecology and historical data.[471] Paleoecologists had long used the past to forecast the future and had increasingly thought that prediction would be possible. But, as a group of prominent scientists admitted after their 1992 workshop on analogs in paleo-climatic data, "the past is *a*, but not necessarily *the*, key to understanding the present and the future."[472] This section explores the role that the past would continue to play in understanding a future characterized by rupture. It argues that once paleoecologists came to realize that their data and methods had only limited predictive power, they began to emphasize paleoecology's role offering cautionary tales, rather than predictions.

As paleoecologists came to believe that novel climates would look dissimilar from much of the geologic record, they recognized the serious challenge these novel climates and ecological communities posed for forecasting. They often described this situation using the nautical metaphor of "sailing into uncharted waters," which emphasized how little they knew about the future, even as humans barreled into the unknown.[473] They wanted to understand these unknown seas, but had no data for it. As paleoeoclogists John (Jack) Williams and Stephen Jackson stated:

> As we sail into the future, we need to forecast what lies ahead. However, novel climates represent uncharted portions of climate space, where we have no observational data to parameterize and validate ecological forecasts. They are the climatic equivalent of

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[471] In other fields that looked at environmental change over long periods, scientists were asking similar questions about analogs and the role of the deep past in predicting the future. See, Russell A. Graham, "Quaternary Mammal Communities: Relevance of the Individualistic Response and Non-Analogue Faunas," *The Paleontological Society Papers* 11 (January 2005); Stephen T. Jackson, "Natural, Potential and Actual Vegetation in North America," *Journal of Vegetation Science* 24, no. 4 (2012): 772–76.

[472] Webb III et al., "Group Report: Use of Paleoclimatic Data as Analogs for Understanding Future Global Changes," 52. Italics in original.

[473] Crowley, "Are There Any Satisfactory Geologic Analogs for a Future Greenhouse Warming?"

> uncharted regions of the world, to which early European cartographers supposedly applied the label, ‘Here there be dragons.’[474]

With these metaphors, paleoscientists were expressing a desire for forecasts, along with a frustration that prediction was becoming increasingly impossible in a no-analog world.

These fears continued in the twenty-first century, when paleoecologists came to discuss the diminished predictive power of paleoecology. For instance, in their 2007 article on novel climates and no-analog communities, Williams and Jackson stated, “at worst, we may only be able to predict that many novel communities will emerge and surprises will occur.”[475] This proclamation emphasized a very different role for future-oriented paleoecology than a couple of decades earlier, when scientists thought that they would be able to quantify the magnitude and direction of change. Scientists came to claim that changes were likely, but the outcome of the changes was difficult to forecast. Given the minimal power that scientists thought paleoecological predictions would have in a world increasingly dominated by quantified predictions and unprecedented change, they reconfigured their field towards model verification and cautionary tales built from scenarios.

Paleoecologists, after lamenting the limitations on analogs in an era of unprecedented change, routinely stated that their data were still valuable, especially for testing climate models, something they had already been doing with these data since the 1970s and 1980s with the CLIMAP and COHMAP projects. As evidence of the greater role modelling would play in light of the no-analog situation, Overpeck, Webb III, and Webb published a paper in 1992. The paper claimed that there was a high likelihood that future trace-gas-induced climate change would lead to no-analog situations, meaning forecasts would be more successful if they relied on computer

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[474] Williams and Jackson, “Novel Climates, No-Analog Communities, and Ecological Surprises,” 480.
[475] Williams and Jackson, 479.

models rather than analogs. But, in the sentence immediately following this claim, they state "the record of past environmental change, however, provides information needed to improve these models and to test how well they can simulate observed changes in the past."[476] In their estimate, proxy data could be used for model construction and model assessment; paleoecology still had a large role to play.

In 2007, Williams and Jackson echoed these sentiments about the role of paleoecology amid unpresented change:

> Past no-analog climates differ from those we will encounter in the future, but they can be used to test the robustness of ecological models. Demonstrating the ecological models can accurately simulate past species distributions and community composition is necessary but not sufficient to impart confidence in future predictions.[477]

Basically, scientists hoped that their models would be able to predict no-analog communities in the past, in order to indicate that their models captured elements likely to be implicated in future no-analog communities.[478] If the climate models could simulate the observed changes in the past, scientists would gain more confidence that these models could also simulate future climate change and provide forecasts where analogs could not.

Models traded in a different kind of forecast, though. They often focused on a limited set of variables – usually climatic ones – whereas analogs, since they looked at the assemblage as a whole, could incorporate many more features of environmental change. Many analogs, for instance, captured vegetation and human actions in ways that most global circulation models did

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[476] Overpeck, Webb, and Webb III, "Mapping Eastern North American Vegetation Change of the Past 18 Ka," 1071.

[477] Williams and Jackson, "Novel Climates, No-Analog Communities, and Ecological Surprises," 480.

[478] For an example of some of these efforts, see Samuel D. Veloz et al., "No-analog Climates and Shifting Realized Niches during the Late Quaternary: Implications for 21st-century Predictions by Species Distribution Models," *Global Change Biology* 18 (2012): 1698–1713; Overpeck, Bartlein, and Webb, "Potential Magnitude of Future Vegetation Change in Eastern North America."

not.[479] By reducing the future to these models, a more limited picture of future environments was emerging, one not just focused on the atmosphere but also dominated by models.[480]

While some thought these models could make robust predictions, not everyone was convinced. Beginning in the 1990s, a number of scientists who regularly published on the limitations of models or analogs rejected predictive future-talk altogether. This talk continues today, with Matthew C. Fitzpatrick and his colleagues suggesting that “forecasts will be only slightly better than random” because earth systems were complex and difficult to understand and model.[481] These scientists preferred to say that historical data could be used as a “cautionary tale” or scenario building, not for prediction.[482]

In claiming that paleoeoclogical data and techniques could be used as a warning, paleoecologists were signaling that, unless widespread environmental change was minimized, a wide variety of changes was possible, changes that might drastically alter the composition of ecological *and* human communities. In these tales, paleoecologists spoke of the likely effects on silviculture, conservation, and other human practices.[483] Since they had admitted that quantitative prediction was likely impossible, they did not have to worry about how ecological factors and human factors might interact in the future: they were free to muse broadly about many different effects of potential changes, without worrying whether their models had the capacity to incorporate all the various elements. As Jack Williams said, “We live in a changing world – change is inevitable… Populations are growing worldwide. Climates are changing worldwide. Economies are changing worldwide. Our goal is to give people the best information about how

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[479] Huntley, “Quaternary Palaeoecology and Ecology.”
[480] Hulme, “Reducing the Future to Climate: A Story of Climate Determinism and Reductionism.”
[481] Fitzpatrick et al., “How Will Climate Novelty Influence Ecological Forecasts?,” 9.
[482] Jackson and Overpeck, “Responses of Plant Populations and Communities to Environmental Changes of the Late Quaternary.”
[483] Jackson and Overpeck; Overpeck, Bartlein, and Webb, “Potential Magnitude of Future Vegetation Change in Eastern North America.”

the world is changing, and why."[484] In this sense, late twentieth century and early twenty-first century paleoecology, even as it expressed a desire for quantitative predictions, looked more like the prophecies of paleoecologists in the early twentieth century: they were broad, unquantified pronouncements of potential scenarios if humans continued with their current practices. They incorporated human and ecological factors alike, in order to paint broad pictures of environmental and social change.

But, if historians have argued that numerical quantifications were the accepted way that people expected scientists to engage with the future by the 1970s and 1980s, what was the response to paleoecologists who moved away from prediction and back towards prophecy? Some readily accepted these new kinds of forecasts: there was increasing recognition that the future in a profoundly changed world was a wicked problem, a problem that is difficult or impossible to solve because of incomplete or contradictory knowledge because of the interconnectedness nature of the problem with other problems.[485] The complexity of the problem might mean that one day, scientists might be able to loosely model or understand the model. In the meantime, scientists could work to improve these models and understand environmental change. Even if scientists could not predict the future, the desire to know the future, in some sort of tangible way, remain strong. Cautionary tales and scenarios provide a sense of what is possible, filling in important gaps left as the analog method has lost favor, and as computer models have made forecasts that encompassed only a limited set of variables.

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[484] "Jack Williams," *Research University of Wisconsin-Madison* (blog), accessed May 31, 2018, https://research.wisc.edu/jack-williams/.

[485] In a 2007 conference paper and 2012 publication, scientists recognized that climate change may not be a wicked problem, but a super wicked problem. Super wicked problems shared the same issues as wicked problems, but have additional characteristics, including: time is running out; there is no central authority able to deal with the problem; those seeking to solve the problem are also causing it; and policies discount future irrationality. See, Kelly Levin et al., "Overcoming the Tragedy of Super Wicked Problems: Constraining Our Future Selves to Ameliorate Global Climate Change," *Policy Sciences* 45, no. 2 (June 1, 2012): 123–52.

Overall, these recent cautionary tales look similar to the prophecies that the early generation of paleoecologists had made during the Dust Bowl of the 1930s. In both cases, paleoecologists painted broad pictures of possible environmental and social harms that resulted from human actions. They also exhorted people to make changes that would minimize these harms. In the 1930s, paleoecologists had had to introduce ecology as a scientific field capable of managing the future and reducing the harms. Paul Sears had waited until the end of *Deserts on the March* to introduce the term ecology, thinking that most readers would be unfamiliar with the term, even if they were likely to understand some of its principles.[486] In the 2000s, paleoecologists rarely introduced basic concepts in ecology, knowing that many people already knew ecological principles that would make a difference. Yet they also knew that action on a problem like climate change was difficult, so recent ecological writing has served to warn people about a future filled with extinction, loss of habitat, decreased commerce, migrations, and unpredictable change. These warnings serve as calls to action.

With twenty-first-century cautionary tales, the prediction-prophecy pendulum is swinging away from prediction, which historians have held up as the main way that environmental scientists have engaged with the future since the 1970s. My findings refute this claim, suggesting that scientists have been forced away from prediction once they noted new limitations to how their data and methods could be applied to the future, especially a future understood as unprecedented. Prophecy, reframed as in terms of possible scenarios, better described their engagement with the future.

This story of changes in paleoecological forecasting at the turn of the twenty-first century reflects some of the broader themes in my dissertation. Throughout this dissertation, I have

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[486] Gene Cittadino, “Paul Sears: Cautious ‘Subversive’ Ecologist,” *The Bulletin of the Ecological Society of America* 96, no. 4 (October 2015): 519.

argued that historians' linear story of increasing quantification and prediction in the environmental and earth sciences doesn't fully capture the ways that the availability of data and techniques that would use the past to forecast the future influenced the nature of environmental forecasts. At times, these forecasts took the forms of prophecy, at other times prediction better described what paleoecologists were doing. With the no-analog situation projected for the future, paleoecologists have been moving back towards a form of prophecy because their data and methods could not support prediction. The question remained, however, whether anyone would heed the warnings of the latest generation of prophets.

Conclusion

This dissertation has covered pivotal episodes in the development of paleoecology, demonstrating how this field engaged with the future during moments of environmental crisis. These episodes reveal that proxy data, and the uncertainties surrounding conclusions drawn from these data, have had an enormous influence on the development of paleoecology. While proxy data were a source of uncertainty, they simultaneously provided a means by which paleoecology could become relevant to important environmental and social problems, including desertification, extinction, and climate change. Paleoecologists intervened in these disasters, offering predictions and prophecies about where our actions would lead us as well as alternative courses available to us. In this way, paleoecology was a science of the future.

In the three environmental crises this dissertation explores – desertification, extinction, and climate change – we see that paleoecologists' capacity to forecast, and the character of their forecasts, has been deeply tied to the proxy evidence paleoecologists replied on. I argue that paleoecologists' experiences collecting and interpreting biological proxies led to particular interpretations of phenomenon like climate, and climate change. They saw the climate as dependent on multiple interactions between the atmosphere, vegetation, and people, meaning that climate was most were visible in the complex communities found on earth. This finding suggests that scientific phenomena arise due to scientific data and its interpretation, an issue which I explore in each of the chapters.

In chapter two, I show that during the Dust Bowl of the 1930s, which represented the first moment when paleoecologists looked to the future, two different visions for paleoecology dominated. These visions were tied to how scientists read evidence from the past. One vision, promoted by Frederic Clements, emphasized prediction, the process of making quantitative forecasts of environmental futures similar to weather forecasts. Clements read history as a series of cyclical processes, which would form the basis of predictions. While he suggested that a predictive ecology had yet to be fully realized because natural cycles were not fully understood, he was able to show that one of the best-understood cycles, the sunspot cycle, could help predict rainfall, which would allow farmers to plant crops best-suited to the predicted conditions. Paul Sears advanced the other future orientation for paleoecology, calling ecology “the science of prophecy.” For Sears, there were few regularities on which to base predictions. Instead, the geological record offered other instances of desertification which the skilled interpreter could draw upon as a reference for the future. His prophecies encouraged practices that would restore the land, and he called people to adopt the strategies that had ensured the prosperity of other civilizations, rather than those that had led to their downfall.

The next environmental crisis – Cold War fears of nuclear annihilations, which paleoecologists related to mass extinction – became a moment when paleoecologists discussed the limits of their data again. They had originally set out to determine the drivers of the extinctions which took place at the end of the Pleistocene, particularly whether the extinctions were climate- or human-caused. A number of new proxies aided in these efforts, but still debate continued. I argue that part of the problem resulted from the ways that paleoecologists were using the geological record to discuss contemporary problems. Dust Bowl ecologists had shown that history offered a guide to the future, but Cold War ecologists were drawing out contested ideas about human nature and certain races of people from the geological record. Scientists and the public critiqued paleoecologists for this reading, suggesting that the geological record offered poor evidence for paleoecologists’ hypotheses. Concerns about evidence encouraged critics to argue that oral traditions might better reflect the past and, by extension, ways forward.

In my final case, climate change, I show that the availability of data limited paleoecologists as they sought to predict the future. Beginning in the 1980s, paleoecologists sought to use the analog technique to understand the effects of climate change, but they also recognized that, in an unprecedented future, they would find few analogous moments of change on which to base their predictions. I argue that they found new uses for history and proxies: proxies could be used to verify models, and history could be used to provide cautionary tales, even when quantified predictions were not possible. My findings thus suggest a need to pay more attention to the ways scientific data influence our visions of the future and engagements with environmental problems.

My discussion of the ways that forecasting, proxies and the authority of science have shaped discussions of desertification, extinction, and climate change only begin to scratch the surfaces of some important issues at play in the emergence of the earth and environmental sciences. Several directions for further research unfold. The first is the way that climate looms as a scientific object in the background of each of these environmental disasters, and in paleoecology more broadly. The founder of pollen statistics, Lennart von Post, had wanted to use the method to trace climate change, and many ecologists soon applied the method to understand climatic shifts in concert with the rise and fall of vegetation and civilizations. In this early work, climate determined the development of so-called “primitive peoples,” but, even after the rejection of climate determinism, it has creeped into subsequent discussions of environmental disasters.[487] How scientists over the course of the twentieth century have understood the ways that climate has shaped the lives of humans and non-humans in the past and to what degree

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[487] Paul Coombes and Keith Barber, “Environmental Determinism in Holocene Research: Causality or Coincidence?,” *Area* 37, no. 3 (September 2005): 303–11.

climatic determinism has emerged in current discussions about climate change are questions needing further exploration.

A focus on climate would also have to explore the fact that “climate” did not mean the same thing for each of the people working on environmental problems. I have made a start here by revealing that climate meant something different to pollen workers and ice core researchers. I have also shown that, in the early twentieth century, some ecologists saw the climate as regular and cyclical, while others viewed it as less consistent. Other historians have also posited that “climate” has meant something different through time, although most of the historiography has focused on periods before the twentieth century.[488] These differences suggest that there is a need to better understand the ways that climate becomes manifested materially through different methods, and whether the resulting notions of climate have changed over the twentieth and twenty-first centuries.

Lastly, my work has explored how paleoecologists engaged with the past and different notions of history, but it says little about how these ideas were received. The reception of ideas seems particularly important because it involves larger issues related to faith in scientific predictions, and the roles that history and science can and should play in articulating the future. Depending on which vision of history people accept, and the extent to which they subscribe to various future-oriented pronouncements, very different futures may emerge.

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[488] Theodore S. Feldman, “Climate and History in the Late 18th and Early 19th Centuries,” *Eos, Transactions American Geophysical Union* 73, no. 1 (January 1992): 1–5; Matthias Heymann, “The Evolution of Climate Ideas and Knowledge,” *Wiley Interdisciplinary Reviews: Climate Change* 1, no. 4 (July 2010): 581–97; Deborah R. Coen, “Big Is a Thing of the Past: Climate Change and Methodology in the History of Ideas,” *Journal for the History of Ideas* 77, no. 2 (April 2016): 305–21; Mike Hulme, *Why We Disagree about Climate Change: Understanding Controversy, Inaction and Opportunity* (Cambridge: Cambridge University Press, 2009), chap. 1.

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