PLANETARY TIMEMAKING

Paleoclimatology and the Temporalities of Environmental Knowledge, 1945–1990

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Academic Dissertation which, with due permission of the KTH Royal Institute of Technology, is submitted for public defence for the Degree of Doctor of Philosophy on Friday the 20th October 2023, at 2:00 p.m. in F3, Lindstedtsvägen 26, Stockholm.

Doctoral Thesis in History of Science, Technology and Environment
KTH Royal Institute of Technology
Stockholm, Sweden 2023
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Abstract

This thesis concerns the history of paleoclimatology in the postwar period. It follows the trajectory of two climate proxy records – ice cores and deep-sea cores – in the North Atlantic region, from their emergence as scientific objects in the 1940s to their incorporation into Earth System Science in the 1980s. In doing so, the thesis highlights how scientists have used these records to produce time and how this has affected the temporalities of the environment as a scientific and political concept. As anthropogenic impact on the planet arose as a political problem, the vast timescales of paleoclimatology became increasingly interwoven with climate modeling and environmental policy. Often, the records were understood as “natural archives”, providing unmediated access to past environmental conditions. This thesis shows how the times of ice- and deep-sea cores were, on the contrary, constantly re-shaped by scientific practices, institutional frameworks and competing temporal sensibilities in different scientific fields.

With the rise of the Anthropocene concept in the last two decades, historians have begun to question the division between historical and natural times. Scientifically produced times, such as those made by ice- and deep-sea core drilling, have permeated discussions about the properties of historical time in the twenty-first century. Yet, the origins of these times often remain out of view. This thesis argues that environmental historians and historians of science can contribute to these discussions by treating environmental times as objects of historical inquiry. Looking beyond planetary-scale models and following environmental times in situ, as they emerge, form and travel, can open up for more multi-faceted approaches to historical time in the proposed new geological epoch. The history of postwar paleoclimatology is therefore both a history of how scientists have produced environmental times and how planetary pasts enter the political present.

Keywords: paleoclimatology, environmental history, temporality, ice core, deep-sea core, North Atlantic, timemaking, theory of history, planetary-scale environmental knowledge
Acknowledgements

It is often said that writing a doctoral thesis is a lonely job. To some degree it inevitably is. But mostly it is not. The many conversations I have had, during the years working with this thesis, have been the greatest joys of this project. I am indebted to countless colleagues, institutions and friends for their support and contribution to this thesis.

Thank you first of all to my supervisors Sverker Sörlin, Sabine Höhler and Adam Wickberg. Sverker, my main supervisor, has been an unfailing source of inspiration and guidance since I first began this project. By challenging me to think big and to try to push beyond what we already know, he has been a constant reminder of why this academic life is worth pursuing, and why it is important. I am deeply grateful for Sabine’s support, generosity and ability to provide the sharpest comments when I have needed them the most. Adam joined the supervision team a little later, but I am so glad for his insights – about everything from media theory to life in academia – during the last two years of this project.

This thesis is part of a larger research project, which is generously funded by the European Research Council: SPHERE (Study of the Planetary-Human Environment Relationship). Being part of this research community has not only shaped the content of this thesis, but also provided a space for wide ranging discussions, collaboration and friendship. Thank you especially to my SPHERE doctoral student colleagues Jasmin Höglund Hellgren, Gloria Samosir and Thomas Schroeder and SPHERE postdoc Eric Paglia.

The Division of History of Science, Technology and Environment at KTH has been an academic home for me during these years. It is a remarkable research environment, and that is of course because of the people that are part of it. Thank you to the doctoral student group: Achim, Siegfried, Alicia, Liubov, Klara, Erik, Araujo, Domingos, Ulrika, Camilla, Jean-Sébastien, Dmitry, Jesse, Corinna and Daniele. Thank you also to current and former Division colleagues. All of you have, in different ways, shaped this project to what it is. Thank you to Nina Wormbs for teaching me what academic collegiality is all about. Thank you to Sofia Jonsson who has sorted out the most complex webs of KTH bureaucracy while giving me the best literature tips at the same time. Thank you also to Miia Jylkää and Emilia Rolander for all your help.

I had the pleasure of spending a few months at the Centre for History and Economics at the University of Cambridge. Thank you to Paul Warde who made this possible, and for your always wise input – both over coffee in Cambridge and over Zoom in our SPHERE
meetings. I am also grateful to Inga Huld Markan for helping me with all the practicalities and the Environmental History Reading Group and the Centre for History and Economics Graduate Workshop for letting me be a part of your academic community.

Thank you to Emil Flato for being a brilliant colleague in the time business, and for your friendship; to Helge Jordheim and Dania Achermann who provided me with much needed guidance at my mid- and final seminars; to Richard Staley for the careful review of this manuscript; to Victoria Höög for opening the door to the world of research; to Leonoor Zuiderveen Borgesius, Johan Gärdebo, Staffan Bergwik, Anders Ekström, Susanna Lidström, David Larsson Heidenblad and Perrin Selcer for insights, comments and conversations through these years; to my friends for their support and for reminding me that there is a world beyond the thesis. Many others have generously given of their time to share their knowledge, by asking a question at a conference, locating archival material or engaging in discussions at workshops, seminars and after work beers. I am grateful to all of you.

Tack till min familj – min bror Per och mina föräldrar – för att ni finns där för mig, och för att ni alltid har uppmuntrat mig att våga tro på mina idéer.

Tack till Julia, min älskade.
Acronyms and abbreviations

BARK – Binär Automatisk Relä-Kalkylator
CLIMAP – Climate: Long range Investigation, Mapping, and Prediction
CORE – Consortiums of Oceanic Research and Exploration
CRREL - Cold Regions Research and Engineering Laboratory
DSDP – Deep-sea Drilling Project
EGIG – Expédition Glaciologique Internationale au Groënland
ESS – Earth System Science
ESSC – Earth System Science Committee
GARP – Global Atmospheric Research Program
GCM – General Circulation Model
GEOSECS – Geochemical Ocean Sections Study
IDOE – International Decade of Ocean Exploration
IEA – International Energy Agency
IGBP – International Geosphere-Biosphere Program
IGY – International Geophysical Year
IPCC – Intergovernmental Panel on Climate Change
JOIDES – Joint Oceanographic Institutions for Deep Earth Sampling
LDGO – Lamont-Doherty Geological Observatory
LOCO – Project Long Cores
MODE – Mid-Ocean Dynamics Experiment
NAC – NASA Advisory Council
NASA – National Aeronautics and Space Agency
NASCO – National Academy of Sciences Committee on Oceanography
NCAR – National Center for Atmospheric Research
NOAA – National Oceanic and Atmospheric Administration
NSF – National Science Foundation
OECD – The Organization for Economic Co-operation and Development
ODP – Ocean Drilling Program
SMIC – Study of Man’s Impact on Climate
UCAR – University Corporation for Atmospheric Research
UNESCO – United Nations Educational, Scientific and Cultural Organization
WMO – World Meteorological Organization
ØK – Det Østasiatiske Kompagni
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5.4 The Bretherton Diagram. Earth System Science Committee, Earth System Science: A Program for Global Change, 24-25.

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On November 23rd, 1948, a somewhat odd group of scientists gathered in a lecture hall at the Zoophysiological Laboratory at the University of Copenhagen. Niels Bohr, the Nobel Prize winning physicist was there, as was the Nobel Prize winner in medicine August Krogh and 1943 Nobel Prize winner in chemistry, George de Hevesy. Denmark’s state geologist Johannes Iversen and the geophysicist Willi Dansgaard were present as well. In total 69 scientists from fields such as physics, chemistry, geology, medicine, biology, and engineering sat gathered in the room. They did not normally meet up in this way.

The scientists had all been invited to the first ever Isotope Colloquium, an interdisciplinary seminar initiated by the physicist Hilde Levi. She had earlier in 1948 returned from a stay at the University of Chicago where she had studied isotope dating techniques with the geochemist W. F. Libby. Even though many of the scientists present at the colloquium were not primarily interested in geochemistry, the prospects of isotope dating had not passed them by. They worked with vastly different materialities: ice, sediments, pollen, archeological remains and even human bodies, but they were all, in some way and on different scales, interested in changes unfolding over time. By comparing the presence of a radioactive isotope within a material to its known abundance on Earth as well as its half-life, it had become possible to accurately date the previously unknown age of organic materials.  

1 Details of the meeting are described by Torkild Andersen in Datering af fortiden: Om det første danske kulstof-14 laboratorium (Aarhus: Aarhus Universitetsforlag, 2007), 39-40.  
2 Isotopes are any of two or more kinds of atoms of a particular chemical element that has the same atomic number and close to identical chemical behavior, but that does not share atomic mass. This tiny difference in atomic mass opened the door to comparative studies. The carbon-14 method, which Libby was part of inventing, is the most famous example of how this dynamic could be utilized scientifically. It uses the $^{14}$C isotope – which has six protons and eight neutrons in its nucleus – and the stable $^{12}$C – which has six protons and six neutrons – to make a comparison possible. The two additional neutrons in $^{14}$C make it so unstable that it decays radioactively over time, while $^{12}$C remains the same. When an organism dies, it ceases to absorb carbon which, Libby realized, meant that its existing $^{14}$C would begin to decay. By measuring an organism’s radioactivity, its $^{14}$C could be estimated and compared to its $^{12}$C, which in turn enabled the measurement of its time of death. For a history of the carbon-14 method, see: John F. Marra, Hot Carbon: Carbon-14 and a Revolution in Science (New York: Columbia University Press, 2019).
now, finally, the means to make these times visible had come into being. At the time of the first colloquium in 1948, the exact ramifications of this new technology was still unclear, but it appeared to open up new temporal dimensions in well-known scientific objects.

At the interdisciplinary Isotope Colloquium – a total of 15 were held between 1948 and 1951 – an elite group of Copenhagen scientists brought with them their own studies and materials, in order to elaborate on their temporal qualities. Isotope dating methods were used on hazelnuts, chunks of wood, pine cones, sea shells, peat bogs, and many other objects. Over time, their scale grew: by the 1950s, isotope dating was used to study atmospheric circulation and how the exchange of carbon dioxide between the atmosphere and the oceans had evolved historically. Sediments from the ocean floor and ice cores from the cryosphere were at the same time emerging as new technologies that could access a far deeper climatological history. New questions began to emerge too: could these methods and objects be used to track how the Earth’s climate had changed in the past? How did these different times relate to one another? Behind these questions loomed another, even larger question: could human activities affect the processes that science just had made visible?

At the 1948 gathering in the Zoophysiological Laboratory in Copenhagen, the 69 scientists were not at all concerned with the environment. The term itself had barely begun to circulate in society, and the utility of isotope dating did not at the time seem to stretch into the domain of tracing human impact on the planet. But things were about to change. At the same time as the scientists were gathering in Copenhagen, the notion of a fragile global environment was being discussed in books such as William Vogt’s The Road to Survival and Fairfield Osborn’s Our Plundered Planet, both released in 1948. These books marked the beginning of a particular style of environmental imaginary which conceived of the Earth as interconnected, vulnerable, and impacted by human activity.

These two phenomena – the temporalization of natural objects and anthropogenic impact on the planet – would in the decades that followed become increasingly intertwined.

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3 Carbon-14 is the most famous, and arguably also the most important isotope dating method invented at this point in time, but other dating methods that utilized stable oxygen isotopes rather than radioisotopes would become increasingly important in paleoclimatological research. E.g. Willi Dansgaard, “The Abundance of O^{18} in Atmospheric Water and Water Vapour,” *Tellus* 5:4 (1953): 461-469.


One crucial context in which this encounter would happen was the field of paleoclimatology, the study of changes in the climate over long periods of time, which drew on both isotope dating and new technologies from the geosciences, such as ice and deep-sea cores. In ways that no participant at the Copenhagen colloquium foresaw, the times they were studying would begin to intersect with the times of human history and environmental politics.
In this thesis, I aim to explore how the scientific production of time has affected the way the environment is understood scientifically, politically and socially. Time and temporality are treated as the central categories through which the rise of planetary-scale environmental knowledge can be conceptualized as a historical process. The thesis specifically considers the history of postwar paleoclimatology, and the rise of ice- and deep-sea core drilling, as a history of planetary timemaking. Measuring and conceptualizing how the planet had changed over time was of course nothing new to the postwar era. Geologists and geophysicists had known for decades, even centuries, that the Earth was a dynamic and occasionally rapidly changing planet. Yet, postwar paleoclimatologists would enable different temporalizations of the planet in comparison with their pre-war predecessors. They were interested in how climatological and environmental conditions had changed over vast periods of time, but, as anthropogenic impact on a planetary scale became an increasingly pressing issue, they had to reconcile multiple times at once. Paleoclimatology, throughout its postwar history, continuously negotiated the boundaries between geological, environmental and political times.

Time and temporality are two similar, but not identical concepts. Vanessa Ogle defines the two concepts in a useful way: “Time’ is understood here as the time measured by clocks, calendars and natural timekeepers such as the sun and the moon. ‘Temporality’ is taken to describe how past, present and future relate to one another, for instance through repetition and cyclical temporalities or ruptured and discontinuous temporalities, and through experiences and expectations.” In this thesis, which indeed concerns “natural timekeepers”, time will be the preferred category of choice. However, this is not to naturalize the times produced through paleoclimatological practice, rather, time is treated a historical category, which is made through conceptual and material technologies as well as the material through which it manifests. Occasionally, when writing about how paleoclimatological times became enrolled to efforts of projecting the future and aligning the deep past with environmental politics, I see it more fitting to talk about temporality, as these instances concern how past, present and future should relate to each other. See: Vanessa Ogle, “Time, Temporality, and the History of Capitalism,” Past and Present 243:1 (2019): 314.
The thesis follows a loosely connected group of scientists in the North Atlantic region, from the 1940s to the 1980s. Some of them worked with ice, some with sediments from the ocean floor, while others were climate modelers and mathematicians. They can be grouped together not by their disciplinary affiliation or research object. Rather, they belong together because of their shared interest in how planetary dynamics could be studied and how they affected and were affected by human activities. Many of them were producers of environmental knowledge without considering the environment as a political object. They were gradually, as the scale of environmental politics grew, drawn into the work of synchronizing anthropogenic and planetary times.

A scientist such as Hilde Levi, who initiated the Copenhagen Isotope Colloquium, might seem an unlikely character in environmental history. But if we consider the scientific production of time as a part of the history of planetary-scale environmental knowledge, other actors appear and alternate epistemic geographies grow more important. In the last 20 years, temporal concepts such as the Anthropocene and the Great Acceleration have become ubiquitous in environmental discourse and moved matters of time into the foreground, while often taking the planetary scale as a given. They have brought forward a geophysically and geologically informed understanding of the environment, which transcends ecology and biology as the main scientific frameworks for thinking about planetary change. This relatively recent development builds on a longer postwar history, in which the spatial and temporal properties of the planet as an environmental object were defined.

At a very basic level, an environment at any scale or location, is fundamentally temporal: it can change, and it is potentially vulnerable to human impact. Any environmental problem – be it acidification, biodiversity loss, or global climate change – unfolds over time and relies implicitly on historical data in order to make sense in the present moment. The environment is, in this sense, a concept that puts nature in motion and gives it a direction. How it does so is a historical question. As Etienne Benson notes, the very idea of the environment has varied over time and “been materialized or put into practice in particular settings” throughout modern history.

Even though environmental awareness is far older than the postwar era, a particular conceptualization of human-nature relations was formed with the rise of the environment

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concept after 1945. “The environment”, as one unified concept, enabled a new narrative around anthropogenic impact on the planet as well as new forms of expertise, scientific practices and politics. This rise of the global environment has been widely covered by historians in recent years. These histories make evident that the global environment was not the natural outcome of scientific progress, but was rather the product of a multiplicity of factors, and in particular political efforts towards internationalization, Cold War geopolitics and science, and increased capabilities in computer modeling.

Another central, yet often overlooked, aspect of how environments are conceptualized is time. In contemporary environmental discourse, time is a crucial, almost self-evident, factor: it is running out, being budgeted for the future, passing tipping points, and stretching into new geological epochs. Scientifically produced timescales are at the heart of planetary-scale environmental knowledge and politics by providing a temporal framework and a baseline for future projections. Within the context of the humanities, and the field of history, questions of time and temporality have recently animated discussions about the Anthropocene and the relation between human history and geological time. While it stands clear that scientifically produced environmental timescales have permeated recent debates within both the humanities and environmental discourse more broadly, the origins of these timescales are far less evident.

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12 “Environmental historians broadly have engaged with the spatial and material elements of the past, but neither they nor even big history or evolutionary history have fully engaged with how societies construct time or constitute human temporalities” as Mark Carey and Alessandro Antonello put it in a 2017 article. See: Mark Carey and Alessandro Antonello, “Ice Cores and the Temporalities of the Global Environment,” Environmental Humanities 9:2 (2017): 186. There are of course notable exceptions, such as: Emil Flata, When Science Could Not Wait: Climate, Experts, and the Times of Anthropogenic Change, 1945–1979 (PhD diss., Oslo: University of Oslo, 2023); Alessandro Antonello, “Antarctic Krill and the Temporalities of Oceanic Abundance, 1930s–1960s,” Isis 113:2 (2022): 245-265.


14 As an example, the recent assessment report from the Intergovernmental Panel on Climate Change (IPCC) makes use of proxy record data in order to arrive at its conclusions. See: IPCC, Climate Change 2021: The Physical Science Basis. Contribution of Working Group I to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change (Cambridge: Cambridge University Press, 2021).

Research objectives

In the four empirical chapters of this thesis, I follow the emergence and political trajectory of two climate proxy records – ice cores and deep-sea cores – from their emergence in the years after WWII to their consolidation as scientific underpinnings of Earth System Science in the 1980s. Even though ice cores have received quite a lot of attention from historians, they have mostly been considered as a technology of their own, rather than part of a larger paleoclimatological endeavor to compare and synchronize different proxy records into a coherent whole. My ambition is to draw inspiration from the work already done on the history of ice core drilling, and extend it further towards planetary-scale environmental knowledge. Deep-sea cores, or deep-sea sediment cores as they sometimes go by, have not been the subject of much historical inquiry at all, and have not previously been studied in relation to other proxy records. In this sense, the ice- and deep-sea cores can be both empirically relevant study objects in their own right, but also serve as examples of how proxy records more broadly have evolved and been synchronized in the postwar era.

Geographically, this thesis is limited to the United States and Scandinavia or, put differently, the North Atlantic region. Previous research in environmental history have highlighted the importance of the North Atlantic as a space for making planetary-scale knowledge. Similarly, the North Atlantic was a limited, yet crucial, context for the postwar evolution of paleoclimatology, as knowledge, technologies, and the scientists themselves continuously moved across the Atlantic in the formative decades after World War II. Considering the influence scientists working in the North Atlantic region have had, it thus

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serves as an important context for the emergence of paleoclimatology on a global scale and it also speaks to the ways in which localized knowledge can be scaled up to speak for the entire planet. By following emblematic actors and technologies in this postwar North Atlantic context, my ambition is not to write a complete history of postwar paleoclimatology, but rather to explore the scientific, environmental, and political implications of producing new geochronologies in an era of exponentially growing anthropogenic impact on the planet. Contrary to popular description of ice- and deep-sea cores as “the Earth’s own archive” or “natural time machines”, the political and scientific geographies of paleoclimatology highlight the historicity of how scientists produce time.20

In short, I see this thesis to have two main research objectives. One is empirical, the other theoretical. Empirically, my aim is to expand the history of environmental knowledge in the postwar era to also involve time and temporality. The history of paleoclimatology, and the proxy records the field relies on, has, despite its foundational role for contemporary environmental understanding, remained at the background of historical writing on postwar environmental sciences, as space has been the preferred category over time. Following the paleoclimatological work conducted in the North Atlantic region in the postwar era is a way to historically examine the temporalization of planetary-scale environmental knowledge.

Theoretically, I seek common ground between two strands of history that have, despite their shared interest in planetary-scale conceptualizations, remained mostly separated: history of science and environment and theory of history. Critically and historically examining the origins of the scientific timescales that underpin contemporary discussions on historical time, the Anthropocene and the planetary scale, add historical texture to the theoretical developments in theory of history.21 I also seek to show how there are benefits for historians of science and environment to utilize concepts from theory of history as theoretical frameworks from theory of history open up empirical questions regarding how time and temporality have been configured through scientific practice. As the relation between human history and planetary dynamics are increasingly moving to the forefront of humanist inquiry and global environmental politics, history of science and environmental history can provide a deeper understanding of how planetary pasts enter the political present.

Paleoclimatology and planetary times

The planet, Dipesh Chakrabarty asserted in a 2019 article, is emerging as a humanist category. The increasingly conflated relationship between humanity and the dynamics of the Earth System, he argues, marks the beginning of a “planetary regime of historicity”. Suddenly, “the history of the planet, the history of life on the planet, and the history of the globe made by the logics of empires, capital, and technology” appear as part of the same history. Duncan Kelly similarly argues that one challenge of the Anthropocene is to bring together “…deep time, democratic time and accelerated time…” with “…more mundane time-frames of politics such as electoral or news cycles and leadership bids”. Chakrabarty and Kelly are far from the only ones grappling with the relationship between historical and natural timescales in light of planetary-scale anthropogenic impact. In the last decade, scholars across disciplinary boundaries have taken on the task to understand and narrate the “epic of the Anthropocene” as well as the clashing temporalities it is proposed to entail. For history in particular, the emergence of the Anthropocene concept has brought with it difficult questions regarding the properties of historical time itself: how should the relationship between natural and historical times be understood? What does the assertion of anthropogenic geo-scale impact mean for the writing of history?

There is already a rich literature about these matters. Some call for an integration of scientific and historical knowledge in order to match the new temporal configurations of scientific and historical knowledge in order to match the new temporal configurations of

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23 Chakrabarty, “The Planet,” 2. The traditional division between the globe and the planet is thereby called into question. Joyce E. Chaplin provides a good description of this divide: “The global is social as it implies the social relations that extend over the globe. The planetary, however, is physical, implying the physical planet itself.” Joyce E. Chaplin, *Round about the Earth. Circumnavigation from Magellan to Orbit* (New York: Simon & Schuster, 2012), 11.
25 He was, however, among the first to bring the temporal conundrums of the planetary-scale into broader humanist discourse. See: Dipesh Chakrabarty, “A Climate for History: Four Theses,” *Critical Inquiry* 35.2 (2009): 197-222.
the planetary scale, while others argue for a more cautious stance in relation to the universalizing ambitions of Earth System Science and the Anthropocene concept.\(^{28}\) The sudden rise of the Anthropocene concept – it was first coined in 2000 – is often taken as a point of departure in theoretical discussions about historical time and the planetary scale.\(^{29}\) It has rapidly, as François Hartog recently put it, “upended all our temporal preconceptions”.\(^{30}\)

In this framing, this new kind of historical consciousness marks a radical break with earlier conceptions of the relationship between human and planetary timescales and binds together times that were previously seen as separate. While this is true within the field of history, and possibly the humanities and public discourse more generally, an overly idealist narrative of a sudden emergence of integrated human-geological timescales in the early twenty-first century risks to miss the longer history of temporal integration in environmental sciences that preceded it.

For scientists working with ice- and deep-sea cores in the postwar era, thinking across multiple timescales was an integral part of their work well before anyone talked about the Anthropocene. They were not only concerned with dating and measuring planetary timescales, but also with establishing fault lines relating to whether and how different times – geological, geophysical, biological and historical – should be aligned. These alignments were not self-evident, and were renegotiated over time. Contrary to the temporal duality of geological versus human time, often visible in Anthropocene discussions, the temporalization of the planetary-scale environment in the second half of the twentieth century points towards a much more pluralistic temporal landscape.\(^{31}\)

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\(^{31}\) Dipesh Chakrabarty provides one example of this approach, where the Anthropocene equates a division into a geological and a human temporalization, in a 2018 article: ‘The Anthropocene requires us to think on the two vastly different scales of time that Earth history and world history respectively involve: the tens of millions of years that a geological epoch usually encompasses (the Holocene seems to have been a particularly short epoch if the Anthropocene thesis is right) versus the five hundred years at most that can be said to constitute the history of capitalism.” Chakrabarty, “Anthropocene Time,” 6.
The recent interest in the Anthropocene was not a naturally given development. Its emergence can be situated, together with postwar paleoclimatology, in a longer history of negotiating the boundaries between historical and natural time. Since the mid-eighteenth century, and the rise of what Aleida Assmann, and other historians, have called the “modern time regime”, natural and historical timescales have been increasingly divided up into distinct disciplinary temporalizations. They place the rise of the modern time regime in the decades after 1750. Here, “the plurality of historical times characteristic of the emerging modernity”, as Helge Jordheim argues, “was synchronized into the linear, homogeneous, teleological time of progress.” The modern time regime enabled a distinct division between past, present and future. Reinhart Koselleck has famously called this time period, roughly 1750–1850, the Sattelzeit. This saddle period, he argued, marked a fundamental transformation in the experience of time. A range of concepts, such as “progress”, “history”, “development” and “democracy”, were temporalized in a new way. They began to signify movement and the anticipation of a future that would differ from the present. The “space of experience” was, using Koselleck’s vocabulary, beginning to drift away from the “horizon of expectation”. This is the temporality of modernity, with a narrow present, a long past behind us and radically different future awaiting around the corner.

The Sattelzeit also marked a separation between historical time and a variety of natural timescales. The rise of new professionalized scientific disciplines – geology, biology, archeology, paleontology and so forth – with their own temporal sensibilities enabled a new division of natural times. The early-modern historia naturalis, which was particularly oriented towards collecting, observing and mapping natural phenomena, had not sought to

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36 This distinct division between the present and the future is a key feature of the modern time regime and also an argument to why it ended in the aftermath of the Cold War. It was, during this time, replaced by presentism,
create laws or rules for historical developments over time. The emergence of these disciplines, and the practices and epistemologies attached to them, enabled a different temporalization of the natural world.

Geology in particular, with its vertical, deep and multilayered understanding of time enabled a distinctly different temporal framework in comparison to history, with its homogenous, progressive and linear understanding of time. Geology became a science of time, which used its own practices and standards in order to systematically understand the planetary past. The rise of geology is popularly described as a radical break with Christian orthodoxy and the idea that there is no pre-human history of the planet. Stephen Jay Gould famously declared the discovery of geological time as a fourth revolution, an event of the same magnitude as the Copernican revolution, Darwinian evolution theory and Freud’s discovery of the subconscious. A similar narrative of sudden enlightenment regarding the deep planetary past is visible in Stephen Toulmin and June Goodfield’s classic *The Discovery of Time* from 1965 as well. Geological time appears, in this historical narrative, as a revolutionary new idea, which was suppressed by religious and cultural dogmatism. The question of the age of the Earth can thereby be located in the late-nineteenth century “conflict thesis”, which emphasized the incompatibility of science and religion.

Recent scholarship have, however, begun to question this story. Ivano Dal Prete argues that the conflict between theology and geology was a product of political tensions in Enlightenment Europe, rather than being an inherent tension caused by two radically different cosmologies. Geological time emerged as a distinct temporalization as well as an argument against religious and conservative opponents to Enlightenment philosophes, and thereby further came to emphasize the fundamental difference between geological and

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40 Stephen Jay Gould, *Time’s Arrow, Time’s Cycle: Myth and Metaphor in the Discovery of Geological Time* (Cambridge, MA: Harvard University Press, 1987). The three other revolutions were not chosen by Gould, but by Freud, who placed himself along with Copernicus and Darwin. Gould’s argument is that geology belongs there as well, together with cosmology, evolutionary biology and psychology.
human history. But the early modern period was not devoid of deep time thinking. On the contrary, debates about the eternity of the world and evidence of deep planetary pasts took place well before the advent of geological sciences. Noah Heringman makes a similar case when he argues that the emergence of deep time was not the same as the emergence of geological time. Prominent historical figures such as Charles Lyell and James Hutton are important to the history of deep time, but they were not the only ones thinking about the pre-human past. Before the formal establishment of geology as a scientific discipline, deep time was both anthropological and geological, as "concepts and narrative forms associated with deep time, including revolution, reversion, catastrophe, species memory and the primitive, result from cross-pollination rather than disciplinary specialization." Deep time, in other words, did not emerge as a distinctly geological temporal category. It was only later geological time and deep time came to appear as synonymous, and thereby could be contrasted against the time of human history.

Even though these deliberations took place centuries before the emergence of postwar paleoclimatology, I zoom in on this history, because it shows the historical instability of the division between natural and human timescales. The early history of geology, and the scientific attempts to divide time between disciplines and epistemologies, makes clear that the emergence of deep time was not merely about empirical findings in the strata. Instead, knowledge about a pre-human past was much more multi-facetted and deeply interwoven with political and social temporalities and geographies. Deep time had to be made, not just discovered. As historians of nineteenth century earth sciences, such as Deborah R. Coen, Pratik Chakrabarti and Fredrik Albritton Jonsson, have shown, geological temporalization and imagination emerged in tandem with colonial expansion and visions of impending disasters and abundant reserves of fossil fuels.

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44 Deep time refers to thinking about the pre-human past, but it would be anachronistic to talk about geological time in this setting, as geology did not yet exist as a scientific discipline.
45 Ivano Dal Prete, *At the Edge of Eternity*, 9-12. Lydia Barnett makes a similar argument concerning discussions about planetary-scale environmental disaster in the early modern period, see: *After the Flood: Imagining the Global Environment in Early Modern Europe* (Baltimore: Johns Hopkins University Press, 2019).
In other words, the divide between human and natural timescales can be seen as a historical process in itself. This divide has, as these examples show, not been quite as strict as it may first appear and has been continuously re-negotiated over time. However, proponents of the theory of a modern time regime have not, until very recently, engaged with times beyond the strictly sociopolitical. Both its proposed emergence in the Enlightenment and its proposed collapse in the post-Cold War era has taken place separately from processes of temporalization taking place in the natural sciences. 48 The sudden twenty-first century appearance of the Anthropocene concept has challenged this historiography, but at the same time this telling of the story does not question the cohesiveness of the modern time regime. The Anthropocene concept arrives at the scene when the modern time regime has already ended. 49

A different way of approaching this is to not take the integrity of the modern time regime for granted. Drawing on Koselleck’s work on multiple temporalities, Helge Jordheim questions the monolithic status given to the modern time regime and instead argues that it has never been as cohesive as Assmann and Hartog assert. In order to make a plurality of times fit together in a coherent regime of historicity, a wide range of practices of synchronization are necessary. Historical time needs to be actively put together. How this happens, through which technologies and media, and by which actors, are not a given, but a historical question to be investigated. 50 Similarly, Vanessa Ogle has shown how modern Western ideas about time was continuously challenged and complemented by alternate temporal orders in non-Western settings. 51

The rise of the environment concept in the postwar era could be seen as another kind of challenge to a strictly sociopolitical understanding of the modern time regime. Postwar environmental knowledge, as it took shape in the decades after 1945, would enable new ways of binding together natural and historical times. In particular, the rise of isotope dating technologies and paleoclimatological practices, such as deep-sea- and ice core drilling,

48 Aleida Assmann, Is Time Out of Joint; François Hartog, Regimes of Historicity. Zoltán Boldizsár Simon, for example, argues that we need to expand the thinking around regimes of historicity to domains beyond the sociopolitical and critiques the limited view of previous scholarship: “What we need to understand today is the emerging historical sensibility in those domains, and what we need to conceptualize is the way in which those domains, in their own right, conceive of change over time”. Simon, History in Times of Unprecedented Change, 4.

49 The modern time regime is often said to have ended around 1990, at “the end of history” as Francis Fukuyama famously asserted, and been replaced by a “broad present”, in which the past, present and future lose their distinct temporal qualities and are replaced by a present which swallows both the past and the future. E.g Hans Ulrich Gumbrecht, Our Broad Present: Time and Contemporary Culture (New York: Columbia University Press, 2014).


could dramatically expand the temporal scope of environmental thought and placed events from the deep past in political and scientific discussions about anthropogenic influence on the planet.\textsuperscript{52} Rather than assuming that the integration of deep planetary timescales into environmental thinking was a linear process of discovering new temporal data and then having to manage it in different ways, the unstable relationship between historical and natural times is a developing historical process.

A key theoretical insight comes from the work of Reinhart Koselleck, who discussed this particular phenomenon – how natural times can enter history – already in the 1980s. He argued that this in itself was an historical process unfolding over time. Koselleck’s theorizations on multiple historical times, and how temporal experiences themselves are historical products, have been picked up in recent work on the Anthropocene, as they offer a historical theorization of how different times can co-exist.\textsuperscript{53} In a 1986 essay, entitled “Space and History” he made an analytical separation between the historical, what is normally perceived as human history, and the metahistorical, the natural “pregivens” outside the framework of human impact.\textsuperscript{54} Even though these are two distinct categories, Koselleck does not perceive of them as static, instead he opens for the possibility that the metahistorical can enter the historical:

Theoretically this would entail asking where the metahistorical pregivens of the human Lebensraum shift or are transformed into historical pregivens that humans can influence, master, or exploit […] Seen in this light, the relational scale between space and history shifts depending on whether spatial pregivens are conceived of as metahistorical or historical.\textsuperscript{55}

In this view, the unstable relationship between the historical and metahistorical has implications for the conceptualization of historical time. He sees the “conversion of metahistorical situations into historical spaces” as an important issue to be discussed by a theory of history, and as something every historiography makes implicit or uses explicitly.


\textsuperscript{54} This can be compared to earlier attempts at theorizing the relationship between different layers of time, for example in the work of Fernand Braudel, who divided historical time into three different speeds, but did not consider the fluidity between these different temporalizations. Fernand Braudel, \textit{The Mediterranean in the Ancient World}, trans. Sian Reynolds (London: Allen Lane, 2001).

Koselleck notes a tendency to assign quasi-ontological qualities to phenomena, such as ecological degradation, that actually occur within the space of history, and therefore also of political action. It is the historian’s task to question this quasi-ontological status of natural pregivens and incorporate them “into every analysis of historical and political conditions.”

The contemporary Anthropocene concept is often considered such a pregiven in academic and popular discourse. The way this temporal reconfiguration came about is frequently taken for granted, despite also being the outcome of negotiation between different ways of temporalizing human and planetary histories. Koselleck’s historiographical approach questions this quick renaturalization of the Anthropocene (as a new “pregiven”). His focus on the fluidity between different layers of time makes it possible to see how the metahistorical, under specific circumstances, can become historical. The demarcations of the historical domain also have political ramifications as they determine both the scope of political action and what is naturalized outside it. On the topic of configuring the globe as one unit of experience, encompassing both the historical and the metahistorical, Koselleck writes, “…how it will be formed as a unit of action is a question of politics and not of geography.”

Studying how different natural pregivens have been transformed into history thus becomes a way to reconstruct how these pregivens also entered the political realm.

The way this process unfolds is, however, rarely free from conflicts. Dan Edelstein, Stefanos Geroulanos and Natasha Wheatley argue that Koselleck’s approach to multiple temporalities tend to obscure how past, present and future are fragmented, situated, and occasionally irreconcilable categories. Even though multiple times can exist all at once, they do not always do so in a harmonious way. In order to capture these tensions, Edelstein, Geroulanos and Wheatley use the concept chronocenosis. Stemming from the ecological term biocenosis, which refers to the cooperation, adaptation and conflict between individuals and populations in a particular ecosystem, chronocenosis “offers a sense that multiple temporal regimes are not merely concurrent, but at once competitive, conflictual, cooperative, unstable and sometimes anarchic.” The relationship between multiple temporal regimes should therefore be understood as part of a “power-time nexus”, in which the temporal order is deeply interwoven with social dynamics and political authority.

56 Ibid, 40.
58 Koselleck, “Space and History,” 40.
Placing time as a central category in the history of postwar paleoclimatology can, following Koselleck’s distinction, open up for a critical examination of how the metahistorical and the historical have been negotiated in this particular context. Edelstein, Geroulanos and Wheatley add a critical dimension and bring forward how the times that are not synchronized or integrated into dominating temporal frameworks are also relevant objects of inquiry.

These theoretical insights are not only relevant to the sociopolitical domain. Times produced through scientific practice are also possible to analyze and approach through the theory of history. Environmental times are made, they are assembled and synchronized in particulars ways by particular actors.

Making time through science

Not only theorists of history have taken an interest in the planetary-scale. For environmental historians and historians of science, the political and scholarly interest in the planetary have opened up new historical questions: How was planetary-scale knowledge configured? Which actors, geographies, and technologies have been able to speak for the planetary? Whereas “planetary history” often denotes vast scale histories across time and space, which take place at arrogated levels of “civilization” or “humanity”, this approach to the planetary rather seeks to find the localized conditions for conceptualizing

61 This does not mean that Koselleck’s work is a perfect fit with in regards to scientific and environmental times: Vanessa Ogle, for example, argues that Koselleck’s notion of multiple temporalities still presupposes some form of progression, albeit in different tempos and with occasional interruptions, and thus does not fully allow for other ways of conceptualizing time. Koselleck’s Western-centric outlook has also been the subject of criticism and there have been attempts to re-work is conceptual apparatus in order to make it more applicable in non-Western contexts. See: Vanessa Ogle “Time, Temporality and the History of Capitalism,” Past and Present 123 (2019): 312–327; Hagen Schulz-Forsberg, “The Spatial and Temporal Layers of Global History: A Reflection on Global Conceptual History through Expanding Reinhart Koselleck’s Zeitgeschichte into Global Spaces,” Historical Social Research 38:3 (2013): 40–58.

environmental matters on a planetary scale. The planetary is not taken for granted as a category, but is rather the study object in itself. For ice core scientists in the 1950s, for example, the planetary-scale had to be assembled by a wide range of practices. It did not manifest itself to the scientists in the field.

This strand of scholarship builds on a longer tradition in history of science, environmental history, science and technology studies (STS) and geography of historicizing and situating environmental knowledge and concepts. Environments, on any scale, are not existing “out there” as something to be discovered and mapped, rather they are the products of human intervention, both materially and conceptually. They are thereby historical, and signify a “knowledge-based representation of the material world in which humans and their actions are embedded”. Environmental knowledge has been shaped by the historical context in which it is produced as well as through the technologies and media that have made the environment coming into being. From satellite remote sensing to ecological statistics, technologies of measurement, quantification, mediation and prognostication have shaped how environment come into being at different places in space and time.

However, histories of the scientific and conceptual making of environments have often been more oriented towards spatial concepts and scales. There is, for example, a rich

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63 This large-scale planetary history is similar to “big history” as well, which staples the brief timescale of human history onto the immensely longer timescale of the Earth, and thereby expands the scope of the historian’s traditional temporal framework, while also being significantly indebted to scientific knowledge from archeology, biology, geophysics and astronomy. E.g Jo Guldi and David Armitage, The History Manifesto (Cambridge: Cambridge University Press 2014); Walter Alvarez, A Most Improbable Journey: A Big History of Our Planet and Ourselves (New York: W. W. Norton, 2018). The field of climate history, which draws on both paleoclimatological, paleoecological as well as historical records to connect historical events with changes in the natural environment occurring at the same time, is another genre of planetary history. For an overview of these approaches, see Dagomar Degroot, Kevin Anchukaitis, Martin Bauch, et al, “Towards a rigorous understanding of societal responses to climate change,” Nature 591 (2021): 539–550.


scholarship on how the environment was scaled up to the global level in the postwar era.  
But, as Mark Carey and Alessandro Antonello have argued, the production of time and temporality has, despite its centrality to the conceptualization of environments on different scales, remained outside the scope of most historical research.

In this thesis, I use the word timemaking to point to an active way of producing time through practices, technologies and materials. Time is not merely discovered, as it is sometimes framed in the literature, but actively constructed through the work of humans and their interactions with the materials through which time becomes legible. “Materials are not in time”, Tim Ingold writes, “they are the stuff of time itself” and thereby points to how time needs to be understood as deeply interwoven with the materials it appears in, rather than merely being inscribed or discovered.

Ingold’s claim does, however, only take us half way: in order for materials to become “the stuff of time itself” work, infrastructure, and scientific knowledge are necessary. A chunk of ice is not the same thing as an ice core. Materials become the stuff of time through practice, they acquire meaning through an interplay between materiality and human intervention. How materials can become time is therefore also a matter of historical contingencies, knowledge regimes and the active labor of, in the case of ice- and deep-sea cores, scientists.

Timemaking is in this setting not a passive act of glancing at different natural clocks, but an active and creative effort to reconcile multiple proxy records, timescales and materialities into a planetary-scale framework. It is by definition multiple: numerous proxy records have been merged, discarded or aligned with each other and with the timelines of history and environmental policies. Contrary to descriptions of ice- and deep-sea cores as global climate archives, in which they function solely within a planetary system and become detached from the geographies in which they emerged, the history of planetary timemaking stems from localized encounters and particular scientific networks and geographies.

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69 I self-consciously use this concept, which is not widely established, to find a way to distinguish particular scientific activities that are aimed at producing time. It is a way to get around the sometimes passive language of “timekeeping” or of, in the case of paleoclimatology, “natural archives”. With the notion of timemaking I seek to highlight something more elaborate and active.
70 E.g Toulmin and Goodfield, The Discovery of Time; Alley, Two-Mile Time Machine.
Geographers of science have critiqued the “globalizing instincts” of environmental knowledge making and pointed to the political and social ramifications of how knowledge becomes global.72 For example, in her studies of global oceanographic knowledge, Jessica Lehman shows how the ways through which the planetary was constituted as an object of knowledge built on situated practices and older networks of power, imperialism and scientific authority.73 Planetary-scale environmental knowledge is in this sense assembled and created through means of technology, concepts and practices, rather than being uncovered as ready-made facts in the world.74

In this larger epistemic geography of environmental knowledge, climate records occupy an important position. They form a baseline against which anthropogenic influence can be measured and they provide temporal depth to the efforts of modeling and predicting future climate conditions. But despite their universal appeal, climate records can serve different functions in different contexts and their meaning is in constant transformation. They exist as scientific objects in the field in the form of, for example, a recently retrieved ice core sample, yet to be analyzed and quantified.75 Most climate scientists encounter them already in a mediated form: either as data or catalogued samples in a storage facility. Climate archives, no matter in which form, are however most often seen as ahistorical; they are data harvested from nature, repositories of time that scientists can access when they need to.76 Vladimir Jankovic argues that this idea of the repository is flawed. We should instead conceive of climate archives as ongoing processes, which are produced and shaped by their perceived utility and input from scientists, but also by stakeholders such as policymakers, data managers, administrators and the private sector.77


76 One of the most famous deep-sea core storage facilities in the world is for example called the Lamont-Doherty Core Repository. https://corerepository.ldeo.columbia.edu. Accessed 2023-04-22.

Within climate science, climate records are divided into two categories: instrumental records and proxy records. Instrumental records are direct measurements of for example temperature, which often date back to the nineteenth century, and occasionally as far back as the mid Seventeenth century, although in a less systematic format compared to later measurements. Different data series – such as the HadCRUT in the UK, GISTEMP and NOAAGlobalTemp in the U.S. – provide continuous annual temperature measurement since the mid-nineteenth century, and have become central to climate policy advice institutions such as the IPCC. Proxy records are on the other hand, as the name indicates, stand-ins for when instrumental records are not available. Contemporary environmental sciences make use of many different kinds of records and the information stored in ice, sea floor sediments, pollen, corals, tree rings, and others, in order to access data from the deep past that preceded the instrumental records. Different records tend to have different qualities: they can capture rapid events such as floods and volcanic eruptions, or they can capture gradual changes – in for example sea levels, temperature and atmospheric CO₂ – unfolding over longer periods of time. The kind of information a proxy record can convey varies in kind. Biological proxies, such as tree rings and pollen, gives a record of how temperature as well as flora have shifted over time in particular geographies, while geochemical proxies – often oxygen isotopes stored in ice or sea floor sediments – provide


Figure 1.1: A technician inserts a new core to the deep-sea core collection at Scripps Institution of Oceanography, SIO Deep Sea Drilling Project Records, 1961–1987 Special Collections & Archives, UC San Diego, La Jolla, Digital Collection.

Figure 1.2: Willi Dansgaard holding an ice core sample after it has been recovered from the ice sheet. Willi Dansgaard, Frozen Annals (Copenhagen: Niels Bohr Institute, 2005): 67.
data on temperature, atmospheric CO$_2$ and sea levels for hundreds of thousands of years. Physical records, such as foraminifera (fossils), diatoms (microfossils), leaf waxes, and lithified marine and terrestrial sediments, can often be found in marine and lake sediments and rock formations and enable even longer timescales as well as more detailed analysis of droughts, vegetation and geomorphology. Proxy records do in this sense not just produce timescales, but create historical frameworks for particular environmental metrics.

Even though I will particularly focus on ice cores and deep-sea cores in this thesis (I will elaborate on these decisions in the next section of this chapter), the proxy records are often functioning as a system themselves in a contemporary setting. Within the system different records compare and combine with each other to produce a coherent understanding of larger environmental trends. Historically, however, the relationship between different proxy records and how they related to each other, as well as to human history, has been a continuous negotiation. Several materialities – ice, mud, rocks, pollen, fossils, wood – have gotten enrolled to the larger project of timing the planetary system, but their origins, both materially and disciplinarily, differ.

Ice cores, perhaps the best known proxy record and a key technology in this thesis, emerged through the field of glaciology in the postwar era. Although interest in the vertical dimensions of glaciers and the cryosphere had existed for a longer time, the particular technology of ice core drilling provided scientists with the possibility to penetrate the ice sheets all the way down to bedrock. By the end of the 1960s, ice cores were increasingly used to map past climatic changes and also to project future ones. Since then, ice cores have become “climate change messengers”, records of planetary change against which humanity can measure its own impact on the Earth System.

They way ice cores work is, on the most rudimentary level, fairly straightforward: they are cylinders of ice, recovered from the ice sheet with advanced drilling technology and are after their recovery shipped and stored in research facilities across the world. They can run several kilometers long, even though they are stored in smaller pieces once they are ready to be transported to the storage facilities. The data they contain stems from the

85 For an overview of paleoclimatology and adjacent fields, see Colin P. Summerhayes, Paleoeclimatology: From Snowball Earth to the Anthropocene (Hoboken: Wiley, 2020).
86 Chapter three will provide a more in depth account for the history of ice core drilling and its place in the history of glaciology.
processes through which the planet’s ice sheets and glaciers have come about in the first place: from snowfall over thousands of years. Each layer added, and later compacted as time progresses, traps tiny bubbles of air inside the ice sheet and by recovering ice cores these air bubbles trapped inside the ice becomes accessible for the scientists on the surface. The bubbles serve as tiny samples of the atmospheric conditions at the time they were trapped in the ice, making the perfect round bubbles a kind of microcosm of much larger planetary conditions in the deep past.

The air bubbles are however not the only thing of interest in the cores. While atmospheric conditions can be accessed directly through the air bubbles, temperature data cannot be retrieved this way. Instead, this data is inferred from the isotopic composition of water that comes from the ice cores if they are melted down. In an interview with *Scientific American*, the British glaciologist Robert Mulvaney describes this process:

> Water is made up of molecules comprising two atoms of hydrogen and one atom of oxygen (H₂O). But it’s not that simple, because there are several isotopes (chemically identical atoms with the same number of protons, but differing numbers of neutrons, and therefore mass) of oxygen, and several isotopes of hydrogen. [...] Using sensitive mass spectrometers, researchers are able to measure the ratio of the isotopes of both oxygen and hydrogen in samples taken from ice cores, and compare the result with the isotopic ratio of an average ocean water standard known as SMOW (Standard Mean Ocean Water).

Similarly to the ice cores, deep-sea cores are also of interest both for their stratigraphy as well as their more dispersed contents. Materially, the deep-sea cores are a sibling to their cryosphere counterpart (see fig. 1.1). They, too, are long cutouts from a remote place, through which a stratigraphy becomes visible. Similar isotope dating methods are applied to analyze the deep-sea cores as well and the different materialities dispersed inside the cores, often foraminifera, algae, and plankton, offer different kinds of temporal data. Stemming from a different unhospitable geography – the ocean floor – deep-sea core drilling has evolved within the ocean sciences, and primarily physical oceanography, during

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89 Martin Skrydstrup sees the shape of the bubbles as a part of a larger aesthetic vision of paleoclimatology, noting how ice core drilling facilities in Greenland are similarly shaped geodesic domes, which all correspond to the shape of the planet itself. See: Martin Skrydstrup, “Of spheres and squares: Can Sloterdijk help us rethink the architecture of climate science?” *Social Studies of Science* 46:6 (2016): 854–876.
the twentieth century. Even though earlier interest in the ocean floor existed before the twentieth century, the technological means and scientific infrastructure did not. In their 1942 foundational book *The Oceans*, Harald Ulrik Svedrup, Martin Johnson and Richard Fleming noted how knowledge about ocean floor sediments had barely improved at all since the *Challenger* expedition in the 1870s. It was only in the postwar era, as a part of Cold War mobilization in oceanography, that deep-sea core drilling became a practically feasible and financially possible scientific activity.

The utility of deep-sea cores has varied over time: they have been used in the debates about plate tectonics, studies in geomorphology and the geological history of the oceans, as well as paleoclimatological- and oceanographical studies of past environmental conditions in the oceans. The very field of paleoceanography emerged in the 1970s and 1980s as a disciplinary umbrella covering the many different efforts to use deep-sea cores and other kinds of oceanic proxy records. In a contemporary setting, deep-sea cores can be utilized to study a variety of past processes in the oceans: how the oceans have interacted with changes in the positions of the continents, wind patterns, solar radiation, polar ice coverage and variations in the Earth’s orbit. They can also provide data concerning currents, oceanic fronts, sea-surface temperatures, changes in biogeography, changes in the vertical thermal structure of the ocean and, which is crucial to current environmental knowledge, how the oceans have affected and interacted with the global climate.

Ice cores and deep-sea cores are of course only two proxy records out of many more, but they have proven to be crucial technologies both within and outside the disciplines in which they emerged. During the last decades of the twentieth century, entire research areas have been founded around them (paleoceanography is one such example) and the cores themselves can be understood in a larger scientific infrastructure. In this sense, if we are to understand how the ice- and deep-sea cores work, accounting for their contents and the methods through which they become legible is not enough. Most climate scientists do not directly engage with the material cores of ice and sediments, instead they encounter them in mediated formats as datasets and visualizations. They are stored in large facilities, proxy record archives, in which visiting scientists can borrow samples to study.

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92 The first issue of the journal *Paleoceanography* was released in 1986.
93 Summerhayes 118-119
94 One example is the National Ice Core Laboratory (NICL), established in Denver, Colorado, in 1993. [https://icecores.org/about#facility](https://icecores.org/about#facility). Accessed 2023-02-05.
Previous research

The history of paleoclimatology has already been the subject of historical studies within environmental history and history of science and technology. Scholars from geography, STS, anthropology and political science have also engaged with the implications of using proxy records in a contemporary setting.

Ice cores, given their prominent role in contemporary paleoclimatology, have gathered the most attention from historians of the proxy records I will investigate in this thesis. Initially, the history of ice core science was written by the practitioners themselves: Chester C. Langway, Jr. who was an important figure in early attempts to recover ice cores in Greenland summarized the formation of early ice core science in a brief monograph.  

Another key figure in this thesis as well as in ice core science in general, Willi Dansgaard, has written an autobiographical account of his impressions of his years in Greenland and the scientific community of ice core scientists. As ice cores have grown increasingly more well-known in the media and popular culture, there have also been a few works by glaciologists, climate scientists and environmental journalists aimed at a broader audience.

In addition to the work made by glaciologists and people who themselves have been involved in ice core science, a number of historians, anthropologists and sociologists of science have directed their attention towards ice cores. This thesis is very much indebted to this work, and seeks to build upon these important contributions. In particular, Janet Martin-Nielsen have written several articles primarily or partly dedicated to the history of ice core science in Greenland and its geopolitical implications. Her work has brought

95 Chester C. Langway Jr., The History of Early Polar Ice Cores (Buffalo: ERDC/CRREL TR-08-1, 2008).
forward how ice core drilling was part of a much larger transformation of science in Greenland during the postwar era, turning the island from a place of geopolitical concern to environmental concern. Maiken Lolck has written the most extensive exposé of Denmark’s role in ice core science, and particularly the working life of Willi Dansgaard, which serves as a biographical foundation for my two chapters that concern Dansgaard’s career.99 Dania Achermann’s recent article on the work of Willi Dansgaard and the rise of “vertical glaciology” gives a particularly useful framework for thinking about the connections between ice core drilling and climate science in the postwar era.100 In this more specific, ice core related, historiography, my aim is to contribute with two aspects of its history that are hitherto overlooked but nevertheless, I argue, important for the synchronizing function ice cores have come to occupy in planetary-scale environmental knowledge. Firstly, in chapter two, I will situate the rise of ice core drilling in Greenland, and the notion of “isotope glaciology”, in a disciplinary setting outside glaciology and geophysics. Secondly, in chapter four, I will provide a more extensive history of the ways in which ice core data got picked up in scientific and political settings beyond the cryosphere. With this, I hope to continue the empirical and theoretical contributions made by the authors above by further connecting ice core drilling with other paleoclimatological methods as well as the emergence of Earth System Science. Ice core drilling was always, in other words, about more than just ice.101

Deep-sea cores have received considerably less attention from historians and other scholars from the social sciences and the humanities. Much like the ice cores, early attempts at sketching histories comes from the field of paleoceanography and some of the scientists who have been part of the story themselves.102 Among historians, Christoph Rosol has

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100 Dania Achermann, “Vertical Glaciology”.

101 In fields beyond history of science and environmental history, there are several works concerning ice cores and their cultural and societal implications. Kathryn Yusoff has written on the discursive construction of ice cores as a natural archives and illuminated the politics of the ice core in larger a geopolitical and social order. Similarly, Aant Elzinga and Jessica O’Reilly have written on the politics of working with ice cores, but from an anthropological perspective by studying ice core scientists in the field. Kathryn Yusoff, *BIPolar* (London: The Arts Catalyst, 2008); Aant Elzinga, “Making ice talk: Notes from a participant observer on climate research in Antarctica,” in Sabine Maasen and Matthias Winterhager (eds.), *Science studies: Probing the dynamics of scientific knowledge* (Bielefeld: Transcript, 2015), 181–210; Aant Elzinga, “Polar Ice Cores”; O’Reilly, “Sensing the Ice”.

written extensively about paleoceanography and climate knowledge, and especially on the relationship between drilling and climate models. David van Keuren provides, in his work, an important overview of the rise of ocean drilling – scientific and commercial – in a U.S. setting. However, the history of deep-sea core drilling remain for the most part to be written. The ambition of this thesis is to place the history of deep-sea drilling in a broader history of a re-temporalization of the oceans themselves. The Anthropocene ocean, as Jessica Lehman has called it, is a conceptually different scientific object compared to previous ocean epistemologies. Deep-sea cores played, I argue, an important but hidden role in the rise of this new kind of planetary ocean. Additionally, I seek to connect the history of Scandinavian oceanography and geophysics with this history, and point to the role of instruments and research frameworks originating in Scandinavia in postwar U.S. oceanography and climate modeling.

Even though the literature on the history of deep-sea core drilling is sparse, historians of science, technology and environment have engaged much more with the history of deep-sea exploration and its implications for environmental knowledge production. Helen M. Rozwadowski has covered the history of oceanography in several books and articles, with research spanning from the 1800s until the present. Sounding and sampling practices related to deep-sea research have also been topics in the history of ocean sciences and thus provides a pre-history and broader context around the deep-sea core drillings of the postwar era. The literature on the history deep-sea exploration provides key insights into the overarching framework in which ocean scientists have operated and how the scientific, institutional, and political ramifications for conducting research in the deep-sea have changed over time. However, the attentiveness towards time, visible in the literature on ice cores, is not as pronounced in the history of deep-sea research. The depths of the ocean and the ocean floor are typically presented as spatial scientific objects, rather than temporal ones, which is one perspective I hope to add with this thesis.

The relationship between ocean sciences and the geopolitics of the Cold War has been covered extensively by historians in recent years, including both studies of science diplomacy and the technologies and practices of oceanographers during the cold war.

103 Rosol, “Hauling Data”; van Keuren, “Breaking New Ground”.
Naomi Oreskes provides an overview of oceanography’s relation to Cold War geopolitics in an American context, but several other works have covered this history as well, both in the U.S. and beyond.¹⁰⁶

The central role oceans play in planetary dynamics and global environmental conditions has, in the context of the Anthropocene, led to an increased interest in the history of environmental ocean sciences. Sarah Dry’s book *Waters of the World* tells the story of how the oceans entered the framework of planetary-scale environmental knowledge and provides in-depth discussions on particular actors in this historical process. It also contains a chapter on Willi Dansgaard and ice core drilling, thereby connecting the activities in oceanography and glaciology in the postwar era.¹⁰⁷

Tracing the history of the “Anthropocene Ocean” has been the subject of several recent publications on ocean sciences and the environment, pointing towards how the ocean has become entangled with human activities in new ways with the rise of planetary-scale anthropogenic impact.¹⁰⁸ As Lino Camprubi points out, historians have mostly dealt with what has been unfolding on the ocean surface, rather than in its depths, but the turn towards the planetary has also become a turn towards the ocean depths. The ocean appears increasingly as a three-dimensional space rather than a two-dimensional surface on which history can unfold.¹⁰⁹ Deep environments are, in this perspective, human environments too.¹¹⁰ Within the humanities more broadly, calls for “blue humanities” connects to this notion of the oceans as a space which is intertwined with humanity, rather than an alien site outside the domain of culture and history.¹¹¹


Since this thesis will consider the Scandinavian context, the work of Vera Schwach, Bo Poulsen, Kristian Hvidfelt Nielsen and Urban Wråkberg is of particular relevance, with their focus on ocean sciences in Norway, Denmark and Sweden. The particular research environment in Gothenburg and its Oceanographic Institute has been written about by Staffan Bergwik and Peder Roberts (the latter focuses more on the Albatross expedition and the postwar activities at the institute). Even though this thesis is not primarily concerned with writing a new Scandinavian history of oceanography and glaciology, I still hope to be able to contribute to this body of scholarship by tracing the circulation of knowledge between scientific fields as well as across the North Atlantic region in the postwar era. A more transnational perspective can highlight how technologies, methods and research approaches travelled to and from Scandinavia during a formative time for paleoclimateology.

Beyond the domains of ice cores and deep-sea cores, historians have approached other fields concerned with proxy records and past environments, such as paleoecology. The wider history of environmental prediction and prognostication sometimes also covers histories of paleoclimateology and long-term climate reconstruction. Here, the history of individual proxy records and sub-disciplines connects with the history of global environmental knowledge and climate science more broadly. Scientific practices aimed at anticipating and prognosticating future environmental changes emerged gradually during


the twentieth century, and marked the beginning of the elaborate planetary-scale work of Earth Systems modeling present in contemporary Earth System Science and IPCC-reports.116 These developments were underpinned by the emergence of increasingly detailed proxy records and isotope dating methods, and while this aspect of the larger history of the global environment is often placed in the background, histories of climate modeling remains implicitly concerned with the history of proxy records.117 Jenifer Patricia Barton’s work on the Earth System Science Committee is an especially useful contribution to the scholarship on ESS, as it both provides a conceptual history of the Earth System itself as well as a rich empirical overview of the activities of NASA’s Earth System Science Committee in the 1980s.118

I also see this thesis as engaging with scholarship about how nature becomes governable. Even though these works do not explicitly deal with environmental temporalities, or their scientific underpinnings, they provide a setting in which proxy records were enrolled to global environmental governance.119 Similarly, scholarship on the complicated landscape of politics in the Anthropocene also provides historical and contemporary context of the role of ice- and deep-sea cores.120


Delimitations and archival material

This thesis is concerned with a large shift in the human-Earth relationship: the rise of planetary-scale environmental knowledge and politics. Even though I will focus on one particular aspect of this shift – the relationship between human and planetary times in postwar paleoclimatology – it is still a topic that could grow so large it would get impossible to handle within the scope of a single doctoral thesis. In order to make this manageable, and also as relevant as possible for my research objectives, some decisions on delimitations have been necessary.

It is perhaps easiest to start with what this thesis is not: it is not a comprehensive account of all developments in postwar paleoclimatology and it does not claim to present the only possible narrative of the rise of ice- and deep-sea core drilling. Nor does it assume that the planetary scale was invented in the postwar era or that previous ways of knowing the planet as a singular scientific object are irrelevant to the present Anthropocene discourse. Rather, the aim is to trace the epistemological, institutional and temporal frameworks in which ice- and deep-sea core drilling developed in the postwar era.

There are, I seek to show in the thesis, good reasons to focus on this particular period, as there are concrete connections – institutions, actors, instruments – between this history and the recent rise of the Anthropocene concept. My North Atlantic geographical focus (the first two chapters are more oriented towards Scandinavia and Greenland, and the two subsequent ones towards the United States) should not be understood as the only possible way of understanding this history or as a way to deem other geographic areas irrelevant. Instead, my aim with this geographical focus is to show two things. Firstly, it was a relatively small scientific community that were able to become spokespeople for the entire planetary system and successfully mobilize scientific practices, frameworks and technologies in the making of an Earth System approach that is today often taken for granted. Historians of the earth sciences have previously shown how local knowledge can become universal. Andrea Westermann, for example, in her study of Swiss geognosy in the late nineteenth century,

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121 The "Making Climate History Project" at the University of Cambridge traces the roots of contemporary climate knowledge back to the early nineteenth century and other historians have shown examples of early-modern forms of environmental thinking. E.g Lydia Barnett, *After the Flood: Imagining the Global Environment in Early Modern Europe* (Baltimore: Johns Hopkins University Press, 2019); Warde, *The Invention of Sustainability*. 

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highlights the dynamics between intimate knowledge of a particular territory and the ambition the aggregate data to a larger scale.\textsuperscript{122}

Secondly, my hope is that some of the theoretical work in this thesis, especially drawing on theory of history in the history of environmental knowledge, can be useful in contexts beyond the North Atlantic region in the postwar era.

My temporal delimitations are perhaps less surprising. I have decided to begin the story after 1945 and end it by 1990. Even though attempts to drill in ice and the ocean floor had occurred in limited fashion before 1945, ice- and deep-sea cores are quintessential products of Cold War science and the influx of funding to the geosciences at the time.\textsuperscript{123} The International Geophysical Year 1957–1958 marked, as several historians have already written about, the beginning of planetary-scale knowledge production in a wide range of disciplines, including glaciology and oceanography.\textsuperscript{124} Even though influential conceptualizations of the planetary-scale emerged before WWII, the particular scientific practices that this thesis is concerned with can’t be disentangled from the political and scientific geographies of the Cold War and decolonialization.\textsuperscript{125}

By starting in 1945, and particularly in the Scandinavian context, I will follow the increasingly planetary scope of ice- and deep-sea cores in the decades that followed and the scientific, technological and institutional changes that the expanded scope brought with it. The decades after 1945 also marked the beginning of modern environmental thought and I perceive of the history of ice- and deep-sea cores to run parallel, and the closer we come to present day, increasingly intermingled with this larger rise of “the environment”.\textsuperscript{126}

In the 1940s, the connections between ice- and deep-sea cores were not obvious to the scientists themselves, and much less obvious were the ways in which these technologies would become part of Earth System Science and the attempts to measure, project and govern the environment on a planetary scale. My approach is therefore to track two crucial proxy records – ice- and deep-sea cores – for Earth System modeling, to their scientific


\textsuperscript{126} Warde, Robin, and Sörlin, \textit{The Environment}. 
origins in glaciology and oceanography and the work that preceded their role in planetary-scale science and politics.

The story ends in 1990. At this point, Earth System Science had been institutionally, scientifically and politically established as a dominant mode of accounting for the planetary-scale environment. It marks the culmination of the longer process of translating ice and the ocean floor into coherent datasets that can function within the framework of global environmental sciences and governance. Given how much the Earth Systems approach still regulates and dictates contemporary environmental discourse, I also believe that this empirical material can speak to the theoretical aspects of my research objectives and this thesis’s engagement with contemporary discussions about the planetary scale.127

I follow the ice- and deep-sea cores in this transformational time for environmental knowledge, where they travelled, how they were mediated, visualized and enrolled into other scientific programs. Since they travelled across disciplinary boundaries, and into new research settings that were inconceivable just a few decades before, it is difficult to stay within just oceanography and glaciology. In order to follow the cores, I therefore consider them in the different settings in which they appeared. The first two chapters follow early ice- and deep-sea core drilling activities in Scandinavia and the circulation of knowledge, technologies, and methods between and across the North Atlantic. The next two chapters are more oriented towards the United States and the evolution of ice- and deep-sea core drilling in U.S institutions and the emergent concern for the global environment. The last chapter will rather consider the cores in a mediated format, as abstracted datasets travelling across geographical and disciplinary boundaries in the formation of Earth System Science. A key assumption here is that the cores appear in different stages of mediation during the decades under study, and are therefore not bounded to particular experts or scientific disciplines.

Empirically, I rely mostly on archival material, from both institutional and personal archives. For the first two chapters, which mostly concerns Scandinavia, several personal archives serve as the empirical foundation for the research. The archive at the University Library of Gothenburg keeps the personal archives of oceanographers Börje Kullenberg and Hans Pettersson, who were leading early deep-sea core drilling efforts in Sweden and their papers make up significant parts of the material for the second chapter. The German physicist Hilde Levi, the initiator of the Isotope Colloquium in Copenhagen, has her personal papers stored at the Niels Bohr Institute Archives in Copenhagen and this collection has also been used for the first chapter.

For the third chapter, I have used material from Willi Dansgaard’s personal archive at the University Library in Copenhagen as well as archival material from Dartmouth College, where Dansgaard’s early collaborator David C. Nutt worked. The material consists of both drafts of research articles and proposals as well as correspondence.

In the fourth chapter, on the institutionalization of deep-sea core drilling in Cold War U.S. oceanography, I rely on several different archives: The Deep-sea Drilling Project’s institutional archive at Scripps Institution for Oceanography in La Jolla, California, the personal papers of the British paleoceanographer Nicholas Shackleton, in the Royal Society Archives in London, as well as the personal papers of oceanographer Maurice Ewing, stored at the Briscoe Center for American History in Austin, Texas. The archives of Shackleton and Ewing have been utilized not so much to study them as individual scientists, but rather to map their scientific networks and institutional contexts.

The last chapter also draws on the archives of Ewing and Dansgaard, but particularly on the internal records from NASA’s Earth System Science Committee, which is stored at the Archives of the National Center for Atmospheric Research in Boulder, Colorado. This chapter also draws on published material, mostly scientific articles and books, in addition to the archival material.

Chapter outline

The thesis will follow a chronological order, beginning in the 1940s and ending with the establishment of Earth System Science in the late 1980s, although with some overlapping between the chapters. By following ice cores and deep-sea cores from their first materialization in the 1940s and 1950s – along with the rise of isotope dating methods – until their full implementation in computerized Earth System modeling, the ambition is to follow the materials from the cryosphere and the ocean floor into the abstract models of ESS. The chapters will focus on particular actors, technologies and sites with different roles in this larger process.

The Albatross and the Colloquium: Making material times in Scandinavia, 1945–1957

The first empirical chapter focuses on two contemporary and partly overlapping scientific efforts in Scandinavia in the immediate years after World War II: The Swedish Deep-Sea Expedition (or the Albatross Expedition) of the Gothenburg Oceanographic Institute and the Isotope Colloquium held at the Zoophysiological Laboratory at the University of Copenhagen. The Albatross expedition was focused on accessing sediments from the ocean
floor around the globe and developed crucial new technologies for deep-sea core drilling. The samples from the expedition were circulated among scientists and the novel isotope dating technologies were applied to the samples taken from the *Albatross*. The Isotope Colloquium in Copenhagen sheds light on the interdisciplinary discussions on how, why and where isotope dating could be utilized. Together, these two efforts, point towards the methods and technologies that rendered new material times possible in postwar Scandinavia. The chapter primarily focuses on two key actors in this history: Gothenburg oceanographer Hans Pettersson and Copenhagen physicist Hilde Levi.

**Old water: Willi Dansgaard and isotope glaciology, 1952–1966**

This chapter follows the implementation of isotope dating methods in glaciology, and in particular, the early work of ice core scientist Willi Dansgaard in Copenhagen and Greenland. In particular, one episode in Dansgaard’s early career, where he collaborated with the physiologist Per Scholander, is the main focus of the chapter. It argues that, ice core drilling and stable isotope dating of air bubbles inside the cores, did not self-evidently belong in the cryosphere. Rather, Dansgaard and Scholander developed their methodologies in dialogue with both geophysical and geochemical frameworks as well as research in the life sciences. The chapter also considers different ways of conceptualizing time in glaciology and the scientific and geopolitical infrastructure, which rendered the work of Dansgaard possible.


The third empirical chapter picks up where the first left off and traces how the samples from the *Albatross* expedition were utilized in new contexts in the United States. Knowledge, materials, and personnel left Gothenburg for the U.S in the 1950s and were part of establishing new research projects on the history of the oceans. The first half of the chapter follows the institutionalization of deep-sea core drilling at Lamont-Doherty Geological Observatory and Scripps Institution of Oceanography as well as the establishment of JOIDES (Joint Oceanographic Institutions for Deep Earth Sampling) in the 1960s. The second half of the chapter focuses on two projects that relied on deep-sea core drilling in the 1970s: The DSDP (Deep-sea Drilling Project) and CLIMAP (Climate: Long range Investigation, Mapping, and Prediction). Both these projects drew on deep-sea core timescales in order to connect them, in different ways, to questions concerning the environment and agricultural and economic prognostications.

This chapter considers ice cores, after the milestone drilling at Camp Century, Greenland, in 1966, were increasingly circulated around the globe – both as material samples and abstract datasets. The multiple forms of mediation ice cores underwent between the 1960s and the 1980s, this chapter argues, enabled the ice cores to travel from a remote existence in the cryosphere into the models of Earth System scientists. It also seeks to situate ice core data, as well as other paleoclimatological data, in the burgeoning Earth System modeling taking place in the early 1980s. By zooming in on the work of NASA’s Earth System Science Committee, the chapter shows how ESS made use of paleoclimatological data, and simultaneously broadened and narrowed the epistemological and political scope of proxy records.
In a radio studio in Gothenburg, in the spring of 1943, the guests of Julius Rabe’s show were upbeat. They were recording a two-part program about the “mysteries of the deep-sea” and various experts from the city’s university had been invited to tell the listeners about state of the art research within marine biology and oceanography. The uncertainties about the deep-sea were vast, its temporal and spatial properties largely unknown, and actually researching this distant geography, the guests agreed, was an immense challenge. Still, optimism was in the air in the Gothenburg studio. New opportunities were lining up ahead. As the show was coming to an end, Julius Rabe exclaimed: “As a Swede, I can say that it would be an honor for our country if this unique tool could be used to solve the riddles of the deep-sea”. What was the source of their optimism? What was this tool Rabe was referring to?


130 Manuscript for radio show, 1943, 9.
In the radio program, Hans Pettersson, professor at the Oceanographic Institute in Gothenburg, and Börje Kullenberg, associate professor at the same institution, had told the other guests about their plans to conduct a circumnavigation of the Earth. A research endeavor of that kind had not been done within Swedish oceanography before and marked an opportunity to put Sweden on the map in the world of deep-sea research. What made the prospects of this expedition especially appealing to the other guests were the new sampling technology Börje Kullenberg had developed: the piston corer. With this tool, Pettersson and Kullenberg could take samples of the ocean floor that reached several meters down into the seabed. Previous attempts at sampling the ocean floor had remained on a surface level, and by reaching further down, Pettersson and Kullenberg explained, much longer geochronologies of the ocean floor could be made visible. The new piston corer was not the only source of excitement. Pettersson also discussed how new dating technologies, made possible through studying radioactive decay and the half-life of radioactive substances in the deep-sea sediments, could provide more exact timelines that reached even further back in time. The ocean floor had not only been a spatially distant geography, but also a place devoid of temporality, a space without a clearly defined history or future. Drilling down through the seabed, Pettersson and Kullenberg hoped, could change the picture.

In this chapter, I am going to trace these two technologies – the piston corer and isotope dating – in two Scandinavian research projects between 1945 and 1957. During these years, new connections were drawn between field work in the deep ocean on the one hand, and laboratory work on radiometric and other isotopic dating methods on the other. New sampling technologies, such as the piston corer, and recent developments in geochemistry and nuclear physics enabled new geochronologies to take form. I focus on the already mentioned Swedish Deep-sea Expedition (1947–1948) – or the Albatross expedition as it is often referred to – and the Isotope Colloquium at the Zoophysiological Laboratory at the University of Copenhagen (1948–1951). Even though these two scientific endeavors differed significantly – one was a global oceanographic expedition and the other a series of seminars on isotope dating methods – they were both parts of an interdisciplinary mobilization of knowledge and resources towards making new geochronologies in areas that had previously lacked coherent timescales.

The chapter is divided into three core parts: one is about the preparations for the Albatross expedition, one considers the Isotope Colloquium and its surrounding scientific context in Copenhagen, and one draws the two of them together and looks at how isotope dating and deep-sea drilling enabled the formation of the new field of paleoceanography as

130 Ibid, 7.
the *Albatross* returned home again. My aim with this division is to show how both field- and laboratory work were instrumental to the making of a temporalized deep ocean and how the interplay between them came to shape new kinds of ocean science temporalizations. There were some contacts between the oceanographers in Gothenburg and the geochemists and physicists in Copenhagen, particularly between August Krogh and Hans Pettersson, but the two projects unfolded mostly independently of each other. It was only later, in the early 1950s, when oceanographers from both Scandinavia and the United States connected the dots in a more explicit way. They brought together the cores from the *Albatross* with oxygen isotope dating, in order to determine past temperature trends, and the connections became more evident.

The goal of this chapter is to consider the work done in Gothenburg and Copenhagen not just as important scientific efforts in themselves, but also as early examples of a postwar interest in making and synchronizing planetary timescales. Emily M. Kern argues that the history of isotope dating technologies, and especially radiocarbon dating have, despite its origins in nuclear physics and the Manhattan project, remained somewhat hidden in the history of Cold War science. She attributes this to the institutional settings in which the dating was conducted: isotope dating did not become, like many other geo- and environmental sciences at the time, a military priority, instead most work was conducted in archaeological and geological research environments. The Zoophysiological Laboratory in Copenhagen was one such research environment. Kern especially highlights the importance of isotope dating for archeology, but, as I will elaborate on in this chapter, this development also affected oceanography.

The temporalization of the ocean floor would become crucial for establishing knowledge about the planetary-scale climatic system in the 1960s and 1970s, as I elaborate on in chapter four. However, when the *Albatross* set sail in 1947 or the Isotope Colloquium initiator Hilde Levi sent out her invitations in 1948, this larger project was still out of view. The exact utility of isotope dating methods was yet to be established and the prospects of deep-sea core drilling were still prone to uncertainty and technological difficulties. The rise of these technologies coincided with broader efforts to map and predict the circulation and dynamics of the world ocean. As several historians of ocean sciences have pointed out, the 1950s would mark the beginning of a Cold War surge of funding and interest in the oceans,

especially in the United States. Jessica Lehman argues that a particular kind of ocean was produced during this time: it was planetary in scope and understood as a coherent, singular object of knowledge. Through efforts such as the International Geophysical Year 1957–1958, coordinated data practices distributed across different geographies enabled a planetary-scale view and situated the world ocean within a burgeoning understanding of a planetary system.

This making of a world ocean is often conceived of as a process of spatial connection and imagination, centered round Cold War oceanography in the United States. However, conceiving of the world ocean as dynamic space also necessitated temporalization and the establishment of technologies that could measure changes in the world ocean over time. Placing these technologies at the center brings alternate epistemic geographies of ocean science to the foreground.

Jacob Darwin Hamblin and Vera Schwach have shown how oceanographic technologies, epistemologies, and methods developed in Norway, and to a smaller degree Sweden and Denmark, were exported to the United States in the 1950s. It is, in other words, nothing new that Scandinavia was a crucial context for the development of twentieth century oceanography. Yet, the *Albatross* expedition and the Isotope Colloquium only partly fit in this history: they build upon this longer tradition, but they also bring together new institutions, practices and temporal sensibilities that have previously existed mostly outside the history of oceanography.

**Finding time in the oceans: a prehistory of the *Albatross* expedition**

Even though humans have interacted with the ocean since we first emerged as a species, the history of the ocean floor as a scientific object is quite short. Up until the nineteenth century, the deepest parts of the oceans were out of reach, appearing as infinitely deep and

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spatially vague. By the late eighteenth century, the common view in Europe of the deep seas was that of a featureless and empty space. The ocean floor itself was at times not perceived as a discrete object at all, but as gradually emerging the deeper one went: if a sailor would fall over board, one common theory asserted, his body would never reach solid ground, instead, it would float at the depth where the density of the body would match that of the salinity of the ocean.\textsuperscript{139} The making of an ocean floor occurred largely for commercial reasons: the prospect of laying telegraph cables across the Atlantic necessitated some knowledge about what was beneath the surface.\textsuperscript{140}

Technologies for probing the ocean floor were, by the mid-nineteenth century, still quite rudimentary in their construction. Dredging was one early form of sampling technology that allowed scientists to get a glimpse of the contents of the ocean floor.\textsuperscript{141} Depth soundings, which had existed before the nineteenth century as well, became more systematic and were translated into charts, which could serve as a mediated ocean floor for sailors.\textsuperscript{142} Steam powered winches, wires rather than ropes, and mechanic sinkers all made the deep ocean technologically more accessible when they emerged in the late nineteenth century as more advanced ways of sensing the ocean floor. Oceanography was gradually coming together as a coherent scientific endeavor. It could serve as a tool for expanding the spatial dimension of commercialism and imperialism from the terrestrial to the aquatic.

The British \textit{Challenger} expedition (1872–1876) was the first expedition equipped to exclusively focus on the ocean depths and to make a global-scale sounding of the ocean floor. Over the course of four years, over 400 soundings were made, and marked the beginning of the transformation of the abyssal ocean depths into numbers, charts, and diagrams.\textsuperscript{143} The rise of acoustic sounding technologies in the early twentieth century expanded the possibilities of mapping the ocean floor, and further opened up the imperial scramble for resource extraction at sea. Five decades after \textit{Challenger}, the German \textit{Meteor} expedition (1925–1927) was the first oceanographic expedition to utilize acoustic sounding technologies in a systematic fashion, following a strict sampling grid set up in advance by the expedition leader Alfred Merz.\textsuperscript{144} The \textit{Meteor} expedition used more

\begin{itemize}
\item \textsuperscript{140} Rozwadowski, \textit{Fathoming the Ocean}, 77-78.
\item \textsuperscript{141} Antony Adler, \textit{Neptune’s Laboratory: Fantasy, Fear, and Science at Sea} (Cambridge, MA: Harvard University Press, 2019), 23.
\item \textsuperscript{142} Rozwadowski, \textit{Fathoming the Ocean}, 70.
\item \textsuperscript{143} Sabine Höhler, “Creating the Blue Planet from Modern Oceanography,” in Franziska Torma (ed.), \textit{A Cultural History of the Sea in the Global Age} (London: Bloomsbury Academic, 2021), 28.
\end{itemize}
advanced sampling technology and a multi-dimensional set of measuring tools (deep-sea thermometers, current meters, water samplers, closing nets, bottom samplers, and coring tubes) in comparison to its predecessors. Through these technologies, a three-dimensional dynamic ocean came together as a scientific object, which diverged from the previous vertical and two-dimensional conceptualization of the oceans.\textsuperscript{145}

The scientists on the \textit{Meteor} expedition were theoretically informed by the Norwegian physicist Vilhelm Bjerknes and his theorem of atmospheric and oceanic motion. This brought an additional aspect: not only was the ocean three-dimensional, it was also in motion, and a key task became to trace ocean currents and follow the movement of ocean drifts. The ocean was, albeit in a rudimentary fashion, acquiring a temporal dimension, and the instruments, such as the Ekman current meter (developed by the Swedish oceanographer Vagn Walfrid Ekman in 1903) responded to this imperative to track changes in the ocean.\textsuperscript{146} Onboard the \textit{Meteor} was also the Finnish chemist Karl Buch, whose early interest in the carbonate system in the sea had brought him there. He later shared his findings from the \textit{Meteor} with the Swedish meteorologist Carl-Gustaf Rossby and sparked his interest in the role of carbon dioxide in the ocean-atmosphere exchange.\textsuperscript{147}

The research questions posed by this three-dimensional dynamic approach were different compared to earlier descriptive ones from the nineteenth century. Rather than sampling the ocean for particularities, individual measurements that could underpin topographical charts of the ocean floor, the three-dimension approach posed generalized questions about how water masses circulated or how the oceans interacted with the atmosphere.\textsuperscript{148} By the end of the 1930s, the ocean began to come together as a scientific object in its totality, spanning biological, chemical and physical forms of knowledge. In their 1942 overview of the field, \textit{The Oceans: Their Physics, Chemistry, and General Biology}, Harald Ulrik Sverdrup, Martin Johnson and Richard Fleming, stated that “since

\textsuperscript{145} Höhler, “Creating the Blue Planet,” 32.
\textsuperscript{146} Magnus Vollset, Rune Hornnes, and Gunnar Ellingsen, \textit{Calculating the World: The History of Geophysics as Seen from Bergen} (Bergen: Fagbokforlaget, 2018).
\textsuperscript{147} Bert Bolin, “Carl-Gustaf Rossby: The Stockholm Period, 1947–1957,” \textit{Tellus A: Dynamic Meteorology and Oceanography} 51:1 (1999): 10. Apart from Bolin’s recollection in this article, I have not encountered this fact in any of the historical literature on the \textit{Meteor} I have come across. It seems to point towards that the \textit{Meteor} has been more influential in the history of climate change knowledge than the current historiography suggests. Maria Bohn has discussed Buch’s role in early CO\textsubscript{2} measurements in Scandinavia and pointed out how a scientific infrastructure around CO\textsubscript{2} measuring grew in the early twentieth century Scandinavia. It seems possible to further consider how oceanographic institutions and expeditions fit in this history. Maria Bohn, “Concentrating on CO\textsubscript{2}: The Scandinavian and Arctic Measurements,” \textit{OSIRIS} 26:1 (2011): 165-179.
\textsuperscript{148} Eric L. Mills, \textit{The Fluid Envelope: How the Study of Ocean Currents Became a Science} (Toronto: University of Toronto Press, 2009)
1900, great advances have been made within all of the marine sciences, and the contacts between the special fields have become more and more intimate.”

During this time, Swedish oceanography had evolved from mostly concerning fisheries research in the late nineteenth and early twentieth centuries to encompass a broader research program involving synoptic methods and physical, chemical, and biological studies of the ocean. It was also very much a family affair. In the late nineteenth century, the two chemists Gustaf Ekman (a relative of Vagn Walfrid Ekman who invented the Ekman current meter in 1903) and Otto Pettersson (father of Albatross expedition leader Hans Pettersson) were foundational figures for Swedish oceanography. They were also able to utilize their significant monetary and scientific capital to establish a Swedish oceanographic research program. For their own money, they founded a research station at Bornö in 1902, on the coast north of their hometown Gothenburg, thereby further strengthening the institutionalization of oceanography in Sweden, and Gothenburg in particular. By 1939, Gothenburg had its own Oceanographic Institute, Sweden’s first professorship in oceanography (held by Hans Pettersson) and a strong local political and commercial support for further oceanographic research.

Hans Pettersson, who by the late 1930s was the leading figure of oceanography in Gothenburg, was following in his father’s footsteps. Otto Pettersson had raised his son to inherit his scientific project and Hans had helped him on the research station in Bornö since he was a child. Hans went on to do an undergraduate degree at Uppsala University and a PhD at the University College in Stockholm. His father’s friend Svante Arrhenius served as his supervisor. At the time, Arrhenius worked with what he called “cosmic physics”. It was a predecessor to geophysics, but also included cosmology and astrophysics. For Hans Pettersson, Arrhenius’ approach opened up research questions regarding the utility of nuclear physics and radioactivity in the study of the interactions of the ocean and the

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151 Wråkberg, “Om djuphavets gåtor,” 103. See also: Artur Svanson, *Otto Pettersson: Oceanografen, kemisten, upfinnaren* (Göteborg: Tre Böcker Förlag, 2006). The founding of Bornö as well as the broader institutionalization of Oceanography was typical of the family centered way of conducting and legitimizing science. Family relations were a structuring force in the establishment of oceanography in Sweden as well as the academic infrastructure more generally. See: Staffan Bergwik, “Father, Son, and the Entrepreneurial Spirit: Otto Pettersson, Hans Pettersson and the Early Twentieth Century Inheritance of Oceanography,” in Staffan Bergwik, Donald L. Optiz, and Brigitte van Tiggelen (eds.), *Domesticity in the making of modern science* (London: Palgrave Macmillan, 2015), 192-214.

152 Bergwik, “Father, Son, and the Entrepreneurial Spirit,” 205.

153 Ibid., 199.
After his PhD, Hans tried to gain independence from his father’s influence. He took up a position at the Radium Institute in Vienna in 1921 in order to explore his interests in radioactivity. He remained there until 1928. During his years in Vienna he tried, but failed, to run his own research group in nuclear physics. When a new position in oceanography was advertised in Gothenburg, he returned somewhat reluctantly to the family business at the University of Gothenburg. But even though Hans Pettersson tried to distance himself from his father, Staffan Bergwik argues that Hans nevertheless came embody the entrepreneurial style of fundraising and institution building that his father had spearheaded. At the same time, Hans was also staking out his own research program at the intersection of oceanography and nuclear physics.

Hans Pettersson’s ability to acquire funds from the local elites in Gothenburg and his theoretically informed research framework from nuclear physics, crystallized in the idea to do a circumnavigation aimed at sampling the ocean floor. The Albatross expedition would extract deep-sea cores from around the world and scale up the work of the Gothenburg Oceanographic Institute to the planetary level. Radioactive substances in the cores could, Pettersson hoped, be a resource for writing longer geochronologies of the ocean floor than had previously been possible (see fig. 2.1). The institutional context was not as innovative as the theoretical framework: in Peder Roberts’ words, the Albatross expedition was “emblematic of the pre-1939 age”. It had an almost entirely Swedish crew, a Swedish ship, and was heavily reliant on local patronage. Hans Pettersson himself held somewhat idiosyncratic and romantic ideas of circumnavigation and adventure at sea. He often looked to the Challenger expedition and other heroic predecessors when he discussed and planned the Albatross expedition.


Figure 2.1. An estimation of the increase of temporal scope made possible by the piston corer. The timescales should be understood as primarily rhetorical, as Pettersson did not have enough data to make global averages on the age of ocean sedimentation. Rather, this chart aims to highlight the novelty of the Kullenberg corer and the possibilities Pettersson saw when being able to drill deeper. The chart appears in Pettersson, *Med Albatross över havsdjupen*, 17.

Figure 2.2. A sketch of the piston corer as it was presented in 1944. Börje Kullenberg and Erik Fromm, "Nya försök att upphämta långa sedimentprofiler från havsbotten," 502.
Another key motivation for the Albatross expedition was the advancement in technological equipment. Börje Kullenberg, associate professor at the Oceanographic Institute, had spent the 1930s and early 1940s developing a new sampling tool. The piston corer (kolvlodet, see fig. 2.2) – and its predecessor the vacuum corer (vacuumlodet) – enabled far longer deep-sea cores, taken from greater depths.\(^{157}\) The vacuum corer proved effective, but was not a feasible technology in deeper waters, as the vacuum could not withstand the pressure from the water masses above. The invention of the piston corer is often attributed to Kullenberg himself, but in his first publication on the new corer he noted that the construction had already been tested in a different setting by a mechanic named Axel Jonasson. There were some discussions on how big of an influence Jonasson was, who designed the corer in his role as a mechanic at Statens Järnvägar, the state owned Swedish railway company, and whether he in fact could be attributed with the invention. Jonasson, who had no academic affiliation, does not appear again in Kullenberg’s publications, even though he later joined the Albatross expedition as a mechanic.\(^{158}\) In the Swedish press, the corer was often referred to as the “Kullenberg corer”.\(^{159}\)

After a few successful test drillings in the waters outside Bornö, Pettersson and Kullenberg acquired funding for a “trial run” in the Mediterranean, seeking to try the piston corer on greater depths. The trip was a success. The expedition team were able to acquire some successfully drilled sediments as well as further specify their research agenda for the upcoming Albatross expedition.\(^{160}\) The corer enabled them to approach the ocean floor as a vertical, solid entity beneath the water masses. Their project was not about mapping it as a spatial object, but instead translating it into timescales and the stratigraphic visual language of geology.\(^{161}\)


\(^{161}\) There had been previous attempts at drilling through the ocean floor. The Meteor expedition brought home some sediment samples and the American oceanographer C. S. Piggot made some attempts at systematic deep-sea core drilling the in the 1930s. But the cores were still small, some less than one meter, and on average 2.43 meters, which can be compared with the piston corer’s 20 meter cores. C. S. Piggot, “Foreword,” in United States Department of Interior Professional Paper 196-A, *Geology and Biology of North-Atlantic Deep-Sea Cores Between Newfoundland and Ireland* (Washington, D. C.: United States Government Printing Office, 1940).
The interior of the ocean, as Jacob Darwin Hamblin points out, cannot be seen with the naked eye, it “must be made comprehensible by some intervening technology – instruments, maps, equations. To borrow a term from James Scott, it must be made “legible”.”\(^\text{163}\) The piston corer, by expanding the vertical scope of the ocean, can be seen as one such instrument that enabled a different, temporally oriented, legibility of the ocean floor. It also opened the door for other ways of reading and sensing the ocean as a scientific object by enrolling dating methods from nuclear physics and geochemistry. In the next section, I will trace a different technology in the scientific endeavor to construct timescales out of the ocean floor: the rise of isotope dating methods.

The making of the Copenhagen Isotope Colloquium

In a 1945 letter to the Danish physiologist August Krogh, Hans Pettersson expressed his surprise regarding Krogh’s deep interest in and knowledge about geochemistry. Krogh had commented on a draft manuscript of Pettersson’s and his initiated comments caught Pettersson with surprise. “I am deeply embarrassed to have sent you an unfinished and not proof-read draft, I would not have done so if I knew you would read it so thoroughly!” he exclaimed and added, “I am very grateful for your interest.”\(^\text{164}\) At the time, Krogh was a Scandinavian scientific authority, having won the Nobel Prize in medicine in 1920 for his research on capillaries and the absorption of oxygen and elimination of carbon dioxide in the lungs.\(^\text{165}\) Krogh was not, however, only interested in the functions of the human body and had during his career worked with diverse topics that transcended the medical domain.

Already in 1902, when Krogh was a graduate student, he travelled to Greenland in order to study the respiratory exchange of organisms in the Arctic Sea. Once there, something else caught his attention. He had encountered the work of Svante Arrhenius, who in 1896 postulated that the surface temperature of the Earth depended on the heat absorption of atmospheric CO\(_2\) and decided to study the same respiratory mechanisms he saw in the animals on a larger scale. In a 1904 publication, Krogh presented his measurements of how oxygen in the atmosphere interacted with CO\(_2\), in a kind of planetary breathing process.\(^\text{166}\) He also noted that the amount of CO\(_2\) in the air was a lot higher in


\(^{166}\) Krogh was not the only physiologist with these kinds of research interests. In chapter three I explore the connections between physiology and geophysics in Greenland research in the 1950s.
Greenland than in areas further south. He speculated that the “combustion of coal by man” could raise atmospheric temperature in the future.\textsuperscript{166} After his 1904 publication, Krogh scaled his research objects back down again – his dissertation was on oxygen uptake and gas exchange in frogs – and became a leading authority in Danish medicine and physiology.\textsuperscript{167}

Over the course of his career, Krogh, often working in close collaboration with his wife Marie Krogh, explored a wide range of research topics in the fields of medicine and physiology, but he also maintained an interest in geochemical and geophysical processes. Through his supervisor and mentor, the professor of physiology Christian Bohr, he also came to know Christian’s son, the physicist Niels Bohr who became a professor of theoretical physics the same year as Krogh received his professorship in physiology in 1916. August Krogh had known the eleven years younger Niels Bohr since he was a child and had followed his rapid rise to fame within the field of theoretical physics in the 1910s.\textsuperscript{168} In the 1930s, when the two men already had significant scientific careers behind them – having received their Nobel Prices in 1920 and 1922 respectively – they began to interact again, but this time because of mutual scientific interests rather than through family gatherings.\textsuperscript{169}

Both of them had been introduced to the new promising research field of isotope research by the Hungarian chemist George de Hevesy, who had previously worked with Bohr in the 1920s, returned to Copenhagen in 1934 and acquired a position at Bohr’s institute. de Hevesy’s work in radiochemistry concerned the possibilities of producing “radioactive tracers”, isotopes that through their radioactive decay could be used to determine the time passed between known events. The term isotope comes from the Greek words for “same” (isos) and “place” (topos) and refers to elements of the same kind, but with different atomic masses, that is, different number of neutrons in the nuclei. The mass difference does not affect the chemical properties of the element, but the physical behavior.

\begin{footnotes}
\item[166] August Krogh, “The abnormal CO\textsubscript{2}-percentage in the air in Greenland and the general relations between atmospheric and oceanic carbonic acid,” \textit{Meddelelser om Grønland} 26 (1904): 412.
\item[169] Even though, this affiliation through family cannot be fully separated from the scientific interests. As Staffan Bergwik argues, the family – especially in elite, upper class settings – was a crucial institution for knowledge production in Scandinavia in the early twentieth century. Staffan Bergwik, Donald L. Optiz, and Brigitte van Tiggelen, “Introduction: Domesticity and the Historiography of Science,” in Staffan Bergwik, Donald L. Optiz and Brigitte van Tiggelen (eds.), \textit{Domesticity in the making of modern science} (Basingstoke: Palgrave MacMillan, 2015), 1-15.
\end{footnotes}
While stable isotopes stay the same over a vast amount of time, unstable – or radioactive – isotopes decay in a predictable way over time.\(^{170}\)

By utilizing radioactive isotopes with well-known decay processes, de Hevesy showed, the current amount of the isotope could reveal how much time had passed since an initial state. But the exact applications of this technology were not determined when de Hevesy first experimented on it in the 1910s and developed his methods in the subsequent two decades. Krogh, whose interest in physiology made him curious about the prospects of using radioactive tracers to follow biological processes in the body, saw the possibilities of launching a larger research program on isotope analysis in biological research.\(^{171}\) In a 1937 article, he wrote: “It is my task today to present some thoughts about a new and, as I believe, extremely powerful tool in biological and biochemical research: A small number of isotopes which can be readily distinguished and quantitatively determined by relatively simple physical means.”\(^{172}\)

By the mid-1930s, Krogh, de Hevesy, and Bohr had established a research node for “physico-biological” research at Bohr’s institute in central Copenhagen. Drawing on Bohr’s scientific clout, they had been able to acquire funds for a cyclotron, a kind of particle accelerator that can artificially produce isotopes, from the Rockefeller Foundation. Bohr had long wanted to have one in order to conduct experiments in theoretical physics, but never been able to acquire the funding for it in Denmark. The turn towards biological research opened up new funding prospects, and the cyclotron strengthened Copenhagen’s position in comparison to other institutions for isotope research in Europe.\(^{173}\) In 1938, the first international conference in Physico-Biological research was held in Copenhagen, and further institutionalized the new disciplinary connections that the turn towards isotopes had made possible.\(^{174}\)

Bridging disciplinary divides was, however, still a challenge for the three collaborators. de Hevesy was committed to expanding his research methods into biology and other scientific fields, but his lack of knowledge in these fields made it difficult for him

\(^{170}\) The most well-known example of this is the carbon-14, an isotope with eight neutrons rather than six, which is the amount of the stable carbon-12. Living organisms contain a fixed amount of carbon-14, which starts decaying after death. Knowing this, as well as the carbon-14 half-life of about 5730 years, the amount of carbon-14 can be used to determine the age of organic material.


\(^{174}\) The participant list from the conference is kept at the Niels Bohr Institute Archive (hereafter NBI), Bohr Institute Administrative Records, vol. 5.
to convey the benefits of his research.\textsuperscript{175} Krogh also had trouble convincing his colleagues in medicine and physiology that isotope research was a valuable research field for them.\textsuperscript{176} In order to bridge the disciplinary divides, and produce a more coherent research program that could gather all sciences which could benefit from the isotope analysis framework, the German physicist Hilde Levi was given the task to coordinate the work. Levi had arrived in Copenhagen in 1934 as a refuge from Germany and had before she left completed her PhD in physics in Berlin. Niels Bohr hired her at his institute as a research assistant. When de Hevesy began his work in Copenhagen the same year, Bohr suggested that Levi should work as his assistant and help him develop his theory on radioactive tracers.\textsuperscript{177} Together, they established a research environment for interdisciplinary studies on isotopes at the Institute for Theoretical Physics in Copenhagen, and also, just before the war broke out in 1939, established important international connections.\textsuperscript{178}

World War II, and the German invasion of Denmark, interrupted the work at the institute. Levi, who was Jewish, decided to flee to Sweden and de Hevesy and Krogh did the same. In 1943, de Hevesy was awarded the Nobel Prize in chemistry for his work on radioactive tracers and was offered Swedish citizenship, which he decided to take and thereby lose his Hungarian one. As the war ended, de Hevesy chose to remain in Stockholm and never took up his position in Copenhagen again. Levi, however, went back to Copenhagen as soon as the war was over. The work could not be conducted the same way as before with de Hevesy living in Stockholm. de Hevesy, Krogh and Bohr agreed that the isotope work should move from the Institute of Theoretical Physics to Krogh’s Zoophysiological Laboratory. Krogh suggested that Levi should take a position at the Zoophysiological Laboratory, in order to keep up her research, but also to utilize her background in physics to become an intermediary between physics and biology.\textsuperscript{179} Since de Hevesy first started working on isotopes some three decades before, a lot had happened in the scientific domain: new research on radioactivity, stable isotopes, technological developments, and the interruption brought by the war, had rendered some of de Hevesy’s work obsolete. Levi decided to look abroad for inspiration and possible collaborations. Her previous collaborator James Franck, a physicist who had also worked at the Institute of Theoretical Physics, was based at the University of Chicago, as were the leading figures in

\textsuperscript{175} Hilde Levi, \textit{George de Hevesy: Life and Work} (Copenhagen: Rhodos, 1985).
\textsuperscript{176} Schmidt-Hansen, 192.
\textsuperscript{177} Levi, \textit{George de Hevesy}, 79.
\textsuperscript{178} Ibid., 80–81.
\textsuperscript{179} Ibid., 107.
isotope research at the time: W. F. Libby, Harold C. Urey, Cesare Emiliani and Martin Kamen.\textsuperscript{180}

Levi was able to secure funding for a longer research visit in 1947 from the American Association of University Women. In his letter of recommendation for Levi’s application, de Hevesy especially emphasized the broad, interdisciplinary skills Levi had acquired by working with him, Bohr and Krogh and that a key objective for her visit was to get acquainted with the novel dating methods that had been developed in Chicago.\textsuperscript{181} In Chicago, Levi was trained in the recently developed carbon-14 dating method, which used the carbon isotopes $^{12}$C and $^{14}$C to date organic matter in organisms that were no longer alive, which opened up the possibility to use isotope dating in other disciplines, such as geology, paleontology, and archeology.\textsuperscript{182} Even though Levi had planned to work mostly with her previous mentor James Franck, she ended working with Libby and became trained in how to practically apply the carbon-14 method on organic materials.\textsuperscript{183} Levi also worked with Harold C. Urey, who developed methods to use stable (non-radioactive) isotopes, particularly oxygen isotopes, for isotope dating. Stable isotope dating would become crucial for paleoclimatological research in the upcoming decade.\textsuperscript{184} Urey, in turn, visited Copenhagen in 1947, further establishing the connection with Chicago.\textsuperscript{185} When Hilde Levi returned home, she did so with new knowledge and research methods, but also an insight into how isotope dating could be used to date organic matter spanning vast periods of time.

Once back in Copenhagen, in 1948, Levi wanted to create a forum for researchers in different fields working with isotopes, especially since she had seen the prospects of this tool in Chicago. Together with Niels Bohr’s brother, Hans Bohr, a professor of medicine, she initiated the Isotope Colloquium, an interdisciplinary seminar series for all scientists with an interest in isotopes. de Hevesy came down from Stockholm to lead the seminars,

\begin{flushright}
\textsuperscript{180} John F. Marra, \textit{Hot Carbon: Carbon-14 and a Revolution in Science} (New York: Columbia University Press, 2019). This research group, affiliated with the Institute for Nuclear Studies, developed new isotope dating methods – most famously carbon-14 – as well as stable isotope research. Emiliani was the one among them who would later venture towards oceanography and paleoclimatology in particular.

\textsuperscript{181} George de Hevesy, Letter to the American Association for University Women, 13 November, 1946, Hilde Levi papers, NBI, vol. 2.

\textsuperscript{182} Marra, \textit{Hot Carbon}, 22-25.

\textsuperscript{183} Torkil Andersen, \textit{Datering av fortiden: Om det første danske kulstof-14 laboratorium} (Aarhus: Aarhus Universitetsforlag, 2007), 38.


\end{flushright}
but Levi was the one who did the organizing on the ground as well as choosing and inviting guests.¹⁸⁶ Between 1948 and 1951, fifteen Isotope Colloquium seminars were held.

They gathered a diverse group of scientists from medicine, chemistry, nuclear physics, geophysics, physiology, geology, and odontology from different departments of the University of Copenhagen.¹⁸⁷ On February 13th, 1951, Levi held a seminar on carbon-14 and the work of Libby and Urey that she had encountered in Chicago. The presentation sparked an intense discussion among the participants and it was agreed that Copenhagen, given the rapid developments of this method in the United States, should try to institutionalize its own carbon-14 laboratory in order to remain at the European forefront in isotope dating. The archeologists and geologists were interested, as were the physicists, biologists and chemists who were regular attendees at the seminars, but they lacked the institutional influence to mobilize enough resources to fund a new laboratory. Niels Bohr, who sat on the first row, the now famous story goes, stood up as the discussions were coming to an end and declared: “Carbon-14 dating is no longer a matter of belief, it is an established fact.”¹⁸⁸

The seminar led to the establishment of the carbon-14 laboratory in Copenhagen, in which Levi would become a key actor, and it further institutionalized the new connections made by isotope dating between physics, biology, geology, and archeology. The laboratory opened later in 1951 under the leadership of August Krogh’s successor Poul Brandt Rehberg and was the first of its kind in Europe.¹⁸⁹ Levi still stood for technical know-how, method development and international contacts, even though her research interest in using radioisotopes for medical purposes was less prioritized in this setting.¹⁹⁰ In 1959, the laboratory moved to the National Museum of Denmark, which marked a further turn towards archeology and Danish natural history.¹⁹¹ The isotope dating technologies were, after their initial interdisciplinary phase, finding a home in different disciplines and institutions, and became adjusted to fit the particular research agendas and objectives of the fields they were incorporated into.

¹⁸⁷ Andersen, 41.
¹⁸⁸ The meeting is described by Andersen, Datering av fortiden, 43-44.
¹⁹¹ Ibid., 194.
The institutionalization of isotope dating in Copenhagen, from the early meetings between Bohr, de Hevesy and Krogh to the inauguration of the carbon-14 laboratory, marked a period of disciplinary openness regarding the utility and use of isotope dating. Hilde Levi appears as a central figure in this history not primarily because of her research results, but through her ability to transcend disciplinary boundaries, create fora for collaborations and strong international alliances. By connecting Copenhagen and Chicago, Levi made circulation of knowledge between the two cities possible, which in turn further strengthened Copenhagen’s position among isotope research environments in Europe.\textsuperscript{192}

The connections manifested in the materials of the research as well: in a 1951 \textit{Science} article, J. R. Arnold and W. F. Libby relied on samples from Denmark which they dated using radiocarbon dating technology.\textsuperscript{193} Levi’s colleague at the National Museum, Jørgen Troels-Smith, had sent samples of charcoal and hazelnuts from West Zeeland in Denmark to Libby as a way not only to get an accurate dating of the materials, but also to maintain a close relationship with the Chicago research group.\textsuperscript{194}

Another aspect of the disciplinary openness at the Isotope Colloquium was how it allowed scientists, who engaged with time in different ways and on different scales, to meet and acquire knowledge about how to use isotope dating in their respective fields. Willi Dansgaard, who will be the central character of chapter three, was one regular attendee who in the 1950s and 1960s brought stable isotope dating to the realm of ice core drilling and glaciology.\textsuperscript{195} Johannes Iversen, Denmark’s state geologist and another frequent attendee at the Isotope Colloquium, brought perspectives from palynology and paleoecology, and also hosted international researchers interested in these topics. One example is Margaret Bryan Davis, an American paleoecologist, who later became president of the Ecological Society of America and worked with Iversen and his colleagues in 1953 as a visiting graduate student from Harvard.\textsuperscript{196}

The Isotope Colloquium appears as a “center of calculation” which mobilized institutional, economic, and scientific interest to the yet quite unspecified field of isotope research.\textsuperscript{197} The prospect of using radioactive decay, and later stable isotopes, to produce a

\begin{itemize}
  \item \textsuperscript{194} Andersen, \textit{Datering av forfriden}, 42.
\end{itemize}
geochemical quantification of geological time caught the interest of historically oriented sciences across disciplinary boundaries.

The oceans were drawn into the world of isotope dating too. The Danish marine botanist Einar Steemann Nielsen became aware of the work on isotope dating, and particularly carbon-14 conducted by Hilde Levi, and decided to try it on phytoplankton from seawater. In 1950–1952, Nielsen was part of the second *Galathea* expedition, a Danish deep-sea expedition which, similarly to the *Albatross*, had a global scope but national funding and personnel. Nielsen, with his sampling of phytoplankton on the *Galathea*, brought the Isotope Colloquium with him to sea and found practical applications for the theories that had been discussed in the Copenhagen seminars.

Hans Pettersson had a longstanding interest in the possibilities of radioactivity and nuclear physics for oceanography, drawing on his experience from the Radium Institute in Vienna, but didn’t apply the recent developments from Chicago and Copenhagen on his own deep-sea cores. Instead, the *Albatross* cores would become subjected to isotope dating by other researchers, primarily Cesare Emiliani in Chicago, independently of the research conducted in Gothenburg. By synthesizing the theoretical developments in isotope dating as well as using the cores from Pettersson’s circumnavigation, a new kind of temporally oriented research could take form. In the next section, I will return to the *Albatross* and its voyage and return home to Gothenburg. I will focus less on the expedition itself, and more on what happened afterward: how the cores were analyzed and distributed, the role of isotope dating, and the transfer of knowledge, personnel, and technology from Gothenburg to the United States.

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199 I have decided to not focus on *Galathea* in this chapter, despite the connections it had to the Zoophysiological Laboratory. The reasons for this are that the *Albatross* was more oriented towards geophysical research and deep-sea core drilling while the *Galathea* instead focused more on ecology, marine botany and zoology and that the connections between *Albatross* and later work in paleoclimatology in the United States are more closely interlinked. The expeditions were, however, quite closely connected and some of the equipment from the *Albatross* were bought by the Danes to be used at *Galathea*. August Krogh offered to serve as an intermediary between Pettersson and the Danish expedition team in order to facilitate the possibility of sharing resources between them. For an overview of the expedition, see: Kristian Hvidtfelt Nielsen, *På jagt efter søslangen: Galathea-ekspeditonen 1950-52* (Aarhus: Aarhus Universitsforlag, 2009). Krogh expressed his offer to help coordinate the expeditions in a letter to Pettersson: August Krogh, Letter to Hans Pettersson, 1945-12-02, 1945, GUL, Hans Pettersson papers, vol. 9, folder 1.

200 Hans Pettersson initially suggested that his professorship in Gothenburg should be in “Oceanography and Radioactivity” as a way to emphasize his background at the Radium Institute. Bergwik, “Father, Son, and the Entrepreneurial Spirit,” 205.
Albatross and its return to Gothenburg

After years of preparation, the *Albatross* left the harbor of Gothenburg on July 4, 1947. The scientific crew onboard consisted of, in addition to Hans Pettersson and Börje Kullenberg, the research assistants Nils Jerlow and Fritz Koczy, the young geologist Gustaf Arrhenius, (the grandson of Petterson’s supervisor Svante Arrhenius), zoologist Orvar Nybelin and mechanic Axel Jonasson. Several younger students, among them Hans Pettersson’s son Rutger, were also part of the expedition.²⁰² Hans Pettersson planned the route of the expedition drawing on scientific and historical interests: Pettersson chose drilling locations in part based on where the *Challenger* had gone seven decades before.²⁰³ The main activity during the fifteen months at sea was to sample the ocean floor for sediments, using the piston corer developed by Kullenberg and Jonasson. But the expedition members also conducted studies in ocean optics, marine biology, and hydrochemistry. A key incentive was to gather deep-sea sediments to bring back to Gothenburg, which in turn could be subjected to different forms of analysis. The work onboard the ship was intensive and physically demanding and the tough conditions in combination with strong personalities among the researchers strained relationships on the *Albatross*.²⁰⁴

The *Albatross* expedition itself had an anachronistic approach in the postwar era, with its nationally organized structure and romantic idea of circumnavigation.²⁰⁵ Scientists in adjacent fields – Hans Ahlmann in glaciology is one contemporary Swedish example – were keen to invent a new, modern and internationalist scientific persona. The adventurous and heroic explorer was fading away as a scientific ideal, but Pettersson nevertheless built his expedition upon the route of the *Challenger* expedition.²⁰⁶

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²⁰⁴ An example of this is visible in a letter Hans Pettersson wrote to Börje Kullenberg in 1948, during the expedition, which he handed over to Kullenberg as he did not believe they would be able to have a face-to-face conversation because of their “hot-tempered nature”. The letter conveys that Kullenberg had expressed that he regretted joining the expedition because of Pettersson’s “poor planning” and the Pettersson felt that Kullenberg, despite his talents, was the most difficult colleague Pettersson “had ever tried to collaborate with”. Hans Pettersson, Letter to Börje Kullenberg, 1948, unknown date, Hans Pettersson papers, GUL, vol. 7.
²⁰⁵ Roberts, “Traditions, Networks and Deep-Sea Expeditions After 1945,” 217
Figure 2.3. Outline of the Albatross travel route. Pettersson, *Med Albatross över havsdjupen*, 27.
Pettersson’s approach and ability to narrate dramatic descriptions of life at sea had been an asset in his fundraising for the expedition, but it could also sometimes make him overestimate the interest in his work. The *Albatross* was filled with deep-sea cores after having sailed around the world and Hans Pettersson expected massive attention from international scientists as the expedition returned home. However, the scope of the material turned out to be so vast that it was difficult to find someone with enough resources to take on all of the cores at once.

Individual scientists took care of the some of the cores and worked with just a few samples at the time. Gustaf Arrhenius, who at the time was young, ambitious and, through his family connections, able to work with the cores on his own, seized the opportunity. He used his father Olof Arrhenius’ laboratory facilities at the family estate Kagghamra outside Stockholm to analyze cores from the *Albatross* expedition. One of the publications that came out of Arrhenius work in Kagghamra provides insight into how how deep-sea cores enabled new connections between the ocean floor and theoretical developments in isotope dating methods. The article appeared in *Tellus*, a Stockholm based journal in geophysics founded in 1949, and was co-written by Arrhenius, W. F. Libby, and the computer scientist Göran Kjellberg from *Matematikmaskinnämnden* (The Swedish Board for Computing). It relied on the data from one single core, number 61B, which had been recovered by the *Albatross* expedition in the Central Pacific from a depth of 4437 meters. Even though it was just one core, the data intensive work in calculating radiocarbon activity in, which was the case of this particular core, pelagic chalk ooze, made Arrhenius and Libby to bring in increased computational capacity from *Matematikmaskinsnämnden*. BARK (*Binär Automatisk Relä-Kalkylator*), a computer developed domestically in Sweden, provided Arrhenius and Libby with the computational power needed analyze the core. By calculating radioactive decay in the carbonate record, the aim was to establish a timeline of the rate of sedimentation in the ocean floor. This particular paper rather sought to establish a method than argue for a coherent geochronology, but Arrhenius, Libby and Kjellberg nevertheless stated that they hoped that “vertical extrapolation” of core contents would in the future enable geochronologies dating back to the lower Pleistocene.

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208 Ibid., 21.
211 Arrhenius, Kjellberg, and Libby, 228
Through the article, the core was enrolled to a larger scientific infrastructure. It drew together the material contents of the core, the drillings conducted on the Albatross, isotope dating technologies, and computers as tools for data intensive calculations. How core 61B could travel to Kagghamra with Arrhenius and, in quantified form, to Libby in Chicago and BARK in Stockholm, in order to become a publishable dataset in a scientific journal points to the infrastructure needed to make timescales out of the ocean floor. It also points to the relative lack of large-scale research environments for this kind of scientific methods and its reliance on small elite circles. In the context of Swedish oceanography, the lack of resources posed a problem for processing the rich material gathered at during the Albatross expedition.

In a 1949 article, Hans Pettersson discussed different available methods for analyzing the cores from the Albatross expedition. He concluded that: “The different approaches to the chronology of the deep-sea deposits reviewed above will be tried out during the working up of the great material, 1640 meters of deep-sea cores, collected by the Swedish Deep-sea Expedition.” However, as the ad hoc work on the cores by Arrhenius indicates, doing this work in Sweden in a systematic fashion was nearly impossible. It would necessitate a different kind of research infrastructure. Just acquiring funds for one circumnavigation at the time would never suffice.

An example of the difficulties to carry out laboratory studies of the cores in Sweden is visible in an example from 1956. The graduate student Eric Olausson reached out to Börje Kullenberg, who had taken Hans’s position as leader for the Oceanographic Institute that year, to ask if there were any opportunities for him to work with Albatross cores at the institute. Kullenberg explained that he had tried to secure funding for a position in marine geology, but failed to do so, and that he did not have a position good enough for Olausson. Olausson ended up coming to Gothenburg anyway, working for a lower salary than his research experience warranted, because he claimed to be so passionate about the cores that he did not mind the pay cut.

Around this time in the United States, deep-sea core drilling was becoming part of the daily work at leading oceanographic research environments. Maurice Ewing at the Lamont-Doherty Geological Observatory in New York and Roger Revelle at Scripps Institution of Oceanography in La Jolla, California, were two major players in American


E. Olausson spent most of his career working with the Albatross cores and published results from his investigation continuously over 30 years. His work is summarized in: Eric Olausson, “Marina Sediment,” *Ymer* 103, (1983): 89-107.
oceanography who saw possibilities in deep-sea core drilling. Rather than showing interest in the actual cores from the *Albatross*, the American oceanographers imported the technologies that had made them possible, and then did their own studies with their own equipment and facilities. Ewing constructed his piston corer inspired by the Swedish original and began gathering deep-sea cores in 1947. In this sense, the *Albatross* expedition did leave a mark outside Gothenburg, but just not in the ways Pettersson had imagined.

Pettersson could not mobilize enough resources and infrastructure to establish a “library” of deep-sea cores in Gothenburg. The global scale enterprise of the *Albatross* expedition was difficult to repeat within a funding structure of local patronage. Ewing, on the other hand, was able to utilize multiple global-scale American naval operations to drill for cores, thereby quickly surpassing the work of the *Albatross*. Yet, this seemingly linear evolution of increasingly scaled-up perspectives and planetary-scale geochronologies was not as evident to the actors themselves by the mid-1940s. Rather, the temporal and spatial qualities of deep sea core research were continuously negotiated during this time, both in relation to institutional possibilities of doing the drilling as well as broader political and scientific conjunctures in the aftermath of World War II.

**Temporalizing the ocean floor**

By the early 1950s, the spatial scope of deep-sea core temporalities were increasingly taken for granted as encompassing the entire planet. Maurice Ewing’s efforts to create his “library” was from the outset underpinned by the urge to uncover a planetary view of the history of the ocean floor. But the early history of deep-sea core drilling in Scandinavia shows that the seemingly self-evident planetary perspective was not as hegemonic as appears in the 1950s and beyond. On the contrary, the times of deep-sea core geochronologies had to be actively synchronized with planetary-scale conceptualizations.

In 1944, Börje Kullenberg published his first scientific results from using his recently invented piston corer. It was published in the journal of the Swedish Geological Society, *Geologiska Föreningens i Stockholm Förhandlingar*, in which Kullenberg wrote the first

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215 Laurence Lipsett (ed.), *Lamont-Doherty Earth Observatory: Twelve Perspectives of the First Fifty Years, 1949–1999* (New York: Lamont-Doherty Earth Observatory of the Columbia University, 1999). The Lamont-Doherty Core Repository is still in use today and is the largest deep-sea core archive in the world. See: [https://www.ldeo.columbia.edu/core-repository](https://www.ldeo.columbia.edu/core-repository) (accessed 2021-09-18).

216 For an overview of how Ewing and the postwar American oceanographers approached global space, see: Naomi Oreskes, *Science on a mission: how military funding shaped what we do and don’t know about the ocean* (The University of Chicago Press: Chicago, 2021).
half and the geologist Erik Fromm (who would become the state geologist of Sweden in 1952) wrote the second. In their framing the utility of the piston corer existed primarily on the national level. In part this was due to the extraordinary circumstances during World War II which made it impossible for Kullenberg and Fromm to go anywhere outside Sweden to try the technology. But it was also due to the context in which they placed their work and the usage of the piston corer. Kullenberg situated their work in a national setting and the efforts of making Swedish geochronology and “connecting the Swedish glacial timescale to historical time”.217 Kullenberg and Fromm decided to test the piston corer in Ångermanälven in Northern Sweden precisely in order to connect deep-sea core data with local postglacial histories of the river. They were interested in local conditions, but also sought to recognize national patterns in sedimentation in Swedish lakes and rivers by comparing the cores with research results from Ragundasjön (Ragunda lake) and Fyrisån (Fyris river). The corer was placed in a longer Swedish tradition of geochronological research, stemming from the geologist Gerard De Geer and a nationally oriented research program from the early twentieth century.218 The synchronization that Kullenberg envisioned was one of different national timescales, both natural and cultural.219

Hans Pettersson, however, had a much more grand vision in mind. While Kullenberg was coring in Ångermanälven, Pettersson planned for his circumnavigation. He saw an opportunity to not only create planetary-scale geochronologies by sampling the ocean floor around the world, but also to connect this endeavor with the postwar internationalist ideals of the United Nations.

In 1946, a few months before the Albatross were to depart from Gothenburg, Hans Pettersson was as one of the invited speakers at UNESCO’s inaugural conference in Paris. He appeared along with intellectual celebrities like the philosophers Jean-Paul Sartre and A. J. Ayer, who were also talking at the event, and the program covered a range of philosophical, scientific and political topics. Pettersson’s lecture was entitled “The Submarine Underworld”. In it, he argued for the global reach of deep-sea core studies and pointed to how deep-sea cores were of interest to humanity, and not only ocean scientists.

Figure 2.4. A diagram showing the connection between deep-sea core data from the floor of Ångermanälven and a recent history of the amount of water in the river. In this way, the cores could be connected with the seasonal changes in the river. Kullenberg and Fromm, "Nya försök", 506.
As Pettersson put it: “In those catalogues of the deep, hidden under tens of thousands of meters of water, we may expect to find evidence of the great tectonic, volcanic and climatic catastrophes which this planet as undergone.” He told the story of how he had been able to see volcanic ashes in deep-sea cores from the Mediterranean and that these ashes could be traced back to the eruption of Pompeii 79 A.D. His deep-sea sediments were connected with the history of Western civilization. The submarine underworld could, in other words, become part of the one world, the one history, which Julian Huxley and UNESCO sought to realize.

At the same time, Pettersson was also keen to emphasize how the upcoming Albatross expedition was a distinctly Swedish, or even Gothenburg, operation. “I sincerely hope that this Swedish expedition, which for the most part will be using methods from Sweden, will induce other expeditions to explore the unknown depths of the sea-bed”, Pettersson put it toward the end of his lecture, making sure none would miss the local origins of his expedition. The quote reveals Pettersson’s commitment to the expedition mode of doing deep-sea research and how he both anticipated the increased interest in the ocean floor in the years to come, but at the same time did not foresee the institutional changes that would take place. As he would learn in the 1950s, the large scale operations of U.S. Cold War oceanographers did not rely on individual expeditions but rather on vast networks of observation and data gathering.

Nevertheless, Pettersson’s internationalist ideals made him appeal to Huxley’s larger ambitions and UNESCO decided to encourage the foundation of the Joint Commission on Oceanography of the International Council of Scientific Unions in 1948, which would also lead to the establishment of a more narrowly focused commission aimed solely at the study of the deep-sea floor in 1951. A year after, in September 1952, the newly formed Joint Commission on Oceanography held an international meeting in Monaco. It featured Hans Pettersson and Fritz Koczy from Gothenburg, Anton Bruun from Copenhagen, Roger Revelle from Scripps and Mary Sears from Woods Hole, among other leading oceanographers.  

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scientists. Harold C. Urey, whose work on isotope dating had taken him to Hilde Levi and Copenhagen in 1947, was present as well. The participants discussed the future of deep-sea research and, as John D. H. Wiseman, a scientist at the British Museum of Natural History, wrote in his report from the meeting: “...it would seem that many fundamental advances will be made during the next decade about the structure of the deep-sea floor, its geochemistry, and the origin of life. It was also considered that research in this field will influence many basic concepts in other fields of natural history.”²²⁵ Through the “inventive genius” of Börje Kullenberg, and the 20 meter long cores his piston corer enabled, the ocean floor had opened up as a potential source of planetary histories. Wiseman saw great opportunities ahead:

> It may be possible, when a sufficient number of long cores have been investigated in detail, to trace accurately with time the world’s past climatic changes, as well as giving some idea of the palaeocirculation both of the oceans and of the atmosphere. By investigations of this nature it may be possible to determine the causes of climatic change, as well as forecasting future changes.²²⁶

Even though Wiseman was open about this development being within the realm of speculation rather than imminent reality, he still conveyed the growing sense of spatial and temporal connection between the deep sea cores on the one hand, and planetary-scale dynamics on the other. Kullenberg’s piston corer, which Kullenberg had used to reconcile the sedimentation on floor of the river Ångermanälven with its postglacial history, was in this context becoming a technology which could speak to the climatic history of the planet. Wiseman also added another temporal dimension when he speculated that deep sea cores could become part of forecasting future changes in the climate.

Harold C. Urey added further promise to the study of deep sea cores, as he was present at the Monaco meeting to inform the ocean scientists about recent developments in isotope dating, and particular the utility of stable oxygen isotopes in the measurement of paleotemperatures.²²⁷ Urey had, in the late 1940s, gotten increasingly interested in the prospects of deep sea core drilling. His interests, according to Matthew Shindell, were probably both scientific and strategic: Urey, whose Institute of Nuclear Studies in Chicago

²²⁷ Ibid, 7.
was partly funded by large oil corporations such as Standard Oil and Shell, could pitch the work on deep sea core analysis conducted at the institute as a search for the origins of oil. He wrote to his oil corporation funders that he thought temperature could be an important factor in the development of oil deposits, which would make paleoclimatic analysis of deep sea cores a possibly lucrative venture. However, Urey, left the paleoclimate studies to, particularly, his colleague Cesare Emiliani in the early 1950s and moved towards other scientific issues, mostly relating to the origins of the solar system.

In 1953, the strengthened international coordination and the results of the Monaco meeting lead to the publication of the first ever issue of the journal *Deep-Sea Research*. The journal was going to be solely dedicated to the deep-sea floor. In his foreword of the first issue, Cameron D. Ovey, a Cambridge geographer who was also the Secretary of the Joint Commission on Oceanography, pointed to the collaborations between Pettersson and Huxley as the main reason for the existence of the journal. But he also cited new technologies for sensing and sampling the deep-sea floor and claimed that one technology was of particular importance: the Kullenberg piston corer. With these developments, Ovey claimed, echoing Wiseman, that deep-sea research could “produce important and far-reaching results, which might well revolutionize many of the theories concerning the history and development of our planet”. Ovey had been a supporter of deep-sea core research, and in particular the Albatross expedition, for years. After the expedition had ended he advertised the existence and prospects of the cores in journals beyond the ocean sciences, such as *Journal of Glaciology* and *Weather*. He also received one of the cores from the Albatross expedition for analysis together with colleagues at the British Museum.

The deliberations on the prospects of deep-sea core drilling, and the spatial and temporal scales involved, were not set in stone in the mid-1940s. Instead, in for example Pettersson’s presentation at the inaugural UNESCO meeting, the idea of a “submarine underworld” with a history that could be connected to a civilizational history, appeared as an appealing framework in this particular context. The temporalization of the ocean floor unfolding in the late 1940s and early 1950s also depended on the availability of isotope

231 Ovey, “Foreword,” 2. The first issue was complemented by a symposium held at the University of Liverpool in September of 1953. It was entitled “The Deep-sea Floor and the History of the Earth” and considered “results of research on deep-sea sedimentation from geological, geophysical and biological standpoints with evidence for possible changes in ocean-volume and climate of the past.” Joint Commission on Oceanography of the International Council of Scientific Unions, “Note of Meeting,” *Deep-sea Research* 1:1 (1953): 64.
dating technologies, as Urey’s paleotemperature interest highlights. Hans Pettersson, and
the Swedish work with deep-sea cores, were at one point pushing the direction of deep-sea
core research but were now getting bypassed by the rapidly expanding U.S. oceanography
community.

In the next section, I will follow the transfer of technology, personnel, and knowledge
from Sweden to the United States. By the mid-1950s, several members of the original crew
from the Albatross had relocated to different U.S. institutions and paleoceanography was
becoming institutionalized as a research field of its own. Hans Pettersson, who retired in
1956, found himself in a research landscape that had changed since he first started planning
for the Albatross expedition in the 1930s.

Pettersson goes West: Hans Pettersson’s trip to La Jolla and Chicago 1954

In the winter of 1954, Hans Pettersson and his wife Dagmar, traveled to two leading
research institutions in his field: the Scripps Institution of Oceanography in La Jolla,
California and the Institute for Nuclear Studies at the University of Chicago. Six years had
passed since the Albatross arrived in Gothenburg after its expedition and Pettersson only
had two more years left in his position before his retirement. He was no longer alone in his
enthusiasm for deep-sea cores: the interest in sampling and drilling in the ocean floor had
grown considerably in the last decade. Visiting the United States was a way for Pettersson
to maintain his international network and possibly find new collaborators. He was also
hoping to discuss his new idea about studying meteoric remains, “cosmic spherules” as
Pettersson called them, in the chemical composition of the sediments. Gustaf Arrhenius,
whose work with deep-sea cores in Kagghamra had taken him to a position at Scripps, was
his host. 233

The postwar research landscape that met Pettersson in the U.S. was significantly
different compared to the Oceanographic Institute in Gothenburg, but also in comparison
to how oceanography had been conducted in the U.S. just two decades before. World
War II facilitated the establishment of new connections between the Navy and
oceanographic research. After the war, oceanography further became a beneficiary of the
dramatic influx of funding from military interests into the geosciences. Roger Revelle,
who worked for the Navy during the war, could exercise his influence at the Office of Naval

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233 Hans Pettersson, Diary entry, February 1, 1954, GUL, Hans Pettersson papers, vol. 3. Arrhenius had a
fraught relationship to Pettersson, who disliked his “careerism” and recent relocation to Scripps. “Behind
the affable surface, GA is still doctrinaire [...] This will not be a pleasant collaboration”, Pettersson wrote in
his diary the first day at Scripps.
Research (founded in 1946) in order to combine the interests of oceanographers and the U.S. navy.\(^{234}\) At the time when Pettersson came to La Jolla, Northern Europe was being replaced by the United States as the central scientific geography for ocean science. Pettersson could see this shift occurring on smaller, more personal level as well: during the 1950s, several members from the *Albatross* expedition (Gustaf Arrhenius, Fritz Koczy, and the technician Kurt Fredriksson) relocated to the United States. The temporally oriented deep-sea research Pettersson had championed in Gothenburg found a new home on the other side of the Atlantic.

The new generation of deep-sea core scientists – of which Arrhenius is a good example – differed from Pettersson in their research practice. They were part of larger research institutions, spent more time in the lab than at sea, and could utilize the funding opportunities that the Cold War mobilization in the geosciences had opened up. Pettersson was ambivalent about these developments: on the one hand, he was hoping to use the increased interest in deep-sea sediments to explore new research prospects, but on the other, he was feeling pressured and concerned that his own research agenda was being pushed aside by the oceanographic mobilization in the United States.\(^{235}\) When Pettersson heard of Maurice Ewing’s plan to sample the Mediterranean for deep-sea cores, he wrote in his diary, about Ewing: “He is getting more and more expansive! All the more reason for us to hurry up with our analysis of our Mediterranean cores.”\(^{236}\)

His travel itinerary in 1954 is indicative of the interdisciplinarity of deep-sea core research at the time. After almost two months in La Jolla, Hans and Dagmar Pettersson left for Chicago and the Institute for Nuclear Studies. In Chicago, Pettersson encountered another research institution of its time. The institute in Chicago was a quintessential product of World War II and the influx of funding to nuclear physics. Harold C. Urey and W. F. Libby, who both had leading positions, had previously worked with the Manhattan Project during the war. They had sought to, as the war came to an end, create a research environment which brought together different strands of research about radioisotopes and radioactivity.\(^{237}\) Cesare Emiliani was one young researcher who had found a home in this interdisciplinary environment. He is often hailed as the “founder of paleoceanography” and received his PhD in at the Department of Geology at the University of Chicago in 1950. By combining his training in geology and geochemistry with an interest in deep-sea research

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\(^{235}\) In his diary. Pettersson expressed frustration with being overlooked by Revelle and the repeated difficulties he had to schedule a meeting with him. Hans Pettersson, Diary entries, February 4, February 5, and February 8, 1954, GUL, Hans Pettersson papers, vol. 3.

\(^{236}\) Hans Pettersson, Diary entry, February 14, 1954, GUL, Hans Pettersson papers, vol. 3.

\(^{237}\) Marra, *Hot Carbon*, 54-56.
and long-term changes in the oceans, Emiliani had a lot in common with Hans Pettersson. The two met during Pettersson’s visit to Chicago. Pettersson was happy to make some new connections after not succeeding to convince Urey about the merits of his “cosmic spherule” research.

Pettersson would later help Emiliani establish his own laboratory at the Institute of Marine Science at the University of Miami in 1957. Emiliani’s Miami laboratory would become an early leading research environment for geochemical and paleoceanographic studies of deep-sea sediments, drawing together the empirical work of Pettersson and Kullenberg with the temporally oriented and interdisciplinary approach to isotope dating from Hilde Levi and the Isotope Colloquium. In the 1950’s, Emiliani became one of the scientists who were part of creating a new, historically oriented ocean research program, that built on both isotope analysis, as it was developed in Chicago and Copenhagen as well as drilling with the piston corer. Similar endeavors took place at the Lamont-Doherty Geological Observatory and Scripps Institution of Oceanography, which I cover in greater detail in chapter four. These were not all driven by military or scientific interests. As the prospects of offshore oil drilling began to appear as financially appealing to the petroleum industries, another kind of deep-sea drilling operation would enter the picture in a large scale.

Pettersson, once back in Sweden again, kept working with his cores. The moment for Swedish deep-sea core research, however, had passed. Fritz Koczy, a close friend of Pettersson and a former researcher on the Albatross expedition, was hired to run Emiliani’s lab in Miami. Eric Olausson, and the geochemists Sture Landergren and Göte Östlund,
also joined the Institute of Marine Science in Miami around this time. The presence of the Swedes made it, in Kozcy’s words: “…the strongest geochemical group in marine science in the world and the largest Swedish laboratory in the United States.”

The International Geophysical Year began in 1957, but the Oceanographic Institute in Gothenburg was left outside. Most leading researchers had either retired or moved to the United States, and the research ship *Skagerak* had broken down. The funding structure Pettersson had spearheaded, which relied on local patronage and spectacular expeditions, had fallen out of fashion. Börje Kullenberg had replaced Pettersson as director of the institute in 1956 after Pettersson’s retirement. He tried to defend himself against bad press about the demise of Swedish deep-sea research and stressed the importance of regional studies of the Baltic Sea rather than spectacular circumnavigations.

### Conclusion

In this chapter, my aim has been to show how the rise of isotope dating technologies and deep-sea core drilling enabled a new kind of temporalization of the ocean floor. Hilde Levi and Hans Pettersson are emblematic figures in this transformation, as they facilitated an expansion of the epistemological, temporal and disciplinary boundaries of how deep-sea research – and isotope dating – could be conducted and what kind of knowledge it could produce. Time appeared, in the decade after World War II, as a central category for understanding the world ocean. Historians of the ocean sciences have argued that the ocean came together as a coherent and dynamic scientific object in the twentieth century, and particularly during the postwar era and the IGY 1957–1958. In this chapter my aim has been to show how the temporalization of the ocean floor, through deep-sea core drilling and isotope dating, were part in shaping the postwar understanding of the world ocean.

In order for coherent planetary-scale ocean knowledge to attain a temporal dimension, just drilling for deep-sea cores was not enough. The *Albatross* expedition produced deep-sea cores from around the world, but the cores needed the theoretical framework provided by isotope dating and developments from fields beyond oceanography in order to acquire new meaning. Cesare Emiliani’s establishment of his Miami laboratory, which brought together isotope dating and oceanography in an institutionalized way in

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1957, is an example of how these previously scattered practices became unified in an established research program.

Following deep-sea core drilling from Gothenburg in the 1940s to Scripps and Lamont-Doherty in the 1950s shows how the scientific production of time is intertwined with institutional structures and ideologies. Börje Kullenberg saw the piston corer as a technology which could operate within the framework of Swedish quaternary geology, while Maurice Ewing, using a similar corer, imagined it as a part of a planetary-scale research program. Hans Pettersson enrolled the histories he made visible in the deep-sea cores with the universalizing ambitions of UNESCO, which came to affect the framework in which early deep-sea core research was conducted. How ocean timescales can be produced, how and whether they should be synchronized, and for whom they are relevant, are historically embedded processes, developing in tandem with broader changes in the ocean sciences. Turning the ocean floor into timescales was not a matter of discovery, but of interdisciplinary collaboration, historical imagination and technological and political developments.

A focus on time opens up new epistemic geographies of the ocean. Isotope dating was developed apart from the ocean sciences, but still proved instrumental for the field of paleoceanography to emerge. Work conducted in laboratories in Chicago and Copenhagen are therefore, I suggest in the chapter, also part of the history of oceanography. The circulation of knowledge between actors such as Hilde Levi, W. F. Libby, Harold C. Urey and Hans Pettersson points towards a larger turn towards time and geochronology in the decade after World War II. It was rendered possible by scientific, geopolitical, and technological changes in Scandinavia and the United States.

The Isotope Colloquium in Copenhagen shows how new dating technologies were not, at the time, clearly divided between different disciplines, and opened up for a broad scientific engagement with temporal issues on different scales. The ocean floor was one scientific object that was drawn into this turn towards making new material times. The transfer of knowledge, personnel and technology from Scandinavia to the United States appear as crucial to this development and points to how the small-scale operations in Sweden were scaled up in the booming Cold War oceanography context in the 1950s.

The rise of isotope dating had ramifications for scientific practices beyond the ocean sciences as well. In the next chapter, I will turn to ice, another central element of paleoclimatology. The chapter will focus particularly on Willi Dansgaard. He was a regular attendee of the Isotope Colloquium and he had his office across the street from Hilde Levi. He chose to utilize the potential of isotope dating in a completely different place: the Greenland ice sheet.
Old water

WILLI DANSGAARD AND ISOTOPE GLACIOLOGY, 1952 –1966

Towards the end of his career, Willi Dansgaard claimed to have had only one good scientific idea: that there is a connection between isotope composition and temperature in precipitation. Water that falls from the sky, in other words, also contains information about atmospheric temperature from time the rain began to move towards the ground underneath. The older the water is, the further back in time you can go. This principle has become a key feature of ice core drilling, and paleoclimatology more generally, and enabled a wide set of scientific practices, projects and temporalizations in the second half of the twentieth century. Dansgaard himself came to be widely regarded as the father of ice core science. Dansgaard’s own recollection of how he came up with the idea resembles other, more famous stories of groundbreaking discoveries, combining mundane everyday objects with a flash of scientific genius. A bathtub overflows when the philosopher lies down in it; an apple falls down on the head of the famous physicist.

For Dansgaard, his own claim to a place in this history came during two unusually rainy days in June, 1952. His workplace at the Biophysical Laboratory at the University of Copenhagen had recently acquired a mass spectrometer, but the biological researchers at the laboratory had not yet found ways to make full use of the new instrument. Dansgaard, with a background in geophysics, had an excellent instrument he could use for isotope analysis, but no research project to connect it to. He began sampling rainwater in his garden, which he analyzed with the mass spectrometer. Using old beer bottles and funnels,

he gathered water throughout the duration of the several day long rain that swept over Copenhagen. Dansgaard noticed that the rain that fell early on during the several-day rainfall – and therefore came from a higher altitude in the warm front and thus also from colder temperatures – had a lower portion of the heavy $^{18}$O isotope than the rain that fell later. He published his findings in the geophysical journal *Tellus* in 1953 and suggested a correlation between temperature and oxygen isotopes in precipitation.\textsuperscript{245}

In reality, these narratives are of course never as simple as their protagonists let on. Dansgaard were, as we saw in the previous chapter, situated in a burgeoning research environment concerned with widely applying isotope dating technologies in a variety of fields. While Dansgaard’s idea to sample rainwater was inventive, it also mirrored the research interests and available equipment of his surroundings. The rise of what Dansgaard would later call “isotope glaciology” – the combination of isotope dating and glaciological research practice – depended on a complex interplay of factors, rather than being the consequence of a stroke of genius in a Copenhagen garden.\textsuperscript{246}

By following Willi Dansgaard’s early career, this chapter uncovers the disciplinary, technological and geographical context around the emergence of isotope glaciology and ice core drilling in Greenland.\textsuperscript{247} I argue that this process was not just about drilling deep into the ice and then applying isotope dating technologies on a clearly defined research object. Instead, I follow Dansgaard’s interdisciplinary collaborations and how the practices and temporal sensibilities of isotope glaciology grew in a far less linear way than Dansgaard and other chroniclers of ice core drilling would later contend.\textsuperscript{248}

In isotope glaciology the ice was not the primary object of inquiry: it was the old water or “ancient atmosphere” stored inside it. Ice could “stand in” for temporally deep and geographically vast phenomena rather than being a, as had typically been the case in the glaciological tradition, scientific object in itself.\textsuperscript{249} The turn in thinking of ice – and the Greenland ice sheet in particular – from a distinct, localized environment to a mass of old water, which could be extracted, quantified and connected to planetary dynamics, marked a larger shift in the temporality and epistemology of studying ice.


\textsuperscript{247} I self-consciously use the term “isotope glaciology” – which Dansgaard first began using in the 1970s – somewhat anachronistically. I see it as a concept that brings together isotope dating and glaciological research, which I argue Dansgaard and other scientists began exploring already in the 1950s even though they had not yet coined the term.

\textsuperscript{248} Willi Dansgaard, *Frozen Annals: Greenland Ice Cap Research* (Copenhagen: Niels Bohr Institute, 2005).

Dansgaard’s close collaboration with biologists and physiologists in the 1950s was, this chapter explores, formative for a new epistemology of ice, which drew on both geophysical and physiological knowledge. It involved scaling between different kinds of bodies in Arctic environments – ranging from humans, whales, fish, and plants to glaciers and icebergs – in order to track their interactions with climatic and atmospheric conditions.²⁵⁰

Previous scholarship on Dansgaard and ice core drilling activities in Greenland have shown how the vertical approach to the ice sheet transformed both glaciological practice and the epistemologies of ice itself. The Camp Century core – the 1966 ice core from Greenland that enabled a 100 000 year timescale of climate fluctuations – was a landmark achievement in a promising new research field. It was also a scientific object that was, as Janet Martin-Nielsen argues, part of a re-conceptualization of Greenland as a site of environmental rather than military interest.²⁵¹ Similarly, Dania Achermann has shown how the discovery of the “third dimension” in glaciology brought ice into the world of climate modeling and contributed to a temporal expansion of climate history.²⁵² Even though the drillings at Camp Century, and their relation to broader scientific geographies of climate science, have been well covered by recent scholarship, the early career of Willi Dansgaard and the interdisciplinary frameworks he was a part of, is less well known.²⁵³

The rise of ice core drilling was not only about verticality. In his collaboration with scientists from the life sciences,Dansgaard developed a new kind epistemology of ice which both built upon and broke with previous glaciological traditions. In his search for “old water” and, later on in the 1950s, “ancient atmosphere”, stored in the Greenland ice sheet, Dansgaard’s isotope glaciology can be understood as part of a broader history of temporalization of ice as well as the planetary-scale environment.²⁵⁴

²⁵⁰ Deborah Coen defines “scaling” as "the process of mediating between different systems of measurement, formal and informal, designed to apply to different slices of the phenomenal world, in order to arrive at a common standard of proportionality. In the natural and social sciences, scaling (upscaling, downscaling) refers to the process of adapting models to apply at larger or smaller dimensions of space and time.” Deborah Coen, Climate in Motion: Science, Empire, and the Problem of Scale (Chicago: The University of Chicago Press, 2018), 16. See also: Gabrielle Hecht, “Interscalar Vehicles for an African Anthropocene: On Waste, Temporality, and Violence,” Cultural Anthropology 33.1 (2018): 109-141.
Dansgaard’s equipment when collecting the first samples of rainwater in his garden in Copenhagen. Lolck, *Klima, Kold Krig og Iskerner*, 32.

Figure 3.2. Diagram from the *Tellus* article, which shows an early temporalization of temperature in precipitation. Dansgaard, “The Abundance of O¹⁸ in Atmospheric Water and Water Vapour”.
The early career of Willi Dansgaard

Born in Copenhagen 1922, Willi Dansgaard would remain in his hometown until his death in 2011. Apart from expeditions and visiting positions, he spent his entire career at different departments and institutes connected to the University of Copenhagen. In the summer of 1947, he received his degree (embedseksamen) in physics, with a specialization in experimental physics and biophysics. During his studies he had become acquainted with Helge Petersen, the director of the Danish Meteorological Institute, who, as soon as Dansgaard had received his degree, offered him a position at the Geomagnetic Observatory in Godhavn (Qeqertarsuaq) on the Disko Island, northwest of the coast of Greenland.

Dansgaard entered the world of Danish meteorology at a pivotal moment: in comparison to its Scandinavian neighbors, meteorological research in Denmark was less developed and less theoretically oriented. But as Denmark regained more control over Greenland after World War II, hopes about a more advanced research program were rising. The reasons were also geopolitical: Denmark would continue the weather observations and share the data with the United States, which was both a scientific venture and an act of diplomacy and claim of legitimacy in a contested phase of Denmark’s control over Greenland. The lack of knowledge and competent staff posed a problem for Danish politicians and gave the Americans a reason to not let go of Greenland. As a part of the Danish effort to prove their credentials in meteorology, a professorship in theoretical meteorology at the University of Copenhagen was announced in 1947 and promising young researchers – Willi Dansgaard being one of them – were encouraged to engage with Arctic matters. Additionally, there was Scandinavian scientific interest in an increased Danish scientific presence in Greenland. The Swedish glaciologist Hans Ahlmann was instrumental to the establishment of the professorship in theoretical meteorology, advocating for the scientific benefits of Scandinavian collaboration in the Arctic.

Returning back home to Copenhagen after a year in Greenland, Dansgaard had gained familiarity with both Arctic environments and working with applying his geophysical knowledge on meteorological and climatological issues. He continued working at the Meteorological Institute in Copenhagen after his return, making short-term weather prognosis for Denmark and the Faroe Islands. The manual nature of the work and the lack

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255 Roughly the equivalent of a MSc today. This was also how Dansgaard himself referred to his degree in his own CV.
of research oriented colleagues at the Meteorological Institute proved unsatisfying in the long run. Dansgaard chose to return to the Biophysical Laboratory in 1949.\textsuperscript{258} Even though he had to give up on a more secure and well paid position at the Meteorological Institute, Dansgaard recalled the move to the Biophysical Laboratory as productive: he could work with other, more theoretically oriented colleagues, and get access to state of the art equipment, not least the mass spectrometer which Dansgaard was tasked with installing.\textsuperscript{259}

The environment at the Biophysical Laboratory proved to be a fertile ground for Dansgaard’s interdisciplinary interests. Housed at the Rockefeller Complex — funded, as the name indicates, by the Rockefeller Foundation – the Biophysical Laboratory shared its facilities with other experimental research groups in adjacent fields. The Zoophysiologival Laboratory, which was spearheaded by August Krogh and served as the workplace for Hilde Levi, was next door and Niels Bohr’s Institute of Theoretical Physics in the same neighborhood.\textsuperscript{260} A multidisciplinary engagement with the prospects of isotope dating and isotope tracers was a mutual interest across the different departments of the Rockefeller Complex, which had become a hub for interdisciplinary research in the natural sciences.\textsuperscript{261} For Dansgaard, this manifested not only in his participation in Levi’s Isotope Colloquium, but also in the possibility to access advanced technology and equipment and create an international network.

The scientific infrastructure was, in other words, already in place when the heavy rainfall reached Copenhagen in June 1952. After the sampling of rainwater and subsequent isotope analysis, a promising research program was opening up: in combining the work on oxygen isotopes, meteorological studies of precipitation, and a surging interest about planetary dynamics across multiple earth sciences, Dansgaard had found a niche he could continue to explore.\textsuperscript{262} The same summer, 1952, the \textit{Galathea} expedition returned home to Copenhagen after two years at sea. Anton Bruun, the leader of the expedition, offered Dansgaard water samples from the Philippine Trench in the Pacific Ocean, some of them originating from more than 10 000 meters below the surface. Using the same methods of determining isotopic composition as on the rainwater he had gathered in his garden, Dansgaard would end up publishing the results in \textit{Deep-Sea Research} in 1959.\textsuperscript{263}

\textsuperscript{258} Willi Dansgaard, “Huskerier,” part 2, 120-121, Willi Dansgaard Papers, Copenhagen University Library (hereafter CUL), Box 23.  
\textsuperscript{259} Dansgaard, \textit{Frozen Annals}.  
\textsuperscript{260} Chapter two covers this research environment in greater detail.  
\textsuperscript{261} Torkild Andersen, \textit{Datering av fortiden: Om det første danske kulstof-14 laboratorium} (Aarhus: Aarhus Universitetsforlag, 2007).  
\textsuperscript{262} Sarah Dry, \textit{Waters of the World} (Chicago: The University of Chicago Press, 2019).  
The results themselves, however, were not particularly exciting for Dansgaard: given the relative rapidity of ocean currents, the water at the bottom of the Philippine Trench circulated constantly and the isotope dating showed no significant difference from the shallow water to the depths of the trench. Drawing on the work of Fritz Koczy of the Albatross expedition as well as the deep-sea research conducted at the Institute for Nuclear Studies in Chicago, Dansgaard concluded that the “...fact that no significant fractionation has been found indicates that the time for a complete exchange of the water masses in the trench is very short, relative to that needed for the sedimentation process to reach equilibrium.” The ocean depths, as Henry Stommel and other oceanographers at the time were confirming, were in constant motion.

Dansgaard did not just venture downwards to the depths in his search for water to sample. Through his colleague at the Biophysical Laboratory, Bent Buchmann, Dansgaard came into contact with pilots at the Danish Airforce. He had a hypothesis that by studying the clouds directly, rather than as rainwater on the ground, he would be able to collect better, less contaminated, samples. Together with his wife Inge, Dansgaard joined the pilot training program during their flights and collected small water droplets from the clouds they flew through. By drilling a tiny whole through the cabin wall and attaching the hole via a tube to a container of dry ice, he was able to immediately freeze the droplets and bring them back down to the ground for further analysis. In this case, quantity posed a problem: it was hard to access enough droplets to produce any kind of quantitative analysis. And then there was the problem of time. The droplets, despite their pristine character, were recent additions to the atmosphere and would not suffice for Dansgaard who wanted to try his hypothesis that isotope dating could be expanded to much longer timescales.

The lack of sufficiently old water was one conundrum to solve for Dansgaard, but other issues were surfacing as well. With his geophysical background, Dansgaard was interested in planetary scale dynamics, but his samples came from a very limited set of geographies. Anton Bruun introduced Dansgaard to his friend Prince Axel of Denmark, who also served as the director and chairman of the Det Østasiatiske Kompagni (ØK) (The Danish East Asiatic Company). Between 1952 and 1954, the ØK gathered water samples around the world and brought them back to Dansgaard in Copenhagen. When Dansgaard compared the samples gathered by the ØK with his own samples, primarily from Scandinavia and Greenland, he noticed a stronger correlation between the abundance of

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265 Dansgaard, “The content of heavy oxygen isotope,” 349.
the $^{18}$O isotope and temperature in the samples from colder regions, which suggested observable seasonal patterns in the precipitation found in Northern and Arctic regions. Relying on recent research in atmospheric chemistry and geophysics, Dansgaard also assumed the water circulated in the atmosphere, and that the precipitation in the samples stemmed from subtropical zones far away from the Arctic.\footnote{Alfred O. Nier, “A Redetermination of the Relative Abundance of the Isotopes of Carbon, Nitrogen, Oxygen, Argon, and Potassium,” Physical Review 77:6 (1950): 789-793; Fredrik Nebeker, Calculating the Weather: Meteorology in the 20th century (San Diego: Academic Press, 1995).}

In 1954, Dansgaard published a summary of his research on fresh water samples in the journal Geochimica et Cosmochimica Acta. Evaporation posed a problem in most parts of the world, Dansgaard argued, as it blurred the timescales of precipitation and the age of the water. But the Arctic could provide other opportunities, Dansgaard suggested towards the end of the article:

> In certain areas on the Greenland Ice Cap is a distinct layer formation caused by melting in the summer season. On the supposition that the character of the circulatory processes, in all essentials, have not varied over a long period of time, the above, in the opinion of this author, offers the possibility by measurements of the $a$, in these layers of ice to determine climatic changes over a period of time of several hundred years of the past.\footnote{Willi Dansgaard, “The $^{18}$O-abundance in fresh water,” Geochimica et Cosmochimica Acta 6 (1954): 259. The variable $a$ refers to $O^{18}$ abundance after distillation of a sample.}

Even though Dansgaard remained on a speculative level in this paragraph and in his article, he appeared confident that studies of the kind he was suggesting were not far away. “An investigation will be undertaken as soon as an opportunity offers” Dansgaard concluded.\footnote{Dansgaard, “The $O^{18}$-abundance in fresh water,” 260.}

Before Dansgaard had a chance to fulfil his research plans in Greenland, other options were lining up ahead. As a part of the Marshall Plan to restore European economies after World War II, the OECD granted individual researchers travel stipends to the United States. For Dansgaard, who had at times felt that the environment at the Biophysical Laboratory in Copenhagen did not focus enough on his research interests, the prospect of visiting the Institute for Nuclear Studies at the University of Chicago and other leading U.S institutions was tempting. In 1954,Dansgaard acquired 2000 Danish krona in funding for an eight month stay in the United States. The aim was mainly to visit the laboratory of Harold C. Urey and his research group in Chicago, but also to make visits to several
laboratories around the United States, in order to take part in the growing field of isotope dating.272

The eight-month long stay would put Dansgaard in contact with leading figures in the development of isotope dating methods, including Harold C. Urey, Alfred O. Nier and Cesare Emiliani. Dansgaard arrived just a few months after Hans Pettersson had visited the same Chicago institution. Even though the interactions with Urey was a disappointment – Urey was absent for most of Dansgaard’s stay and, in Dansgaard’s opinion, neglected his Scandinavian visitor in favor of his own employee Harmon Craig – Dansgaard became acquainted with new technologies and a growing, yet quite undefined, community of scientists working with paleotemperatures and the establishment of a temperature scale.273

The breadth of the scientific communities Dansgaard engaged with is visible in his travel report: he met with the botanist Alan H. Brown at the University of Minnesota, who was using isotope tracing to study photosynthesis, Sam Epstein, who was working on carbon dating of wood at the California Institute of Technology,274 and he met with representatives of the Consolidated Engineering Corporation in Pasadena, CA, who were developing mass spectrometers for commercial use.275

During his stay in the United States, Dansgaard did not, however, meet with any glaciologists.276 Despite his stated interest in ice and the Greenland ice sheet, Dansgaard was much more interested in visiting laboratories and theoretically oriented research environments, than engaging with glaciologists, or any other kind of field-based researchers. In this sense, Dansgaard’s visit in the United States in indicative of a broader tendency in the formation of isotope dating methods, and subsequently also the research practices of paleoclimatology. The materialities and geographies that the scientists engage with – be it wood, ice, water, deep-sea sediments – are secondary to the methodologies they apply and the possibility to produce aggregated datasets that are detached from local variabilities and circumstances.277 As Dansgaard returned home to Copenhagen he had

272 Willi Dansgaard, “Rapport over studierejse foretaget af Willi Dansgaard til nogle amerikanske laboratorier” (1955), Willi Dansgaard Papers, CUL, box 33.
275 Dansgaard, “Rapport over studierejse”.
276 It is of course possible that he did at some point, but neither his travel report nor his unpublished autobiography makes any mention of engaging with glaciological researchers. This is not, should be noted, very surprising, given his geophysical background, but nevertheless speaks to his disciplinary distance from glaciology.
277 Matthias Heymann and Dania Achermann, “From Climatology to Climate Science in the Twentieth Century,” in Sam White, Christian Pfister, and Franz Mauelshagen (eds.), The Palgrave Handbook of Climate...
made new contacts in the theoretical domain, but getting access to the “old water” in the north remained a challenge.278

Between physiology and geophysics: The Bubble Expedition to Greenland 1958

In the first years after World War II, glaciology – along with other earth sciences – saw a rapid growth in its disciplinary scope and institutional structures. The International Glaciological Society (established in 1936) founded the periodical Journal of Glaciology in 1947 and new glaciological societies and chapters were founded in, among others, Canada, United States, Norway, France and Switzerland.279 International collaboration in glaciology grew after the war as well and the International Commission for Ice and Snow, an independent scientific body existing under the umbrella of the International Association of Scientific Hydrology, gathered an international community of glaciologists in the early postwar years.280 The Swedish glaciologist Hans Ahlmann served as the commission’s first chairman.281

In 1954, the Commission met in Rome and decided to pursue their first scientific expedition to Greenland. The expedition, which went under its French name Expédition Glaciologique Internationale au Groënland (EGIG), had as its goal to continue the legacy of interwar expeditions to Greenland – most prominently those of Alfred Wegener – but also to approach the ice sheet from a more pluralistic disciplinary framework. By combining approaches and methods from geophysics, geochemistry, geodesy, meteorology, seismology and glaciology, EGIG would transcend previous ways of producing scientific knowledge about the Greenland ice sheet and enroll a wider set of actors than had previously been the norm for science in Greenland.282 The interdisciplinary approach is indicative of broader movements in early Cold War earth sciences. EGIG was set to take place 1957–1960, thereby coinciding with the International Geophysical Year (IGY)
1957–1958, which sought to realize a similar international and interdisciplinary approach to geophysical matters on a planetary scale.\footnote{There are many works written on the history of the IGY. For an overview, see: Christ Collis and Klaus Dodds, “Assault on the unknown: The historical and political geographies of the International Geophysical Year,” \textit{Journal of Historical Geography} 34:4 (2008): 555–567; Roger D. Launius, James Fleming, and David H. DeVorkin (eds.), \textit{Globalizing Polar Science: Reconsidering the International Polar and Geophysical Years} (New York: Palgrave Macmillan, 2010).}

Just as the IGY, the EGIG was a scientific operation as well as a contentious diplomatic endeavour. Janet Martin-Nielsen points out that the key diplomatic issue for the EGIG was the tense relationship between the expedition’s leaders, particularly the French glaciologist Paul-Emile Victor, and the Danish political, diplomatic and scientific establishment. The status of Greenland as Danish territory was a fraught issue after WWII and science was considered a diplomatic asset by Danish officials, which could be utilized to claim territorial sovereignty over Greenland.\footnote{These acts of science diplomacy by Denmark were one reason for Willi Dansgaard’s placement on Greenland as a student of meteorology and geophysics in 1947.} After much negotiation and some sacrifices by the EGIG leadership – all results of the expedition, for example, had to be published in the Danish periodical \textit{Meddelelser om Grønland} – the expedition could be conducted as planned.\footnote{Nielsen, \textit{Eismitte in the Scientific Imagination}, 96.} Despite the Danish claims to the publications stemming from the expedition, the actual science in the field did not involve any Danish scientists. Out of 40 participants on the ice sheet in 1959, one was Danish, and he was not a scientist, but an observer assigned by the Danish government.\footnote{Ibid., 89.} Willi Dansgaard would later describe the role of Denmark in the EGIG as unimpressive and disappointing, noting that the biggest contribution by Denmark was forcing the scientists to publish in an unknown journal “at their own expense!”\footnote{Willi Dansgaard, “Huskerier, part 2,” 20, Willi Dansgaard Papers, CUL, box 23.}

As the EGIG was being planned in the mid-1950s, Dansgaard had also begun to direct his attention towards ice. Without any clear connections to the domain of glaciology, it was not self-evident how Dansgaard would be able to join the increasing international interest in geophysical research in the polar north. However, his placement at the Biophysical Laboratory would once again prove to be a viable asset. In 1957, the Swedish-Norwegian physiologist Per Scholander was invited by Poul Brandt Rehberg to give a lecture in Copenhagen. Rehberg was August Krogh’s successor as head of the laboratory. Scholander had known Krogh since the mid-1930s and had published extensively on arctic botany and zoology, and in particular developed an interest in oxygen and respiratory metabolism in arctic plants and animals. Inspired by the work of Krogh, Scholander approached cardiorespiratory responses to diving as operating on a systems level. He outlined the

In 1939, just before the beginning of World War II, Scholander received, through Krogh’s help, a Rockefeller fellowship to study Arctic physiology with Laurence Irving at Swarthmore College. Irving was, however, recruited to the Air Force as the United States entered the war and Scholander decided to join him. They did their military service in Alaska, drawing on their physiological expertise to test survival gear, such as sleeping bags and field stoves, and military equipment in harsh weather conditions.\footnote{Knut Schmidt-Nielsen, Per Scholander 1905–1980. A Biographical Memoir (Washington, D.C.: National Academy of Sciences, 1987).} During this time, Scholander was invited to the Harvard Fatigue Lab, working on carbon dioxide transportation and blood-gas analysis alongside biochemist and physiologist J. W. Roughton.\footnote{Per Scholander, Vladimir Walters, Raymond Hock, and Laurence Irving, “Body Insulation of Some Arctic and Tropical Mammals and Birds,” Biological Bulletin 99:2 (1950): 225-236; Per Scholander, H. Niemeyer, and C. Lloyd Claff, “Simple Calibrator for Warburg Respirometers,” Science 122:2911 (1950): 437-438.} As the war ended, Scholander remained interested in heat exchange between bodies and their surroundings. He worked both empirically with Arctic animals and developed theoretical frameworks and laboratory equipment in order to make more general studies of heat exchange and rate of exchange between oxygen and carbon dioxide.\footnote{Per Scholander, John W. Kanwisher, and D. C. Nutt, “Gases in Icebergs,” Science 123:3186 (1956), 104-105.}

Scholander’s interest in tracing and monitoring minute samples of gas led him to develop methods that proved to be useful to apply to scientific objects beyond physiology. During his time in the Arctic, he noticed that glacier ice seemed to contain small bubbles of air. Dropping a piece of glacier ice in a drink in order to cool it would result in a fizzing sound, but when Scholander tried to insert gas into ice he found it impossible to penetrate. He drew the conclusion that the gases inside the ice must be remnants from the time the ice first froze.\footnote{Per Scholander, John W. Kanwisher, and D. C. Nutt, “Gases in Icebergs,” Science 123:3186 (1956), 104-105.} In the mid 1950s, Scholander began publishing scientific work on glaciers and icebergs, despite having no training in glaciology, and noticed how the study of gases
inside glacier ice could open up new research questions. As Scholander, together with Edvard Hemmingsen and L. K. Coachman, put it in a 1956 article in *Tellus*: Many questions arise with regard to the gas pressure and amount of gas in the bubbles of enclosures, such as: to what extent is the atmosphere trapped in the ice preserved since formation of the ice? To what extent do the bubbles reflect the pressures in the ice? To what extent is the ancient ice preserved in its original composition? Only by systematic studies can one hope to answer these questions.²⁹³

Scholander thought isotope dating using the carbon-14 method could be a promising way to approach these questions.²⁹⁴ When Scholander came to Copenhagen in 1957, it is understandable why Paul Brandt Rehberg thought it was a good idea to introduce Scholander to Willi Dansgaard. His work on isotope dating and water samples had many similarities with Scholander’s, and Dansgaard had his office in the basement right underneath the Biophysical Laboratory where Scholander gave his lecture.

Scholander was planning an expedition to Store Breen in Jotunheimen in Western Norway, with the ambition to utilize carbon-14 dating methods on samples of ice. Dansgaard, with his interest in isotope dating, was invited to join.²⁹⁵ The expedition took place in the fall of 1957 and served primarily as a way to explore the prospects of dating ice using the carbon-14 method in order to motivate a larger expedition in the years that followed. For Dansgaard, the expedition also opened up the opportunity to develop his stable isotope methodology, which relied on the oxygen isotopes $^{18}$O and $^{16}$O rather than radioisotopes in carbon. He quickly scrambled for funding to join Scholander and his team in Norway. Dansgaard was able to acquire 1000 Danish krona from Niels Bohr, who was one of the founders of the biophysical research environment in Copenhagen.²⁹⁶ Dansgaard and Scholander saw an opportunity to use carbon-14 dating on the ice to establish its age and then use stable isotope dating on the air bubbles inside the ice in order to access information about the atmospheric conditions from the time the ice froze.

The expedition to Store Breen was in retrospect framed as both a success and a failure: the scientists were not able to get a hold of old enough ice in order to validate their theories, but the few samples they were able to recover seemed to hint at a correlation between

²⁹⁴ Scholander, Kanwisher, and Nutt, “Gases in Icebergs,” 104.
isotope composition and the age of the ice, thereby also motivating further research in the area. In a letter to Bohr, Dansgaard admitted that one flaw of the expedition was the absence of any glaciological expert in the crew and the lack of knowledge about both the composition of snow and ice as well as the geographical area made it difficult to accurately assess the age of the ice. Olav Liestøll, a glaciologist at the Norwegian Polar Institute in Oslo, was consulted and eventually also joined the expedition in order to remedy for the lack of glaciological and local knowledge. But even with Liestøll present, creating a geochronological survey of the glacier in Stora Breen proved difficult. The researchers had estimated that 5, 5 meters of snow would account for 5 years of accumulated snowfall, but in reality, the variations in annual snowfall were too large to facilitate for a general rule. The lack of knowledge of local conditions played in as well. “The thickness of the snow varied greatly from place to place”, Dansgaard noted.

Another factor that made it difficult to test Dansgaard’s hypothesis about a correlation between the isotopic composition of past precipitation and the temperature from the time in which it fell was the lack of technology. At Stora Breen, the researchers first dug through the snow and as they reached the solid glacier ice underneath they began using an axe instead. Such “primitive tools”, in Dansgaard’s words, would not suffice. In temporal terms, they were only able to go a few years back in time, far from the depth needed to draw any kind of conclusions regarding climatic variability and the exchange between oceans and atmosphere.

Dansgaard and Scholander, after consulting Liestøll, agreed that the results from Stora Breen were promising, but insufficient. The glaciers in Norway were not ideal, as they did not contain enough solid and old ice, which could clearly separate recent meltwater from ancient ice, and the technological means were not enough to test the theories in practice. But, as Dansgaard concluded in his letter, new opportunities were lining up in the year to come, and a more advanced expedition to Greenland was underway:

Such an expedition is planned for the summer of 1958, with prof. Scholander of Oslo as the leader. The main object of inquiry is C\textsuperscript{14} dating of the oldest part of the Greenland ice sheet. Thereby the opportunity arises to conduct an isotope investigation in order to acquire information about climatic conditions several thousand years back in time.

301 Willi Dansgaard, Letter to Niels Bohr, September 12, 1957. Willi Dansgaard Papers, CUL, box 29. All translations from Danish are my own.
The 1958 expedition went under the name of the Bubble Expedition (Boble expedition), referring to the air bubbles inside the ice that the expedition was searching for. The study object was not ice, but rather the air that the ice contained. David C. Nutt, a longtime collaborator of Scholander from the United States and a trained botanist, lead the expedition along with Scholander. Dansgaard joined as a researcher on the ship. The main objectives of the expedition was to “determine the composition and age of ancient atmosphere trapped in glacier ice” and “seek information regarding age and origin of icebergs”. It was an expedition historically oriented in its focus, but not in a vertical, geochronological sense, but through the sampling and melting of small pieces of ice from a variety of Greenland icebergs. Ice could be, potentially, a source of historical data without a vertical and stratigraphic outlook.

Scholander and Nutt drew on their previous work on the circulation of heat and gases inside a closed system when they planned the expedition, which they had previously studied inside icebergs as well as the bodies of arctic terrestrial and marine animals, and even humans. The expedition was not planned with current glaciological research problems placed at the center. Instead it was construed by Scholander and Nutt as a multifaceted operation that could gather data and approach a variety of Arctic phenomena of interest to multiple fields. Even though the “ancient atmosphere” inside the icebergs was at the heart of the expedition, they saw the opportunity to study, among other things, lichen, dolphins and ocean currents at the same time as the sampled the icebergs. Their relationship to glaciology was, similarly to Dansgaard, not a very close one: in the preparations for the expedition, Scholander and Nutt had to convince leading figures in glaciology to give them their blessing in order to access funding. Scholander met with both Paul-Emil Victor and Hans Ahlmann in order to make them endorse the project, which they eventually did, but

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306 Per Scholander, Letter to David C Nutt, January 27, 1958, David C. Nutt Papers, Rauner Special Collections Library Repository, Dartmouth Library (hereafter RSCLR. I would like to especially thank Scout Noffke who kindly made these materials available remotely in the midst of the pandemic), Box 5, Folder 71; David C Nutt, Letter to Per Scholander, February 10, 1958, RSCLR, Box 5, Folder 71. One example of the multifaceted approach is Scholander’s ad hoc study of how dolphins could ride the waves behind the ship, which was later published in Science: Per Scholander, “Wave-riding dolphins: How do they do it?” Science 129:3356 (1959): 1085–1087.
with some hesitations given the expedition’s lack of glaciological expertise. Particularly Ahlmann’s endorsement was deemed necessary to access funding from the Arctic Institute of North America.

It was not only in organization and scope that Scholander, Nutt, and Dansgaard differed from their contemporary glaciological colleagues. During the three month long expedition on the research ship Rundøy, the crew and the researchers (19 in total, 9 of them researchers) travelled along the coast of Northern Norway and Greenland. The research practice had more in common with oceanographic expeditions than previous glaciological ones, as the ship also served as the laboratory in which the samples were melted, analyzed and stored.

Scholander, who had held positions at Woods Hole Oceanographic Institution and would go on to work at Scripps Institution of Oceanography for the rest of his career after the expedition, was well versed in oceanographic research methods. He particularly cited Maurice Ewing’s deep-sea core work on the R/S Vema as an inspiration for his own work, noting how Ewing could “do practically anything on his ship, the Vema.”

By studying icebergs, rather than solid inland glaciers, the researchers rarely had to get off the ship and instead gathered their samples directly from calving icebergs floating outside Greenland. The M/S Rundøy was already equipped with a heavy duty commercial steam generator for the processing of fish and shark, which the researchers found use for in using steam to cut off ice from icebergs and for melting the samples onboard. The idea was to “drop the land operation entirely and work on icebergs from a gasoline drum float platform tied onto a vertical iceberg wall, mining the ice right down […] onto the platform.” The hunt for the gases trapped inside the icebergs resembled Dansgaard’s previous attempts to access “old water” from the walls of the Danish Airforce airplanes or the water samples brought to him by the Danish East Asiatic Company. The icebergs were seen as containers of data, which could be shaved off their walls, and extracted through an elaborate laboratory process on board the ship.

David C. Nutt, Letter to Per Scholander, May 12, 1958, RSCLR, box 5, folder 71.

Nutt considered Ahlmann’s letter of recommendation the “best ammunition we have”. David C. Nutt, Letter to Per Scholander, December 27, 1957, RSCLR, box 5, folder 71.


Per Scholander, Letter to David C. Nutt, January 6, 1958, RSCLR, box 5, folder 71.
Other members of the expedition had a comparable indirect interest in the icebergs. Edvard Hemmingsen, a biologist who worked with Scholander and was part of the expedition to Stora Breen, worked with the permeability of gases through ice. But in contrast to Dansgaard’s geophysical framework, Hemmingsen had previously studied how northern plants and animals could survive the winter frozen solid and maintain some respiration despite being almost completely frozen to ice. Just as Hemmingsen and Scholander had previously studied how oxygen could be isolated in the bodies of arctic plants and animals, they began further elaborating on the isolating qualities of ice and how it appeared to be more impermeable for oxygen than previously thought. “This opened up the interesting possibility that gas bubbles enclosed in ice might under ideal conditions remain unchanged for very long times, perhaps millennia”, Hemmingsen put it in 1959, and noted how isotope dating had further opened up the possibility to accurately assess the age of the ice, and therefore also the “ancient atmosphere” inside.

For both Hemmingsen and Dansgaard, the preservatory qualities of the ice were central, but by expanding the research from the bodies of plants and animals to the air trapped inside the icebergs meant a both spatial and temporal jump in scales. The gaseous exchanges happening between icebergs and their surrounding atmosphere was similar to the exchanges between plant and animal bodies and their surroundings, but unfolding on much longer timescales. At the same time, the search for “ancient atmosphere” inside the icebergs also built on imaginaries of the ability of ice to freeze time and preserve life. As Joanna Radin and Emma Kowal point out, the early postwar years were a formative time for cryobiology and the notion of “latent life” embedded in very cold

313 Hemmingsen, “Permeation of Gases through Ice,” 3.
314 They thereby built on a longer history of physiologists applying the logics of gaseous exchanges between bodies and their surroundings on a larger, geophysical scale. See, August Krogh, “The abnormal CO2 percentage in the air in Greenland and the general relations between atmospheric and oceanic carbonic acid,” Meddelelser om Grønland 26 (1904): 406-434.
315 Building on the philosopher Peter Sloterdijk, Martin Skrydstrup argues that ice core science can be seen as a process of making “scalar connections between air bubbles and hemispheric fluctuations” and therefore become an empirical mirror image of Sloterdijk’s philosophical work on spheres as a way to write a theory of spatiality. Dansgaard’s collaboration with physiologists and biologists seems to indicate further “scalar connections”, not only between air bubbles in the ice and the atmosphere, but also between oxygen in the bodies of plants, animals and humans in cold environments and the contents of glaciers and icebergs. Martin Skrydstrup, “Of spheres and squares. Can Sloterdijk help us rethink the architecture of climate science?” Social Studies of Science 46:6 (2016): 854-876; Peter Sloterdijk, Bubbles: Spheres Volume 1: Microspherology (Los Angeles: Semiotext(e), 2011).
Figure 3.3. Ice blocks being prepared for analysis during the *Bubble* expedition. Scholander, *Enjoying a Life in Science*, 110.
environments and frozen specimens.\textsuperscript{317} David Keilin, an important early cryobiologist, speculated in 1958 on the possibilities of using cold temperatures to prolong life and referenced Scholander’s work on arctic animals and plants as one point of departure.\textsuperscript{318}

In practice, however, the scaling between different bodies turned out to be quite tricky. Melting samples in order to access the gases inside them was easier in theory than in practice. The researchers had trouble making sure the air inside the ice was not contaminated by outside air. As Scholander and Dansgaard et. al noted in their entry in \textit{Meddelelser om Grønland}:

It was pointed out in the introduction that the primary objective of the present study was a search for ancient atmosphere entrapped in Greenland icebergs. In this our efforts were thwarted, however, because even small pieces of ice showed slight variability in the gas composition, which of course is incompatible with undisturbed air.\textsuperscript{319}

But, they also noted, these changes in gas composition would not affect the “isotopic composition of the ice itself”. They were able to conclude that the oldest ice, some of it almost 3000 years old, had the lowest $^{18}$O content and thereby was formed at a lower temperature than the younger ice. This, in turn, meant that the oldest ice came from the colder inland parts of Greenland, rather than the more rapidly changing coastal areas. Similarly to the previous expedition to Stora Breen, the Bubble Expedition validated Dansgaard’s theories without bringing any ground-breaking results. Old water and an ancient atmosphere was, once again, just around the corner, out of reach.

Even though the results of the expedition were not as unequivocal as they had hoped, Dansgaard was able to get enough material to work with to complete his PhD and defend his thesis in 1961.\textsuperscript{320} He was able to acquire a professorship the year after, in Mass Spectrometry, as a result of his research results during his years as a graduate student.

\textsuperscript{320} Dansgaard was also able to access material from the drillings conducted by the EGiG, even though he grew frustrated with their lack of collaborative ambition, see: Achermann, “Vertical Glaciology”; Willi Dansgaard, \textit{Isotopic Composition of Natural Waters, With Special Reference to the Greenland Ice Sheet} (Copenhagen: C. A. Reitzel Forlag, 1961).
Jørgen Koch, a professor at the Biophysical Laboratory, supported Dansgaard getting a professorship and specifically cited his ability to “establish a connection between physics and a number of fields within both the mathematical-physical and natural historical-geographical domains”. Dansgaard himself also lobbied for increased collaborations between those working with “physical-chemical methods” and glaciological researchers within the University of Copenhagen, bringing the Biophysical Laboratory closer to glaciological field based research. Three years after the Bubble expedition, Dansgaard, now a professor, was establishing himself as a leading figure in an emerging field. The collaboration with scientists outside of glaciology, particularly Per Scholander, had opened up a new way of scientifically knowing ice, letting it emerge as a medium for atmospheric conditions rather than as a scientific object in itself. Scaling between bodies – animal, human, plant, and ice – enabled a new outlook on the contents of glacier ice and Greenland icebergs. Willi Dansgaard’s development in isotope dating provided a more advanced and temporally deep way of understanding these jumps in scale and brought “ancient atmospheres” into the domain of glaciological research. Even though the Bubble Expedition did not render the results it aspired to, it proved to be an important endeavor for bridging practices and knowledges between physiology and biology on one the hand, and geophysics and isotope dating on the other. In Dansgaard’s own narration of his career, which began with the summer rain in Copenhagen in 1952, the Bubble Expedition marked the next milestone. As he later put it in a letter to Scholander: “Isotope Glaciology was born on Rundøy with you as its obstetrician.”

Ice, time and materiality in isotope glaciology

In the years that followed, ice core drilling would develop from a promising, but technologically underdeveloped, research endeavor to a realized and important research practice within both glaciology and climate science. The vertical element, made possible by advanced drilling technology as well as the Cold War military infrastructure in Greenland, opened up a new dimension in the scientific study of ice. In “translating the...
vertical into time”, as Dania Achermann puts it, ice core research provided temporal depth to conceptualization of climatic fluctuations and thereby enabled new disciplinary connections between glaciology and climate science.  

326 The emergence of the ice core itself as a scientific object offered a visual and material way of rendering time in ice legible in a familiar temporal language. Ice, in the form of the ice core and with the help of isotope dating technologies, could become a timeline.  

327 In early twentieth century glacier research, the inner life of glaciers remained largely unknown, as most of the scientific work on glaciers and cryosphere environments concerned mapping and surveying.  

328 Glaciers were studied for their geomorphological impact, volume, surface extension and location. The gaze was mostly horizontal and the glaciers were studied in their entirety as coherent scientific objects. During the interwar period, Ernst Sorge, a German glaciologist and part of Alfred Wegener’s third and last expedition to Greenland’s Eismitte, became interested in the way the ice sheet was structured vertically. He dug out large pits in the snow, which allowed him to quantitatively study the near-surface snow strata, and notice how older snow was visible further down, while snow that fell more recently was closer to the surface. Sorge stood at the threshold of a vertical conceptualization. He was still concerned with the individual glacier, but he was beginning to uncover a vertical dimension of stratigraphically ordered layers within the glacier itself.  

330 The field based, mostly horizontal research approach in the interwar period, spearheaded by glaciologists such as Sorge or Ahlmann, enabled a particular glacier time. Ahlmann, who had a similar approach of mapping glaciers by making annual stratigraphic measurements, used the metaphor of the “calendar” to describe his annual data gathering of glacier dynamics.  

332 The time of the ice manifested in real time: glaciers shrunk or grew

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329 Sörlin, “The Anxieties of a Science Diplomat.”
annually and were measured by Ahlmann, his colleagues as well as by local inhabitants.\textsuperscript{335} Ahlmann’s multi-site, comparative method did enable glacier ice to acquire a temporal dimension, but did not allow for connections with other geo-chronologies or non-gradualist understandings of environmental change.\textsuperscript{336} The time he was tracking in the ice was spatially bound to the glaciers themselves.

When the ice core began appearing as a scientific object in the early 1950s, it marked the beginning of a new kind of temporality of ice. The Norwegian–British–Swedish Antarctic expedition to Queen Maud Land in Antarctica (1949–1952) involved more advanced drilling endeavors and Walter Schytt, a Swedish glaciologist and student of Ahlmann, drilled a 100 meter deep core.\textsuperscript{337} Similar projects were undertaken by American and French glaciologists in Alaska and Greenland at the same time. But the cores were still too fragile and coarse to read stratigraphically, and the lack of appropriate isotope dating methods made it difficult to produce accurate geochronologies from the layers in the ice. The field based epistemology, which was more interested in the inside of the glacier than the core itself, also remained strong: sticks and thermometers were lowered into the hole in order to measure temperature inside the ice, which was deemed equally interesting.\textsuperscript{338} By the 1960s, the gaze had been turned inside-out, as interest in the core itself was surging and interest in the borehole and individual glacial dynamics were fading. A new temporal metaphor was taking hold as well: rather than Ahlmann’s calendars, ice cores were considered to be “archives”.\textsuperscript{339}

The ice-as-archive imaginary opened the door to other disciplinary connections and other sciences of the archive. Following Lorraine Daston’s notion of the scientific archive as a site which makes historical data commensurable, the role of the ice core can be understood to destabilize the demarcations of glaciological research and connect ice – in a quantified and abstracted form – with other chronologies and geographies. In the process of making planetary-scale environmental knowledge, the commensurability of ice core data would be key to connect ice with geophysical planetary processes and the burgeoning work


\textsuperscript{338} Acherman, “Vertical Glaciology,” 4.

of calculating and projecting the Earth’s climate.\textsuperscript{340} Isotope glaciology would, in other words, open the door to a planetary-scale view.

The landmark drilling at Camp Century in 1966, which reached all the way from the surface to bedrock, further cemented the ice core as a reliable “ruler” of past climate conditions.\textsuperscript{341} In the years that followed, the ice core expanded its temporal, epistemological and political authority, speaking to both distant pasts and environmental futures.\textsuperscript{342} Alessandro Antonello and Mark Carey have argued that ice cores have come to realize a singular, global temporality of the global environment, which obscure difference and multiple potential climate futures.\textsuperscript{343} The familiar visual language of vertical, stratigraphic geochronologies provided an additional rhetorical asset of the ice core as a scientific object.\textsuperscript{344}

But, as Dansgaard’s activities in the 1950s show, the emergence of ice cores as “messengers of climate change” and temporalizing technologies of the global environment did not only rely on verticality.\textsuperscript{345} Even though the Bubble Expedition did not fully realize its research goals, it enabled a different temporalization of glacier ice without drilling into it at all. The success of isotope glaciology was indeed reliant on the emergence of advanced drilling technology in glaciological practice, but it was at the same time drawing on practices and epistemologies from scientific traditions that had nothing to do with previous glaciological studies of drilling, digging and excavating the upper layers of glaciers. The historiography of isotope glaciology belongs only partly in the general historiography of glaciology. Rather, Dansgaard’s approach was based both on his interactions with geochemists and nuclear physicists in Denmark and the United States as well as his


\textsuperscript{341} Kathryn Yusoff, \textit{BiPolar} (London: The Arts Catalyst, 2008).


collaboration with physiologists and biologists. The Bubble Expedition transcended disciplinary boundaries in ways that interwar glaciology had not.

Alexis Rider notes how ice core time is not just made through the logics of chronostratigraphy. It is also a geochemical temporalization, as isotope dating follows the life – and half-life – of isotopic decay, which operates according to different temporal patterns than the linear stratigraphic way of reading time in ice.\textsuperscript{344} Ice cores are, in other words, not to be understood as adhering perfectly to the cumulative temporal order of geology, as they also bring forward the geochemical time of “starts and stops, of recombination and reconstructions”.\textsuperscript{345}

Seen in this light, the work conducted onboard \textit{M/S Rundøy} point to the non-linear and non-stratigraphic timemaking of early ice core research. Isotope glaciology was a framework for rendering ice a proxy for the atmosphere and was not primarily interested in ice itself as a scientific object. It was also not, at this pre-Camp Century point in time, primarily interested in verticality and drilling through the ice sheet. The search for “old water” was not, in its early formative years, a stratigraphic inquiry, as the isotope dating methods enabled an alternate way of measuring the age of the ice. The orientation towards “ancient atmosphere” lay the groundwork for ice as a proxy for past global temperature in the decades that would follow. The environmental timemaking of isotope glaciology was oriented to particular kinds of environmental data – primarily concerning temperature – which would later prove valuable to the efforts of mapping and modeling global climate dynamics.

In the larger history of ice core drilling, the Bubble Expedition is an awkward fit. It consisted of no drilling at all, it was based at sea rather than the ice sheet, and lacked any sense of vertical conceptualization. Yet, as I have explored in this chapter, it made possible a temporal expansion of ice with its ambitions to search for “ancient atmosphere” dating millennia back in time. In order for this research program to later on become fully realized, the vertical element of deep ice core drilling was indeed necessary, but the conceptual and epistemological work that preceded the drillings shows of the origins of ice core science transcends the disciplinary boundaries of glaciology and the temporal logic of stratigraphy.

\textbf{Conclusion}

By tracing the origins of isotope glaciology – epistemologically, geographically, disciplinarily – this chapter has aimed to show how the history of ice core drilling cannot
only be conceived of as a history of vertical expansion into the ice sheet. The proxification of ice became possible in the postwar years, both through technological advancement and Cold War infrastructure, but also through the interconnections between physiology, geophysics, isotope dating and glaciological practices.\(^{346}\) It marked the beginning of a new temporalization of ice, but also of ice as a stand-in for the planet, as the tiny air bubbles inside the ice became temporalized as “ancient atmosphere” rather than just air trapped in the ice.

The early career of Willi Dansgaard is indicative of a broader turn in the scientific study of ice. His background in geophysics and mass spectrometry and interest in the study of oxygen isotopes made him an unlikely figure to emerge as a central figure of postwar glaciology. In his research, ice was not the primary scientific object: as his activities in the 1950s show, he was rather on a search for materials he could subject to his isotope dating methodologies. As water was the medium through which he could perform his isotope analysis, he looked for “old water” in the atmosphere, the deep-sea trenches outside the Philippines and, eventually, the Greenland ice sheet. This marked a significant shift compared to interwar glaciologists who conducted a field based approach and opened up for the study of ice as a proxy science rather than a field science. Contrary to popular descriptions of ice core science, the times embedded in the ice sheet relied on much more than the drilling and discovering.\(^{347}\) The stratigraphy of the ice cores was not self-evident either, as the geological logic of strata alone was not sufficient in order for ice to become legible as a climate archive. By looking beyond the history of glaciology, I have sought to show in this chapter, we can understand the rise of isotope glaciology and ice core drilling more broadly, as a practice that did not have ice as its object of study and rather used it as a medium for answering research questions on a planetary scale.

The Bubble Expedition can, I argue, be considered as an indicative case of disciplinary, epistemological and temporal shifts in the study of ice in the early postwar era. Even though the ramifications of studying “ancient atmosphere” was not evident at the time, the possibility to use ice to track what the atmosphere was like in the past would open up for using ice as a proxy for understanding past and futures climatic shifts. The politicization of climate and carbon dioxide in the atmosphere – and the subsequent debates on habitable planetary futures – depended on knowledge about the past composition of the atmosphere. Over time, the highly localized and contingent practices of Dansgaard, Scholander and their colleagues onboard the Rundøy, would become invisible,


as the ice core data would begin to “speak for itself”, leaving its local origins in favor of a disembodied global outlook. The origins of postwar planetary timemaking were situated in particular epistemic geographies of climate science.\footnote{Martin Mahoney and Mike Hulme, “Epistemic Geographies of Climate Change: Science, Space, and Politics,” \textit{Progress in Human Geography} 42:3 (2016), 495–424.}

I will pick up on the career of Willi Dansgaard in chapter five, in which I will follow the data from the ice core drilling at Camp Century in 1966 into the burgeoning field of Earth System Science in the late 1970s and early 1980s. In the next chapter, I will return to oceanography, and how a similar isotope dating oriented research program took hold in the booming Cold War U.S. oceanography.
Timing a planetary ocean

DEEP-SEA CORE DRILLING IN THE UNITED STATES, 1958–1983

In 1951, Rachel Carson’s book *The Sea Around Us* was first released.\(^{349}\) It was a poetic, yet scientifically grounded, meditation on the world’s oceans and their history, biology, physics and chemistry. It was also a massive hit. In the years following its first publication in the United States, Carson’s book topped bestseller lists, won the National Book Award for Nonfiction and was turned into an Oscar winning documentary.\(^{350}\) In 1960, Carson was planning to release a second edition of the book, as it was still in demand nine years after the publication of the first edition. But given the years that had passed, she also wanted to make sure that her new edition would be up to date with the developments in oceanographic research. In a letter to Maurice Ewing, a prominent oceanographer and director of the Lamont-Doherty Geological Observatory in New York, she specifically asked for new articles on the topography of the Atlantic Basin and a photograph of “a scientist examining cores after their removal from the coring tube.”\(^{351}\)

Carson had written about deep-sea cores in her first edition as well, focusing on the then recent *Albatross* expedition and the development of the Kullenberg piston corer. She had called the deep-sea cores “a sort of epic poem of the earth”.\(^{352}\) But by 1960, deep-sea cores were emerging as a part of a Cold War research program in physical oceanography as well as a metonym for the geohistorical qualities of the ocean floor.


\(^{352}\) Carson, *The Sea Around Us*, 43.
Figure 4.1. Image of a coring tube being sent down into the ocean. Rachel Carson added this photo for the second edition of *The Sea Around Us* in 1960, after Maurice Ewing provided it to her. Rachel Carson, *The Sea Around Us*, 2nd Edition (New York: Oxford University Press 1960), 44.
Hans Pettersson, the leader of the *Albatross* expedition, exclaimed in 1954 that Maurice Ewing and his ambitious agenda to sample the ocean floor for deep-sea cores, were growing “more and more expansive”.353 This expansive program was, when Carson wanted to include the core photograph in her new edition, getting up to full speed.

It was a time of intense development in oceanography in general, and American oceanography in particular. The International Geophysical Year had ended in 1958, and given further incentives to keep doing large-scale research projects in the ocean sciences and strengthen collaborative efforts across institutions and nations.354 Cold War tensions had brought the ocean sciences into the spotlight. They enabled both funding opportunities and research infrastructure, but were also shaping the research conducted by U.S. oceanographers.355 The 1960s would feature revolutionary developments in plate tectonics, which rewrote the history of the planet and enabled a new explanation for the geological past of the continents and the sea floor.356

Deep-sea core drilling was, during this transformative time, a part of a Cold War oceanography in the United States. This chapter follows the institutionalization of deep-sea core drilling in the early 1960s up until the early 1980s. Over the course of these two decades the temporalities of deep-sea core drilling changed dramatically. The geologically oriented research program from the early 1960s, with its emphasis on geological history and the structure of the ocean floor, would in the 1970s turn towards other research questions. In particular, questions of environmental change emerged as an alternative temporal framework of deep-sea core drilling. Deep-sea core drilling was an influential technology for marine geology, but, as environmental issues, and particularly climate change, rose on the scientific and political agenda, additional temporalities were added to the scientific framework.357 Deep-sea core drilling became a practice that scaled between geological, environmental and political temporalities, well before the advent of concepts such as the Anthropocene.358

353 See chapter two of this thesis.
The chapter is divided into three parts: the first traces the origins of institutionalized deep-sea core drilling in the United States between 1958 and 1968. The second part concerns the Deep-sea Drilling Project (DSDP) (1968–1983), which was a multi-institutional effort to globally sample the ocean floor for deep-sea cores, and how the project became increasingly interested in environmental data production. The third part is about another large-scale research program called Climate: Long range Investigation, Mapping, and Prediction (CLIMAP) (1971–1982). This project utilized deep-sea cores to trace the climatic and geophysical history of the world ocean since, primarily, the last major glaciation 18,000 years ago, but some studies ranged even longer back in history, into the mid-Pleistocene. Inferences were drawn in matters such as temperature and circulation patterns of surface waters, the chemical nature of bottom water and the distribution of sea ice coverage. But the project also moved towards societal issues as it went along, and the scientists involved saw opportunities to move towards using their data to prognosticate future climatological, economic and agricultural issues. In this sense, deep-sea core drilling activities blurred the line between what Reinhart Koselleck called the historical and the "spatial pregivens" of the ocean floor. If the deep-sea core for Rachel Carson in 1960 was a symbol for the geological past hidden inside the ocean floor, it was, by the early 1980s, becoming a far more equivocal symbol, encompassing geological histories and environmental futures. It is a history which can be seen in light of the configuration of what Jessica Lehman calls the Anthropocene ocean, referring to the ocean as an integrated part of planetary dynamics. Deep-sea core drilling, I argue, has a place in this history, and brings forward how the synchronization of disparate ocean temporalities was necessary for a particular kind of planetary-scale idea of the ocean to take hold.

362 Lehman, "Making an Anthropocene Ocean".

As the International Geophysical Year (IGY) was coming to an end in 1958, American oceanography was a field with an ambivalent relationship to international cooperation. On the one hand, the IGY had proven to provide new arenas for collaboration and productive interfaces between different national research programs, but on the other, the national interest in oceanography, and the perceived need to be “first” in relation to Cold War antagonists, had made many scientists turn inwards to nationally oriented projects.\(^{363}\) The National Academy of Sciences Committee on Oceanography (NASCO) was founded after the IGY as a way to mobilize national resources in oceanography and stay ahead of Soviet competition.\(^{364}\)

Oceanography was becoming a prestige project, akin to nuclear physics and astronomy, in the political imagination. As military funding came flowing in, particularly to physical oceanography, imaginaries of bright ocean futures took hold. The ocean floor emerged as especially promising: hopes were high that humans, in the near future, would mine, farm and live on the ocean floor thanks to developments in technology.\(^{365}\) In his address to Congress in 1961, John F. Kennedy echoed a similar sentiment, and claimed that knowledge of the ocean was about more than just curiosity: “our very survival may hinge upon it”.\(^{366}\)

The ocean floor was simultaneously a site for geopolitical tensions and power struggles, an imagined solution to the problems of mankind, and a source of information for the history of the planet. It became a space in which American scientists were, as Rachael Squire puts it, “seeking to extend the American frontier into new depths, volumes, and elements as it had been to the skies, the Arctic, and outer space”.\(^{367}\) The competing imaginaries of the ocean floor in the postwar decades have already been the subject of numerous studies by historians of science, diplomacy and environment. Surabhi Ranganathan has shown how decolonization in the 1960s drove neo-Malthusian fears of overpopulation and resource scarcity, leading both public and private actors to direct their attention to the oceans as territories to be utilized.\(^{368}\) The oceans were increasingly


\(^{364}\) Hamblin, *Oceanographers and the Cold War*, 142.


\(^{366}\) Adler, *Neptune’s Laboratory*, 155.


acquiring a vertical dimension: previous legal and political frameworks had focused on the surface, but now, the depths and floor of the oceans were coming into view as political and economic zones. According to Ranganathan they were not, however, just being incorporated into legal frameworks, rather the frameworks themselves reified an extractive imaginary and a neo-colonial commercialization of the ocean floor.

In addition to the extractive and neo-colonial visions, the ocean floor also emerged as a site of surveillance and geopolitical dominance. There was a “surveillance imperative” behind much of the scientific activities taking place under water. Military patronage shaped, as numerous historians of science has elaborated on, how knowledge production was conducted in the oceans. Given the inaccessibility of the ocean floor, and the impossibility of mapping, sensing and experiencing it without advanced technological means, it was an activity particularly dependent on existing infrastructure and the tools through which the ocean floor could be mediated. The many unknowns of the ocean floor – topographical, mineralogical, geological, biological – rendered the work of exploring it an activity of anticipation of what might be down there. As Beatriz Martínez-Rius points out, the future imaginaries of the ocean floor had to take shape before they were actually “technologically possible and economically feasible”.

This temporalization of the ocean floor, with a deep and mostly unknown geological past and an even more unknown, yet possibly bright and prosperous near future, was still quite rudimentary, but the ocean floor was coming into view as a territory with a past and a future.


For the practice of deep-sea core drilling, a surging interest in the ocean floor, emerging in multiple sectors at once – military, commercial, scientific – proved a fertile ground for organized drilling activities. It is, as David van Keuren has shown, hard to overstate the importance of the petroleum industries in the emergence of American scientific ocean drilling. In California, the oil companies Continental Oil Company, Union Oil, Superior Oil and Shell Oil founded the exploration group CUSS in 1949 in order to mobilize resources towards offshore drilling and technology development. They were able to transfer shore-based drilling technologies onto ships, and thereby expand the prospective areas for oil extraction to deeper waters. In 1958, CUSS was disbanded, and its previous chief Louis N. Waterfall founded an independent company, Global Marine Exploration Corporation, which drew on the technology development in CUSS but functioned as an operational subsidiary to larger oil corporations. By the early 1960s ship-borne platforms were beginning, through Global Marine, to become a commercially viable way of drilling for oil.

At the same time, scientific ocean drilling had also been developing. After the Albatross expedition and the invention of the Kullenberg piston corer, deep-sea core drilling had become an increasingly promising field of research. Gustaf Arrhenius, a member of the Albatross, began publishing the results from the expedition in 1952, and further sparked international interest in the scientific opportunities opened up by deep-sea cores. Cesare Emiliani, building on the work of Arrhenius, but also incorporating a more advanced stable oxygen isotope dating method, was able to connect deep-sea cores with temperature. His 1955 article “Pleistocene Temperature” was a landmark achievement and an early precursor to the environmentally oriented deep-sea core research that would emerge more prominently in the 1970s.

The major U.S oceanographic institutions were also beginning to, albeit at a small scale, get involved in a more coherent deep-sea core research program. Maurice Ewing, the director of the Lamont-Doherty Geological Observatory (LDGO), had a long standing interest in the geology and geophysics of the ocean floor and had, by the 1950s, risen to the top of U.S. geosciences. The LDGO was founded in 1949, after a private donation to Columbia University by Florence Corliss Lamont, the widow of banker and J. P. Morgan chief executive Thomas W. Lamont. The observatory was housed in the Lamonts’ former

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377 van Keuren, “Breaking New Ground,” 185. See also: Tyler Priest, The Offshore Imperative: Shell Oil’s Search for Petroleum in Postwar America (College Station: Texas A&M University Press, 2009).


weekend estate north of New York City and Maurice Ewing was hired to be its first director. He remained in his post until 1972.379 Spurred by influx of defense funding and Cold War tensions, LDGO quickly emerged as a leading research institution and became a “scrappy, energetic rival of the earlier established Scripps Institution of Oceanography and the Woods Hole Oceanographic Institution.”380

Deep-sea core drilling was a part of the research activities at LDGO from the start. Maurice Ewing himself, with his research interests in geology and geophysics, saw the cores as a promising part of the research goals of the LDGO. He equipped the research ship Vema with a modified Kullenberg piston corer to incorporate coring in daily work at sea.381 In comparison to previous deep-sea core drilling activities, such as the Albatross expedition and C. S. Piggott’s pre-war attempts, the integration of the piston corer into the continuous work on the Vema enabled a vastly higher number of cores to be recovered. The deep-sea core work at LDGO was led by David B. Ericson, who had not gone through PhD training, but knew Ewing and had a long standing interest in fossils. Together with his assistant, the Swede Gösta Wollin, who had no scientific background, Ewing, Ericson and Wollin formed an unlikely trio.382 The paleoceanographer John Imbrie, who would later be central in both CLIMAP and the Earth System Science Committee, worked at Columbia at the time, and recalled that Ericson and Wollin worked with their core analysis independent from other activities at LDGO.383

Ericson and Wollin’s small-scale enterprise is indicative of the marginal role of deep-sea core drilling in relation to the massive influx of funding pouring into oceanography and the geosciences. Wollin was not formally hired by LDGO, but instead worked as personal assistant to Ericson, getting payed on a month to month basis.

379 Edward C. Bullard, Maurice Ewing, 12 May 1906-4 May 1974: Biographical Memoirs of Fellows of the Royal Society (London: Royal Society, 1975). The institution was initially called the Lamont Geological Observatory, but Doherty was added in 1969 after another donation by Henry Latham Doherty. In 1993, the LDGO changed its name to the Lamont-Doherty Earth Observatory, which is still its name today.


381 The Vema was a private yacht turned into a research ship, which could host 13 crew members and the one ton heavy piston corer, as well as a range of other smaller instruments and sampling technologies. Maurice Ewing and Bruce C. Heezen, “Oceanographic Research Programs of the Lamont Geological Observatory,” Geographical Review 46:4 (1956): 508-535.


Top left: Figure 4.2. The R/V Vema leaving New York City. Maurice Ewing and Bruce C. Heezen, "Oceanographic Research Programs of the Lamont Geological Observatory", Geographical Review 46:4 (1956), 512.

Bottom left: Figure 4.3. David B. Ericson with a few core samples. (https://history.aip.org/exhibits/vema/index.html)

Right: Figure 4.4. Map of the coring sites of the LDGO between July 1947 and June 1956. Ewing and Heezen, "Oceanographic Research Programs", 529.
His main interest, Ericson thought, was just to “get some extra money.” The library of cores that was rapidly accumulating was at first stored in a garage before proper facilities could be put in place.

In a 1953 report, Ericson described the day to day work with the deep-sea cores that were constantly coming in to the research facilities at LDGO:

As soon as the cores arrive at the laboratory they are unpacked, extruded, split, photographed, labelled, sampled, and stored. From all the cores samples are taken for preliminary microscopic investigation, and from some cores samples are taken for density determinations, sound velocity measurements, $^{14}C$ determinations, paleo-temperature determinations by the oxygen isotope method, CaCO$_3$ determinations, and other chemical analyses.

Ericson describes a multi-faceted and increasingly standardized labor of categorizing and analyzing the influx of new cores. The wide range of methods applied to the cores, stemming from geology, geophysics and geochemistry, also reveals the temporal multiplicity of the work conducted by Ericson and his colleagues: the stratigraphy of the cores appears as just one temporal framework among many, as the broad range of dating methods enables multiple temporalizations of the core contents. Ericson stressed the different possible histories as an asset of the breadth of deep-sea core research and noted how he hoped to “add to the knowledge of the structure of ocean basins, the origin of continents, the chronology of Pleistocene climatic changes, diagenesis, and the origin of

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384 David B. Ericson, Letter to Maurice Ewing, July 20, 1962, MEP, box 91-350.128. Wollin appears as an outlier in the exchanges between Swedish and American oceanography at the time. He had no scientific training and was a minor celebrity in Sweden as he was present at D-Day during World War II and chronicled his experiences in the daily Dagens Nyheter. It is unclear how he ended up at LDGO, but J. Lamar Worzel, who worked with geophysics at LDGO at the time, recalled that Wollin happened to live nearby and somehow got acquainted with Ericson. They both shared a Swedish heritage, which could have connected them. Wollin’s extroverted and eccentric personality worked well with the introverted Ericson but, according to Worzel, Wollin gradually came to speak on Ericson’s behalf, thereby alienating colleagues at LDGO. Wallace Broecker, who was also at LDGO at the time, described Wollin as “basically bonkers”. Staffan Kihlström, “Gösta Wollin landade i en kökssträdgård,” Dagens Nyheter, June 6 edition, 2014; Spencer Weart, Interview of Wallace Broecker on November 17, 1997, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD: https://www.aip.org/history-programs/niels-bohr-library/oral-histories/23909-1. Accessed 2023-02-19; Ronald E. Doel, Interview of J. Lamar Worzel on January 3, 1996, Niels Bohr Library and Archives, American Institute of Physics, College Park, MD: https://www.aip.org/history-programs/niels-bohr-library/oral-histories/6914-2. Accessed 2023-02-19.


petroleum.” The cores varied in size – they were somewhere between one and thirteen meters long – and in age as most of them covered some or all of the Pleistocene, but a few reached slightly further back. At the same time Ericson stated that more cores were needed to fully understand the temporal and geographical limits of their research endeavor. Most of the cores (see fig. 4.4) stemmed from the Atlantic and the gaps from other oceans were, at this point, still significant.

Around this time, Cesare Emiliani was establishing his core laboratory at the Institute of Marine Science at the University of Miami and Scripps was also beginning to work systematically with coring as a standard methodology. There was no national structure for coordinating the deep-sea core drilling community, rather, correspondence between the scientists indicate a more loosely connected network with quite divergent methods and foci in their analyses of the cores.

As these initiatives were taking shape at LDGO, Scripps and Miami where funding was increasing, there were also more grandiose dreams connected to drilling in the ocean floor. In March 1957 oceanographer Walter Munk at a meeting of a National Science Foundation advisory panel for geosciences, launched the idea of drilling down to the Mohorovicic Seismic Discontinuity (it is more often referred to as just Moho). The Moho marks the border between the crust and the mantle. Reaching all the way down there, the logic went, would be scientifically relevant as well as a spectacular advance similar to the dramatic developments happening in physics and astronomy at the time. The ocean floor was, according to studies by Maurice Ewing, at particular places much closer to the discontinuity, which would make such a project feasible and more beneficial than drilling on land. When the drilling activities started, outside the Caribbean island Guadalupe, in 1961, it marked the beginning of a big science approach to scientific ocean drilling. Utilizing technology from Global Marine and the oil industry, it also showed the scientific possibilities opened up by offshore oil prospecting. However, reaching the Moho proved

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388 Ibid, 8-10.
389 One example at Scripps is the Capricorn expedition of 1952. Gustaf Arrhenius of the *Albatross* expedition was part of the crew and conducted a similar style of research as he had in Gothenburg. Scripps Institution of Oceanography, *Shipboard Report: Capricorn Expedition 26 September 1952 – 21 February 1953* (San Diego: Scripps Institution of Oceanography, 1953).
392 The project was also widely covered in American press. In 1961, for example, author John Steinbeck reported from the CUSS 1 – one of the ships involved in Project Mohole – for *Life Magazine*. John Steinbeck, “High Drama of Bold Thrust through the Ocean Floor,” *Life Magazine*, April 14, 1961.
393 Van Keuren, 190.
challenging. Despite the generous funding, the drilling never reached all the way down to the discontinuity and the project became increasingly criticized. By 1963, the project was publicly mocked and was rapidly losing political and scientific support. By 1966, the Mohole project, as it was often referred to, was defunded by the U.S. House of Representatives.\textsuperscript{394}

I will not dwell further on the Mohole project, as it has already been widely covered by both historians and the actors themselves and falls beyond the scope of this chapter, but it is impossible to trace the postwar history of scientific ocean drilling without mentioning Mohole.\textsuperscript{395} Despite the financial and practical issues Mohole developed in the early 1960s, it proved that scientific ocean drilling which utilized technology from the oil industry could be done at a large scale. But rather than opting for one extremely deep hole suggestions rose that the same infrastructure could be used for continuous ocean drilling in a vast amount of places.\textsuperscript{396}

In 1962, Cesare Emiliani proposed his project Long Cores (most often he used the tongue-in-cheek abbreviation LOCO), which would expand the temporal scope of the study of the oceans and involve all the leading U.S. oceanographic institutions. Emiliani invited a group of leading ocean scientists to form a “LOCO committee”, among them his Miami colleague and Albatross alumni Fritz Kozc, LDGO’s Maurice Ewing and Bruce C. Heezen as well as representatives from Scripps, Woods Hole, and Princeton. A stated goal was to be independent from the Mohole project, even though Emiliani wanted to maintain the collaboration with Glomar Marine.\textsuperscript{397} The expansion in temporal scope opened up by deep-sea core drilling was the main motivation:

Current observations on geophysical, geochemical, and biological processes only cover a point in time, even if extended through human scientific history. A better understanding can be achieved not only by refining current observations, but by extending them through a time interval comparable to the duration of the processes themselves. In most cases, this duration ranges from tens of thousands to many millions of years.\textsuperscript{398}

\textsuperscript{395} Van Keuren, “Breaking New Ground”; Hamblin, Oceanographers and the Cold War, 159-167; Bascom, A Hole in the Bottom of the Sea.
\textsuperscript{397} Shor, “A Chronology,” 396.
Emiliani, who had developed oxygen isotope dating methods for deep-sea sediments, and thereby had a particular interest in tracing temperature patterns on geological timescales, stressed the importance on establishing generalized temperature curves. Access to temperature data, he argued, could open the door to other scientific inquiries, as a general temperature curve “is of great importance because it provides a dated frame of reference of a variety of geophysical, geochemical, and biological process, and their rates.” Among these processes were “the growth and waning of Pleistocene glaciers, glacial and non-glacial changes of sea levels, tectonic movements, weathering cycles, meteorological phenomena, etc.” These kinds of processes were only possible to accurately date with a large enough number of cores, so that local differences and variabilities were evened out.

The times of the deep-sea cores did not, as the LOCO memo indicates, have a clear disciplinary home. Rather, they appeared as datasets relevant to a wide set of fields, and particularly in matters concerning geological periodization and seismography. Concerns regarding environmental, meteorological and climatological issues were part of the program, but did not have a pronounced role. Shortly after LOCO was launched and had successfully acquired funds from the NSF, the “big three” of U.S. oceanography – Scripps, LDGO and Woods Hole – were dissatisfied with the strong position of University of Miami. They decided in 1963 to start the Consortiums of Oceanic Research and Exploration (CORE) and applied for money from the NSF independently from LOCO. The internal competition among U.S. institutions was not appreciated by the NSF, who suggested that the two programs should be consolidated into one.

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400 Emiliani, “Outline of a Research Program.”
401 Henry King Stanford, the president of the University of Miami, lobbied the presidents of the other involved institutions to support the joint efforts that would be led by Miami. LOCO, he argued, was more modest than Mohole, and thereby also more financially beneficial for all the involved actors. Henry King Stanford, Letter to Franklin D. Murphy, President of the University of California, October 19, 1963. MEP, box 91-350/281.
402 Van Keuren, “Scientific Ocean Drilling,” 199. Access to cores had been a contentious issue since the mid-1950s, often in combination with divergent dating methods and publishing strategies. Emiliani, having a smaller amount of samples at hand in Miami, borrowed cores from LDGO, but was simultaneously criticizing the methodologies of Ericson and Wollin, which created friction. E.g Cesare Emiliani, Letter to Maurice Ewing, July 28, 1961, MEP, box 91-350/228. Emiliani was also, on an interpersonal level, not appreciated by scientists at LDGO. In a letter to Maurice Ewing, William E Donn, a scientist at LDGO, complained that “despite the incredibility of Emiliani as a person”, his critical review of an article by Ericson, Ewing and Wollin had merit. Emiliani had, when submitting the review, added a personal message in which he claimed that his colleagues had advised him against writing such a harsh review, but after consulting his astrologist he had decided, being an outspoken Sagittarius, to proceed anyway. “It is the longest, roughest, most elaborate and most detailed review that I have ever written”, Emiliani wrote, and jokingly suggested that the review, rather than the paper, could be published in Science. He described the article, in his review, as “extremely unsatisfactory from nearly all points of view”. Their disagreements
In 1965, this process of consolidation was finalized with the establishment of the Joint Oceanographic Institutions for Deep Earth Sampling (JOIDES), which incorporated all four institutions (Miami, LDGO, Scripps and Woods Hole). In the agreement, all involved institutions declared that deep-sea core drilling was a direction of research with “a high probability of being rewarding” and that a formalized cooperation would be beneficial for all parties involved. Investigating the ocean floors through the laboratory examination of samples obtained from a considerable depth was, the agreement said, “a pressing need” for oceanographic research. 403

JOIDES rapidly acquired significant funding from the NSF, which enabled the joint institutions to acquire a larger research vessel with more advanced drilling technology. Scripps were awarded the funding and in turn contacted Glomar Marine – the same company that had provided the technology to Mohole – to build the ship in question. 404 The new ship and the institutional formalization of deep-sea core drilling enabled a more coherent research program. Additionally, there were concerns within JOIDES that the activities needed to continuously expand in order to remain competitive. If the project would not be enlarged it would mean “dire consequences and the slow death of JOIDES” according to some members in the JOIDES Atlantic Executive Committee. 405

When the ship stood ready in 1968, constructed especially for the task of drilling for deep-sea cores, the practice of sampling the ocean floor and recovering cores had gone from a dispersed and difficult enterprise to an institutionalized and practically feasible scientific research program. The ship was called the Glomar Challenger, a name alluding to both the HMS Challenger and the oil corporation Global Marine, and it brought together the coring technologies developed within oceanography with the latest technology from the oil industry as well as navigation and sensing systems from the military. 406 It was staffed with mostly concerned the dating of the Pleistocene and the establishment of a timeline of the period. William E. Donn, Letter to Maurice Ewing, September 1, 1964, MEP 91-350/228; Cesare Emiliani, Letter to the editor at Science, undated 1964, MEP 91-350/228; David B. Ericson, Maurice Ewing, and Gösta Wollin, “The Pleistocene Epoch in Deep-Sea Sediments,” Science 146:3645 (1964): 723-732.

403 JOIDES Executive Committee, “Agreement,” September 2, 1965. MEP, Box 91-350/281. The rapid growth of available deep-sea cores, especially those from deep ocean basins, enabled a temporal expansion of deep-sea core research. In 1966, Maurice Ewing hosted a symposium on “Pre-Pleistocene Ocean Sediments” at the Geological Society of America, with the motivation that “very rapid progress has been made in this area during the last year due largely to greatly improved ability to select the best sites at which to core.” Maurice Ewing, Letter to Agnes Creagh, Secretary at the GSA, April 6, 1966. MEP, box 91-350/8.


scientists from the JOIDES institutions but with a crew from Global Marine. JOIDES was also, with this latest expansion in funding and equipment, launching the Deep-sea Drilling Project (DSDP).407


When the Glomar Challenger departed Orange, Texas and set sail for Hoboken, New Jersey, on July 20, 1968, it marked the beginning of the Deep-sea Drilling Project. It would run from that day until 1983. Maurice Ewing was the leader of the first leg of the DSDP.408 In the foreword to the first volume of DSDP reports, Leland J. Haworth, the director of the NSF, framed the project as a successful collaboration between oil companies and scientific research institutions. He saw it as a project that was primarily meant to investigate the geological history of the ocean basins. But even though the research was historically oriented, he also asserted that the DSDP would produce “knowledge that should be valuable to people of all nations in planning the future course to take in the use of both land and sea areas.”409 The idea to use new oceanographic knowledge to enable a utility oriented understanding of the oceans was not unique to the DSDP.

At the same time as the DSDP was taking off, broader political interests in the ocean depths were forming. President Lyndon B. Johnson saw oceanography as an important science for both securing future resources and maintaining diplomatic relationships with other countries. In 1966, the Panel on Oceanography of the President’s Science Advisory Committee released the report Effective Use of the Sea. It defined the most important goals for U.S. oceanography to be to enable “effective use of the sea by man for all purposes currently considered for the terrestrial environment: commerce; industry, recreation and settlement; as well as knowledge and understanding.”410 Rather than conceiving of the oceans as yet another arena for competition, Johnson framed his marine policies in an international and collaborative manner, and put them at the center of his scientific agenda. He outlined the framework of what would become the International Decade of Ocean Exploration (IDOE) in his 1968 State of the Union Address and further stressed the

collaborative aspects of such an effort, possibly in an attempt to offset negative repercussions from the Vietnam War.\textsuperscript{411}

The IDOE could take shape as an international collaborative project, hosted by UNESCO, but with significant support from the United States.\textsuperscript{412} Over the duration of the program – it was running between 1971 and 1980, the IDOE came to enroll several other nations in a range of projects concerning geophysical, geochemical and biological aspects of oceans and ocean environments.\textsuperscript{413} The UNESCO leadership for the IDOE had to navigate a tricky political geography of the oceans: decolonization, the establishment of the United Nations Law of the Sea, large corporations and states prospecting for oil and other resources, were all complicating factors which challenged the scientific and political optimism that UNESCO represented.\textsuperscript{414}

The political agenda of the IDOE also came to shape how oceanographic research was conceptualized. It marked a step away from “descriptive” oceanography and a turn towards a more materially and conceptually engaged relationship with the oceans. The 1969 report \textit{An Oceanic Quest: The International Decade of Ocean Exploration} was written by a committee of U.S scientists from oceanography and ocean engineering and aimed to outline the scientific goals of the IDOE. A returning phrase in the report is the ambition to create an opportunity for “rational ocean utilization”. In the preface to the report, the Steering Committee members outlined their ambitions for the IDOE and how this project was supposed to break with earlier forms of marine sciences:

> During the past decade, marine science has been pursued with the principal goal of gaining fundamental understanding of ocean processes. Although this goal has not been fully achieved, important progress has been made. We consider it appropriate that in the coming decade, emphasis should be given to the goals of prediction and of enhanced and rational utilization. In subsequent years, accumulated knowledge should ultimately lead to the enlightened and responsible stewardship of our ocean heritage.\textsuperscript{415}

\textsuperscript{411} Hamblin, \textit{Oceanographers and the Cold War}, 245.
\textsuperscript{412} President Lyndon B. Johnson called the IDOE “an historic and unprecedented adventure” in 1968, while lobbying the project in the UN. Richard Paul Shaw and Curtis A. Collins, “Program news: The international decade of ocean exploration... and beyond,” \textit{Marine Geodesy} 2:2 (1979): 189-195.
\textsuperscript{414} Hamblin, \textit{Oceanographers and the Cold War}, 250.
The language in the quote above reappears in the foreword by Leland J. Haworth of the first report of the DSDP. Large-scale deep-sea core drilling activities were, in other words, co-evolving with a scientific and political incentive to emphasize “goals of prediction” and “rational utilization” of the ocean. Even though the DSDP was not formally a part of the IDOE, it was a NSF project and closely tied to the ideals and goals of the national research strategy. Given the dual goals of the DSDP – scientific research and oil and resource prospecting – it was also tied to the ongoing debates around ocean resources and decolonialization. Jacob Darwin Hamblin notes how many scientists were ambivalent about the IDOE framing and its emphasis on applied ocean science, but nevertheless came to, at least partially, embrace the IDOE and its ample funding opportunities. The first DSDP publications mostly emphasized its geological aspects – histories of continental drift, ocean floor spreading and magnetic fields and reversals – but also brought forward how it was an endeavor aiming to predict and control planetary processes and “intelligently exploit” the Earth’s resources. The DSDP had a vast organization which consisted of 14 panels and involved hundreds of scientists, which makes it hard to pinpoint one, singular research approach of the project. The self-presentation of the DSDP JOIDES Executive Committee gives some insight into the negotiations on the purpose and direction of the DSDP. Even though individual scientists such as Maurice Ewing often stressed their own research interests when talking about DSDP and their role within it, the leadership of the project continuously had to shape the agenda in order to fit with larger political and scientific tendencies.

Between 1971 and 1981, the DSDP was reviewed annually by the NSF. These reviews, and the presentations given by the DSDP leadership, provide a glimpse into the

417 Hamblin, Oceanographers and the Cold War, 249.
419 The 14 panels consisted mainly of scientists from the JOIDES institutions (Scripps, LDGO, Woods Hole, University of Washington and University of Miami), but also involved engineers and representatives from the petroleum industries. The panels were, at the start of the DSDP (they would come to change as the project expanded internationally in the mid-1970s): JOIDES Executive Committee, JOIDES Atlantic Advisory Panel, JOIDES Planning Committee, JOIDES Business Advisory Committee, JOIDES Pacific Advisory Panel, JOIDES Gulf Advisory Panel, JOIDES Cores Description Manual Panel, JOIDES Sedimentary Petrology and Geochemistry Panel, JOIDES Igneous and Metamorphic Petrography Panel, JOIDES Panel on Interstitial Solutions and Organic Geochemistry, JOIDES Paleomagnetism Panel, JOIDES Information Handling Panel, JOIDES Heat Flow Panel, JOIDES Well Logging Panel, Petroleum Company Research Advisory Panel and Associates as well as a few minor panels for scientific and technical advice and assistance. Deep-sea Drilling Project, Deep-sea Drilling Project Initial Reports, Volume 2 (La Jolla, CA: Scripps Institution of Oceanography, 1969).
420 Maurice Ewing would, for example, often focus on continental drift and geological periodization when he presented the work of the project. E.g Maurice Ewing, Notes for presentation at Society of Exploration of Geophysics, November 9, 1970. MEP, box 91-350/20.
self-presentation of the project and how it came to respond to the changing research priorities around it. The review meetings featured presentations on the state of the project, research objectives, past and future drillings, budget issues and securing of future funding for the project. These meetings also highlighted the multiple temporal layers underpinning the work in the project: the scientists had to consider the present relevance of the deep past, and reconcile geological history, geochemical timescales, ocean-atmosphere interactions as well as the future prospects of resource exploitation in the deep ocean.

On April 26, 1971, in a conference room at the NSF in Washington D.C, a small group of scientists and NSF staff had gathered for the first review meeting, three years after the start of the project. The DSDP was represented by William Nierenberg, the Director of Scripps, Melvin A. Peterson, Scientific Director of the Ocean Sediment Coring Program and oceanographer at Scripps, and N. Terence Edgar, the Chief Scientist of the DSDP and oceanographer at Scripps as well. In their presentation, they outlined the results of three years of work and then went on to present future plans and motivate their claims for additional funding and continued deep-sea core drilling with the Glomar Challenger. The presentation was oriented towards the geological and geomagnetic. In his introductory remarks Melvin A. Peterson stressed the role of deep-sea core drilling in the rapid developments of proving theories of ocean floor spreading and continental drift. Even though David van Keuren argues that the deep-sea core scientists in retrospect tended to overemphasize the importance of deep-sea core drilling in the debates over the existence of continental drift, Peterson could latch on to a rapidly developing research topic. By 1971, the debates had already been more or less settled, but they were still an attractive example of the prospects of postwar geosciences and could be according to Peterson be compared to early twentieth century physics.

Deep-sea cores were, in this framing, primarily a tool for modern geology. Their temporalities were unidirectional: they could help establish a geological history that was

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421 After being awarded to be host institution, it appears as if Scripps played a more prominent role than the other JOIDES institutions. As the cores were stored at Scripps and LDGO, they naturally assumed a more central position in the deep-sea core infrastructure. Others went in the opposite direction. Cesare Emiliani, who had played a central role in the early development of deep-sea core drilling and sought to have Miami as node for deep-sea core research left his JOIDES positions in 1970. His Miami colleague and former Albatross crew member, Fritz Koczy, who led much of the deep-sea core work in Miami passed away unexpectedly in 1966. Minutes of the JOIDES Atlantic Advisory Panel, October 7, 1970. MEP, box 91-350/281.


424 Van Keuren, “Breaking New Ground,” 184,
rapidly getting discovered. But, given the stated ambition by the NSF to move from descriptive to applied, this would not be enough. In his presentation Peterson addressed these concerns head on and brought forward other temporalities than those of sea floor spreading and continental drift:

Paleo-oceanography is a very current subject. Geologists commonly refer to Hutton’s famous law: ‘The present is the key to the past.’ The systems operating in the oceans are sufficiently complex that our knowledge of them is still rudimentary. Seeing the successive developmental stages, the influence of the changing shapes of the ocean basins on current distributions, productivity, climates, and many other factors, permits scientists to view ‘process’ with the full perspective of time.  

Deep-sea cores were, in other words, not only about geological periodization and continental drift. By enabling “the full perspective of time” and trace ocean processes on physical, chemical and biological levels, the temporal depth of DSDP could be situated in another kind of temporalization of ocean knowledge. Peterson acknowledged the multiple temporal scales that could be of interest to the project and connected them to the political and economic needs of the present. But it was not clear exactly how these processes would be tracked and the “rudimentary” state of the understanding of ocean dynamics still left many things open.

When N. Terence Edgar took the stage after Peterson’s introduction he talked about the scientific achievements so far, and once again the very long geologic timescales came into focus. Time was implicitly the distant geological processes of ocean floor spreading and continental drift. Deep-sea core drilling was understood and framed primarily as a geologic scientific practice which, even though the more applied sides were mentioned during the presentation, still had as its primary objective to produce basic research about the deep past of the planet. Rather than resource prospecting or environmental concerns, the main argument for a continued DSDP concerned time itself.

In his concluding remarks, Peterson framed the attempts to understand the history of the ocean floor as a continuation of a heroic ocean exploration of the deep-sea.  

He could thereby allude to the frontier narratives around the oceans circulating at the time, while simultaneously situate the DSDP as an enterprise situated among the great heroes of Western ocean sciences. Two hundred years ago, Peterson said, Captain Cook explored two dimensions of the oceans, outlining their perimeters and mapping their horizontal

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properties. One hundred years ago, he continued, *HMS Challenger* introduced "a third dimension, basically the depths, the broad depth contours of the oceans basins." Today, he concluded, "one hundred years later, the *Glomar Challenger* is introducing this third dimension of time. How did the oceans get like they are today?"\(^{428}\)

The scientific production of time could, in this setting, be interwoven with a longer history of Western narratives of heroic exploration. Even though the geo-historical focus presented by Peterson did not explicitly concern environmental, climatological or human futures, it further emphasized the possibility of considering the ocean as a dynamic environment. The early results of the DSDP indicated that the tacit assumption in the earth sciences, which posited that the oceans “constitute a massive buffer system, stabilizing climate and chemistry on land and sea”, was in fact more complicated. The DSDP had begun to reveal that “dramatic changes have occurred”, both on local and global scales in specific intervals of the geologic record.\(^{429}\)

1972 marked the end of the first two phases of the DSDP and the introduction of Phase 3. Once again, N. Terence Edgar stood before the NSF representatives in order to update them on the progress of the project and the future need for funding and research infrastructure. Many things were similar to the year before, but the beginning of Phase 3 meant some changes in the research focus. Phase 1 had been primarily concerned with the problem of age and origin of ocean basins, while Phase 2 had emphasized the validity of theories of ocean floor spreading, but also encompassed studies in the vertical motion of the crust, geochemistry, igneous petrology and paleoclimatology.\(^{430}\) Many of the activities that began in the first two phases would continue in Phase 3, but it some things would change. Rather than having “a single objective in mind: to reach old sediment and basalt”, the phase would have a more multi-facetted way of conducting the drillings. This was due to improved drilling capacities and new drills, which made older sediments less laborious to reach. The previous drillings were another resource. They were at this point so numerous that they made up a “matrix of information”, to which additional information and new forms of data could be added.\(^{431}\)

The broader framework also meant, according to Edgar, that the DSDP was scaling up its spatial view: “Inherent in all these studies is the notion that we are no longer studying just the ocean, but our observations lead us to interpretations involving the continents or relating in some way to their history or development. Clearly, we are now studying global


\(^{429}\) Ibid. 5.


One effort that would be strengthened in Phase 3 was an increased focus on "reconstructing the ancient oceanographic environment" in terms of surface currents, deep ocean circulation, calcium carbonate compensation depth and water temperature. The pre-existing network of sites enabled a comparative methodology which could render, Edgar hoped, "extremely rewarding" results.

In the two years that followed, the DSDP grew: it both came to broaden its scientific focus and enrolled more institutions from other nations. During the, as the DSDP leadership called it, International Phase of Ocean Drilling, the diplomatic potential of organized deep-sea core drilling became more emphasized. In 1974, both the P. P Shirsov Institute of Oceanology in the USSR and the Bundesanstalt für Bodenforchung in the Federal Republic of Germany joined JOIDES as international partners, and institutions from France, UK, and Japan joined shortly after. Whe Peterson presented the state of the DSDP for the NSF in 1975, the international interest was framed as a testament to the success of the project’s scientific methods rather than as a diplomatic tool. The DSDP had, according to Peterson, since 1968 become “one of the most important scientific efforts of its era”.

But in contrast to the presentations in 1971 and 1972, the importance of the DSDP was less related to its work on continental drift and the heroic task to follow in the footsteps of Captain Cook. In 1975, the work of DSDP was portrayed as being closely connected to resource prospecting, technology development and potential economic gains. Peterson pointed to the future, rather than the past, as the main motivation for the success of the DSDP. No other major scientific project was, he asserted, “so ‘down to earth’, in a manner of speaking, as the Deep-sea Drilling Project, and so directly related to the wise and future utilization as the only planet we, as humans, will ever call ‘home’.”

By invoking the figure of “only one Earth”, Peterson alluded to the discussions around global fragility that had taken a prominent position in political discourse after the 1972 UN Conference of the Human Environment, the Club of Rome report *Limits to Growth* from the same year and the 1973 oil crisis. The ocean’s potential as a space for economic and political life was further connected to the history of the DSDP:

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432 Ibid, 4.
433 The presentation in 1973 made many of the same arguments and, since the project was still in Phase 3, the research priorities also remained the same. I will therefore jump ahead to 1974 and 1975.
Six years ago, it became apparent from the successful work of the specially designed drilling vessel Glomar Challenger, that, by relatively modest technical extension, the deep offshore could be available for economic exploration and exploitation; today the nations of the world attempt to forge a new law of the sea. Six years ago, fully automated dynamic positioning was an exciting new technology; today it is a part of the conventional wisdom of offshore operators.\textsuperscript{437}

Deep-sea core drilling appeared less as a scientific and intellectual pursuit, and more as a technological and infrastructural project aimed at better accessing remote parts of the deep ocean. Even though the geological focus was still central, the 1975 review presentation emphasized the practical utility of deep-sea core drilling and the organizational capacities of the DSDP. Scoping energy futures was at the time becoming, in the words of Warde et al, “a minor industry, driven by business, national governments and the new International Energy Agency (IEA), set up in 1974 to manage allocations of oil among members of the OECD.”\textsuperscript{438}

The presentations from 1974 and 1975 also reveal a minor, but explicit, orientation towards paleoclimatology. In 1974, studying “rates of climatic change” was stated as one of the research goals for the first time and paleoclimatology and paleoceanography was given a more prominent role among the stated goals.\textsuperscript{439} In 1976, as a part of the internationalization and influx of personnel, the DSDP added a panel on the Ocean Paleoenvironment.\textsuperscript{440} Nevertheless, it was not a prioritized area, when geological history, technological development and resource prospecting took the center stage in the annual review presentations.

In the late 1970s, however, the DSDP was framed as more closely aligned with climate change research. At the 1978 review meeting, Malik Talwani, who became the director of LDGO after Maurice Ewing in 1973 and served as the chairman of JOIDES, presented the

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DSDP for the NSF. The DSDP was at this point a vast organization which had been operating for a decade, gradually adding members from the United States and abroad. Talwani expanded on the role the DSDP had played in the earth sciences during this decade and contrasted the early years of the project, when it sought to validate geological theories, with the problems it wanted to address in 1978. Geochemistry and “paleoenvironmental conditions” were two more recent subjects that had emerged more prominently after an archive of cores had already been established. “In a sense”, Talwani argued, “the science of paleoceanography is in its infancy, and drilling of key targets should lead to an enormous increase in our understanding of climate, environment, and the chemistry of the oceans in
the past.”\textsuperscript{441} One reason for bringing up the future prospects of paleoceanography was that the DSDP had not prioritized data collection for this purpose before, which had made this particular data less complete than data concerning for example the history of the ocean basins. In other to map more recent changes in planetary dynamics, there was a need for more extensive shallow drillings, which had not been a priority before.\textsuperscript{442}

In 1979 the role of climate change was even more emphasized. Melvin A. Peterson, who was once again presenting the research progress of the DSDP, mentioned the “understanding of climate and climatic change” in the third sentence of his introductory remarks. The goal of future drilling was to continue the work in solid earth geology but also “have an immediate impact on some major societal projects”. Peterson mentioned storage of nuclear waste in the seabed and earthquake prediction as possible utilities of deep-sea core drilling. He also brought up the assessment of natural variability of long-term climatic changes and, especially, how these assessments could be tested against mathematical models of future climate conditions.\textsuperscript{443}

The project was reaching its final years and would, during the course of its duration, produce nearly 18,000 deep-sea core samples from around the world. Paleoceanography took a more prominent role in the last years – out of the sixteen cruises that took place between 1979 and 1981, six concerned paleoceanography – and was more prominently placed in the review presentations for the NSF.\textsuperscript{444} After the DSDP ended in 1983 it was replaced in 1985 by the Ocean Drilling Program (ODP) which ran until 2004. The International Ocean Discovery Program, which is still running in 2023, is the latest iteration of the series of drilling efforts that began with the DSDP.\textsuperscript{445}

My aim in this section has not been to provide a full history of the DSDP. That would necessitate a far more comprehensive study. Rather, I have aimed to show how the temporal frameworks in which the DSDP were operating changed over time. The early years, with its orientation towards long, geological timescales and the validation of sea floor spreading and continental drift, enabled the language of the frontier to become entangled with the quest to “find” time stored in the ocean floor. But the DSDP had to navigate other temporalities as well: in the aftermath of the oil crisis in 1973, the leadership of the DSDP framed their activities as a search for a prosperous future through technology development and resource exploitation in the deep-sea. The timemaking of the DSDP was in this sense

\textsuperscript{442} Talwani, “Annual Program Review,” 11.
\textsuperscript{444} Melvin A. Peterson, “Annual Program Review: Deep-sea Drilling Project, 1981,” SIO, box 38, folder 6, 4-5.
both about material scientific practice at sea as well as a kind of timemaking rhetoric by the project’s leadership. By connecting the temporalities of the DSDP with societal issues they claimed their relevance, but also moved the deep-sea cores from a geological scientific framework into the scientific efforts to prognosticate the future.

The potential of using deep-sea cores to project future climatic conditions was present throughout the DSDP, but remained in the background until 1978, when climate change research – and particularly the synchronization of past ocean dynamics with computer modeling – was made a more explicit priority. However, deep-sea cores had been used for climate modeling purposes well before the DSDP began to fully embrace this line of research in the late 1970s. The cores derived from the DSDP were used for purposes beyond the scope of that particular project from the early 1970s and onwards. One such enterprise, which relied on the material labor of the DSDP but sought to use the cores for other forms of knowledge production, was the CLIMAP project

CLIMAP: Synchronizing the Paleoclimate (1971–1982)

According to James Hays, one of the founding members of CLIMAP (Climate: Long range Investigation, Mapping, and Prediction), the idea behind the project emerged while he was onboard the Glomar Challenger for the ninth leg of the DSDP. Looking at the immense material the drill was recovering from the ocean floor, Hays thought that it would be impossible for the crew onboard to fully analyze such a vast amount of samples.446 The DSDP’s ninth leg took place in December 1969 to January 1970, and sailed between Papeete in Tahiti to Balboa in Panama.447 When the crew returned home to the United States, they arrived in the midst of the preparations for the International Decade of Ocean Exploration. Back at LDGO, which was Hays’ home institution, Maurice Ewing was preparing a major proposal for the IDOE, which aimed to use the cores that had accumulated since Ewing began the drilling operations on the R/V Vema in the late 1940s. James Hays assembled a group, together with Ewing, in order to enroll the deep-sea core work at LDGO to the framework of the IDOE.

On August 3, 1970, the proposal entitled “The History and Development of the World Ocean” was submitted to the NSF.448 Hays had, just two months after he got off from

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the *Glomar Challenger* in Panama, checked the interest from the NSF in a historically oriented project, which would utilize both the core collection at LDGO and the cores that would become available throughout the decade via the DSDP. “Very large sums of money have been spent on gathering the cores”, Hays wrote to the NSF, pitching his idea, “it seems appropriate that some sort of organizational effort, even though a small one, be devoted to the long term research on these cores.”\(^{449}\) The project would not empirically study the ocean floor *in situ*, it would instead approach it as a scientific object being available in the storage facilities at LDGO and other oceanographic institutions. By aiming to make more qualitative studies of the cores, and apply a wider range of dating methods, detailed biostratigraphical knowledge could, Hays argued, be produced. This knowledge would be “fundamental to understanding natural fluctuations of the environment and will serve as a basis for assessing technological modifications of the environment.”\(^{450}\)

The NSF only partly approved of the proposal. They were more interested in the proposed work involving forams, paleoclimatology and paleomagnetism, rather than the longer geological timescales that was also part of the proposal. The NSF suggested that a group of scientists in the proposal – James Hays, Andy McIntyre and Neil Opdyke from LDGO and John Imbrie from Brown University – should join forces with two colleagues from the University of Oregon, Ross Heath and Ted Moore, who had similar research interests, and form a project of their own. The timescales this group of scientists were operating on, the NSF argued, was more relevant to the objectives of the IDOE and the need for the projects to be of use for mankind.\(^{451}\) The scientists decided to narrow down the temporal range of the project to 700,000 years, which is the time past since the last magnetic reversal of the Earth’s magnetic field. Paleomagnetism could be studied through deep-sea sediments, and therefore it marked a relevant temporal boundary.\(^{452}\)

The project was first named the “Paleo-Oceanography Study” but changed its name to CLIMAP when the IDOE formally started in 1971. It became part of the Environmental Forecasting section of the IDOE, which had as its aim to “provide the scientific base to improve environmental forecasting, which requires a repetition of observations, development of realistic (predictive) models, and understanding of physical principles.”\(^{453}\)


\(^{450}\)James Hays, Letter to Henry S. Francis Jr.


CLIMAP was in turn placed within this framework, by aiming to provide long term data over patterns of climatic fluctuations. By studying deep-sea cores already recovered, the project could trace “surface oceanic climatic fluctuations associated with glacial and interglacial transitions” and the aim was to produce four oceanographic maps that would show the conditions at different moments in geological history. The project also hoped to interpret their own results in close coordination with ice core research in Greenland and Antarctica, and thereby incorporate high-latitude glacial and interglacial climates and how they had affected ocean dynamics.

A key technology for the CLIMAP project would become oxygen isotope dating, especially relating to the $^{16}$O and $^{18}$O isotopes. Nicholas Shackleton, a British paleoclimatologist who had received his PhD 1967 at Cambridge with a dissertation entitled “The Measurement of Paleotemperatures in the Quaternary Era” joined the project shortly after it begun in 1971. Shackleton had built upon the work of Willi Dansgaard and Cesare Emiliani, who had spearheaded the stable isotope dating methods using $^{16}$O and $^{18}$O in the 1950s, but further refined their methodology. Dansgaard had established the correlation between $^{16}$O and $^{18}$O in relation to temperature by measuring rainwater: lower temperatures would give a smaller ratio of the heavier $^{18}$O to the lighter $^{16}$O in the water sample. He could thereby calculate the temperature of the atmosphere during the rainfall. When looking at ice core samples, Dansgaard could use C-14 dating on the ice to establish its age and then use his oxygen isotope dating technique to make assessments of the temperature from the time the ice first froze. Emiliani had developed

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454 CLIMAP was not the only project that sought to provide a baseline for climate and weather models. Two other major projects of the IDOE had similar ambitions, but operated on shorter timescales and with geochemical and geophysical data from the oceans themselves, rather than the ocean floor. The first was the Mid-Ocean Dynamics Experiment (MODE), which aimed to trace ocean currents and eddies, and brought to light how the oceans were, rather than being slow and sluggish, as dynamic as the atmosphere. The second was the Geochemical Ocean Sections Study (GEOSECS). It was the largest project within chemical oceanography during the IDOE and had as a primary objective to map and model the chemical composition of the world ocean, using isotope tracers to map rates of oceanic mixing and the generation and destruction of plant tissue. Jessica Lehman, “Sea Change: The World Ocean Circulation Experiments and the Productive Limits of Ocean Variability,” Science, Technology and Human Values 46:4 (2020): 839-862; Wallace Broecker and Tsung-Hung Peng, Tracers in the Sea (New York: Lamont-Doherty Geological Observatory Publications, 1982).

455 The four points in time were: 1. 6,000 years ago—the postglacial thermal maximum; 2. 17,000 years ago—the last glacial stage; 3. 120,000 years ago—the last interglacial period; and 4. 700,000 years ago—the mid-Pleistocene.” Office for the International Decade, Progress Report January 1970 to July 1972, 10.

456 Ibid, 10.


459 Dania Achermann, “Vertical Glaciology: The Second Discovery of the Third Dimension in Climate Research,” Centaurus 62:4 (2020): 727. The technique was primarily developed at the University of
a similar methods using the same $^{16}$O and $^{18}$O isotopes on fossils (marine planktonic foraminifera) found in the stratigraphy of deep-sea cores. But the foraminifera were difficult to date, and the timescales of Emiliani’s work in the 1950s would later turn out to be flawed, thereby also rendering his historical temperature data temporally inaccurate. As the paleoceanographer Frank Sirocko put it at the 50th anniversary of Emiliani’s landmark article: “The ages assigned by Emiliani to the warm and cold stages have changed, but Emiliani’s article was extremely influential in establishing the oscillating nature of past climate variations and its record in the geological archives of past climate change.”

Shackleton had in his Cambridge laboratory in the second half of the 1960s began developing a method that built on Emiliani’s work, but at the same time diverged from it. By studying a particular kind of foraminifera, benthic foraminifera, which can be found within the sediments on the ocean floor, Shackleton was able to get a stable referent in regards to temperature. Since the temperature in the very deep ocean is always stable and unaffected by temperature changes on the surface, these foraminifera would record changes in oxygen isotope content without being affected by surface temperature. Shackleton saw that oscillations in oxygen isotope content were visible in the temperature independent foraminifera and correlated strongly with the historical data on past ice ages. The cores could, in other words, tell a different story: they would not be primarily “paleo-thermometers”, as Emiliani had claimed, but instead records of the extent of past sea ice coverage. Shackleton argued that this would mean a complete reinterpretation of the stratigraphic record of the ocean floor: “It is simply necessary that every faunal or isotopic curve be re-read, taking ‘cold’ to mean ‘extensive continental glaciation’ and ‘warm’ to mean ‘glaciers reduced to their present level’.”

This method and way of reading the deep-sea core collections they had available proved instrumental for the CLIMAP project. The detailed data on the four latest ice ages could be translated into datasets which, in turn, could be relevant to the work of the climate modeling community. Three years into the project, in 1974, the engagement with the climate modelers grew more pronounced, as CLIMAP was orienting itself towards more recent climate history. By 1974, the project had grown larger, while at the same time

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462 John Imbrie met with a group of climate modellers, also affiliated with the NSF, which, according to James Hays made the CLIMAP members more aware of their proximity to the work of establishing more refined atmospheric general circulation models. Ronald E. Doel, Interview of James Hays on July 27, 1997.
beginning to question the vast timescale that the project proposal had set out to cover in 1970.

On June 25, 1974, a small group – among them were all the founding members of CLIMAP, although Nicholas Shackleton was not present – gathered at the University of Maine campus in Orono, in order to plan for the future of CLIMAP. Many of the key issues revolved around time: “How many climates are needed to yield satisfactory data base for climatic prediction? What are the problems in choosing and documenting these climates?” Finding a temporal framework that would work for both climate modelers and the CLIMAP members had emerged as one of the central issues for the project to solve. They also had to determine which kinds of climate data – “temperature, salinity, ice volume, sea level, land albedo, wind directions, vegetation, etc” – were the most important to this end. The long timescale to the last reversal of the Earth’s magnetic field was being put into question when the temporalities of the project were becoming increasingly future oriented. John Imbrie raised his concerns: “is it necessary to go back to 700 000 years to understand the history of climate – at least those climates that we expect to occur in the near future?”

One result of the focus on climate models was the CLIMAP sub-project “18 000 YBP Experiment”. It was led by Imbrie and sought to produce a map of global ocean surface temperature 18 000 years ago (the last glacial maximum). The project was seen both as a scientific opportunity to create better climate models, by using the detailed data on sea ice coverage made available through Shackleton’s core analysis, but also to better make CLIMAP adhere to the goals of IDOE. In drafting the outline for the fifth and sixth year of CLIMAP, the societal relevance and connection to other, less historically oriented, large-research projects was emphasized. The executive committee of CLIMAP especially wanted to connect their work to the ongoing Global Atmospheric Research Program (GARP), a large-scale program in atmospheric modeling. On the cover of the outline for year five and six, they put a quote by John Steinbeck: “and everything in the world must have design or the human mind rejects it. But in addition it must have purpose or the human conscience shies away from it.”

464 McIntyre, “Committee Structure.”
465 John Imbrie, “Some General Comments for Thought,” June 25, 1974, NSP, box 4. I have not been fully able to confirm that this document was written by Imbrie, since it is not signed, but it appears in the material of the Climate committee that was led by Imbrie, which contains other writings by him, and it would therefore appear as likely that he is the author behind this document.
467 CLIMAP Executive Committee, Draft: Outline of CLIMAP Proposal for Years V and VI.
Finding different ways in which the times of the deep-sea core data could align with more societally relevant times emerged as a way in which the purpose, as Steinbeck put it, could be defined. Given the division of different times into different scientific disciplines, the CLIMAP members tried to create fora in which different temporal frameworks could interact. One such example was an international meeting, which built on the data on the last glacial maximum. The full – and very detailed – title of the meeting was “International Meeting on Map Reconstruction of the Atmospheric and Ocean Circulation and Other Climatic Parameters at the Time of the Last Maximum of Glaciation About 17 000 years ago and Comparisons with Today’s Conditions and Those of the So Called Little Ice Age”.\footnote{CLIMAP Executive Committee, Meeting Programme, May 17 to May 22, 1973. NSP, box 4.} The meeting was held at the Climatic Research Unit at the University of East Anglia in the UK and Hubert H. Lamb was the convener on the British side, while James Hays organized the CLIMAP side.\footnote{I discuss the work of Lamb more elaborately in Chapter 4 of this thesis.} In trying to bridge the shorter timescales of Lamb’s historical climatology and contemporary climate modeling, CLIMAP researchers brought the times of deep-sea core data closer to the environmental, but chose to emphasize geomagnetic and geological time less.

A similar event took place during the Geological Society of America meeting in Miami, November 1974. Cesare Emiliani, Nicholas Shackleton and W. F. Libby hosted a symposium during this meeting with the title “Isotope Climatology”. The aim was to summarize the state of the field some 25 years after Emiliani and Libby had begun their work in isotope dating and climate.\footnote{Cesare Emiliani, Letter to John Imbrie, February 7, 1974. NSP, box 4.} The symposium was less temporally integrative in comparison to the meeting at the University of East Anglia the year before, but it still, in a similar way, showed how the work that began in the 1940s had transformed significantly. The topics brought together several kinds of records – deep-sea cores, ice cores, pollen – and the future was a temporal category that appeared throughout the program.\footnote{Cesare Emiliani and Nicholas Shackleton, “Program: Dialogue on Quaternary Problems,” November 19, 1974. NSP, box 4. Willi Dansgaard was one of the invitees, and presented on the topic “The isotopic record of ice cores”. Cesare Emiliani and Nicholas Shackleton, Letter to Willi Dansgaard, March 5, 1974. NSP, box 27.}

The symposium was followed, the year after, by a larger conference that built upon the symposium but involved in total 84 invited scientists. The “First Miami Conference on Isotope Climatology and Paleoclimatology” as it was called, had Libby and Emiliani as the main organizers. Many of the same scientists – among them Dansgaard, Imbrie, and Shackleton – were present this time around as well. The conference participants decided to suggest that a future International Decade of Isotope Climatology should be undertaken. It would be inspired by the IDOE and be a relevant scientific effort given the “pressing
problems of climatic change”. The imagined outcomes of a changing climate mirrored the fears of the time, primarily concerning population growth, starvation and energy use: “If energy is today’s crisis, food will be the crisis of tomorrow. Because the global food supply depends primarily on climate, current understanding of climate must be vastly improved in order to meet the challenge of tomorrow.”472

Food, and the dangers to agriculture and global food supply, appeared in other events organized by members of the CLIMAP project.473 George Kukla, a LDGO paleoclimatologist who had joined the project, arranged a session at the 1974 meeting for the American Association of the Advancement of Science with the title “To Feed the World: What to Do with Changing Climate”.474 The full day session involved presentations on lessons that could be drawn from the historical and geological record by John Imbrie, on the prospects of “stabilizing” the climate through human intervention by climate modelers William W. Kellogg and Stephen H. Schneider and reports on recent weather anomalies by representatives from NOAA. The arrangement of this particular session had political reasons as well: it was beneficial for CLIMAP to show that they were relevant in prognosticating different future issues, as they were a part of the Environmental Forecasting program of the IDOE.475

The synchronization of deep-sea core temporalities with the future of the world’s food production was, James Hays and John Imbrie thought, worthy of some reflection. It showed how much CLIMAP had expanded its scope in just a few years. They recalled one of their first meetings in 1970. Andy McIntyre had suggested to include a long-range goal of the project: “to cap our paleoclimatic efforts with a set of economic and agricultural impact statements”. He had claimed that in the future, they would need to enroll economists and agronomists in order to “translate our maps of ancient equilibrium into humanistic terms”.476 Hays and Imbrie considered how quickly things had changed:


474 James D. Hays and John Imbrie, Memorandum to the CLIMAP Executive Committee, undated 1974. NSP, box 4. Kukla became a minor celebrity in U.S. media for his dramatic prediction of an upcoming ice age, see: “Another Ice Age?” Time Magazine, June 24, 1974 issue.

475 James D. Hays and John Imbrie, Memorandum to the CLIMAP Executive Committee, undated 1974.

476 Ibid.
At the time this vision seemed to some a pipe-dream - and our friends at IDOE probably passed the idea off as a flight of proposal-writing fancy, not quite as wild as our plan to conduct experiments with global circulation models, perhaps, but still an idea of more rhetorical than scientific value. The over-all impact of Kukla’s symposium was to remove the smoke from this dream. If we want to do it, we can – not alone, of course, but as part of a broader effort, with modellers and others. 477

They confessed that they weren’t sure exactly what this meant for their scientific activities, but that new connections were being made between disciplines, practices and technologies that had previously been separated. “Do we really want to get involved?” They asked themselves, “And, if so, when and with whom? At what point would IDOE officials say we were going beyond their charter?” 478

The worries about going “beyond their charter” could also be understood as a discussion about time: what kind of temporalities, and what kind of futures, were actually being studied in the CLIMAP project? The idea to translate deep-sea core data into “humanistic terms” was appearing, in the minds of Hays and Imbrie, as a practically feasible way forward for their work. The times of deep-sea core drilling were moving from the metahistorical domain into the historical, from the natural background into the frameworks of politics and economics. 479 Even though the impact of this process at the time should not be overstated, it is a revealing example of how deep-sea core drilling was moving into new domains. For Hans Pettersson, when he set sail with the Albatross in the 1940s, this kind of synchronization of deep-sea core times and economic forecasting would have appeared strange, even incomprehensible. For Hays and Imbrie the idea to work with agronomists and economists appeared at first as a “pipe-dream” in the early 1970s, but gradually became both a scientific opportunity as well as a way to politically motivate the importance of deep-sea core drilling.

477 Ibid.
478 Ibid.
Figure 4.7. CLIMAP map of the surface of the Earth at the last glacial maximum. This map was one of the outcomes of the collaboration with the RAND corporation. CLIMAP Project Members, "The Surface of Ice Age Earth", 1132.
Figure 4.8. The Earth's oceans are running towards the end of the CLIMAP project, with the goal to arrive at one coherent understanding of the world ocean as a unified and synchronous scientific object. CLIMAP Executive Committee, "CLIMAP Newsletter, Winter 1979", NSP, box 24.
In 1976, CLIMAP published its major findings from the 18 000 YBP Experiment in the article “The Surface of Ice Age Earth” in *Science*. The approach to climate models was more pragmatic than in the memo quoted above: the article made use of General Circulation Models (GCMs) as well as paleoclimate data in order to create a simulation of the conditions at the last glacial maximum.\(^{480}\) The article marked a great achievement on behalf of the project as a whole as well as a landmark in the entanglement of climate modeling and paleoclimate data. Paleoclimatolgy was becoming fully – epistemologically and institutionally – enrolled to the larger project of producing global climate knowledge and prognosticating planetary futures.\(^{481}\) CLIMAP used the computer modeling capacities of the RAND Corporation in order to make their simulations, creating a visual and quantified version of the Earth at the last glacial maximum that was similar to the output from climate modelers at the time.\(^{482}\)

In the last years of the project, CLIMAP focused on the last interglacial, about 122 000 years ago, and published its final global reconstruction in 1984.\(^{483}\) Similarly to the DSDP, CLIMAP, when it ended in 1982, was transformed into a new project, which was named SPECMAP (Spectral Mapping Project). Over the course of the project, CLIMAP came to both expand and limit its temporal scope: the times of deep-sea core data were synchronized with the climate modeling community as well as political temporalities concerning the future of energy, climate and agriculture. When the project began, the future entanglement with climate modeling – and its subsequent translation into governance – was not taken as a given.\(^{484}\) Rather, the temporalities of CLIMAP gradually adapted to the needs of climate modelers and the members actively sought opportunities to translate, as they put it, “maps of ancient equilibrium into humanistic terms”. This imperative, which crystallized during the duration of CLIMAP, is situated in a very particular political setting. Behind the universalizing ambitions of the “humanistic”, the times of deep-sea cores were enrolled to a larger political project that was occupied with fears of overpopulation, energy shortages and food security. The many different metrics that deep-sea cores could cover – biodiversity, salinity, ocean circulation, and so forth – took a background role as global sea

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level became the focal point for CLIMAP scientists in their efforts to reconcile their work with the growing climate modeling community.

**Conclusion**

In 1960, when Rachel Carson asked Maurice Ewing for a picture of drilling scientists to use in the new edition of *The Sea Around Us*, deep-sea cores were emerging as one of the icons of the ocean floor. Following the Swedish *Albatross* expedition and the invention of the Kullenberg piston corer in the late 1940s, deep-sea core drilling became an institutionalized practice in the booming Cold War U.S. oceanography. This chapter has traced the temporal transformations of deep-sea core drilling from the time when Carson asked for her picture to the beginning of the 1980s. During these roughly two decades, the temporal, institutional and political frameworks of deep-sea core drilling underwent multiple changes.

During the 1950s deep-sea core drilling was a relatively small-scale operation, primarily organized around a few central characters such as Maurice Ewing, David B. Ericson or Cesare Emiliani. It was framed as a primarily temporally unidirectional enterprise, which sought to write a geological history of the ocean floor. The drilling activities were closely interwoven with the oil industries and later, with the project Mohole, large-scale Cold War science. The founding of JOIDES and the acquisition of the *Glomar Challenger* in the mid-1960s further drew together the scientific and the commercial, while at the same time fully institutionalized deep-sea core drilling as a part of the daily operations of major actors in U.S. oceanography. Time could be enrolled to the narrative of deep ocean frontiers, and the idea of heroic exploration to find a “new dimension” of time in the ocean floor was invoked by the oceanographers themselves.

The Deep-sea Drilling Project (DSDP) and Climate: Long range Investigation, Mapping, and Prediction (CLIMAP) emerged by 1970 as two main organizational vehicles for both drilling and analyzing deep-sea core data. The temporalities of these projects – and the way deep-sea core data were activated in relation to other scientific and political issues – varied over time. The DSDP fluctuated between a long geological history and the exploitation of resources for the immediate future, while simultaneously trying to motivate its relevance to environmental issues by the end of the 1970s. CLIMAP, which grew out of the DSDP and was a part of the IDOE, had a climate oriented framework from the outset, but also came to shorten its temporal scope to fit within the growing interest in modeling planetary futures. Time was produced both by the gathering and dating of deep-sea cores as well as through the timemaking rhetoric of the actors, who sought to connect the times of deep-sea core drilling with urgent matters in society. In this sense, the two projects
negotiated and tried to synchronize geological, environmental and political times before there was a coherent epistemological framework for such operations.⁴⁸⁵ These processes were products of their time: they were deeply embedded in visions of “rational utilization” of the world ocean, fears of overpopulation, starvation and lack of energy, and imaginaries of the ocean floor as a new frontier to conquer.

The history of oceanography, as Lino Camprubi has put it, can be viewed as “a history of an increasing political, technological and epistemological integration of the world oceans into the world ocean.”⁴⁸⁶ In this chapter, my aim has been to show how practices of timemaking shaped how the world ocean became a particular kind of environmental object in the 1970s. In the present discussions on the role of the oceans in the Anthropocene, both within and beyond oceanography, the temporalities of the oceans on a planetary scale often appear as naturally given, rather than as products of a longer history of planetary – and oceanic – timemaking.⁴⁸⁷

The chapter has also aimed to bring forward the cross-disciplinary work with time that was conducted by deep-sea core scientists together with climate modelers, ice core scientists and others, as the ramifications of anthropogenic climate change rose on the agenda. Oxygen isotope dating, with its sensitivity to temperature changes, became a particularly fruitful tool for the scientists who wanted to align the research interests of deep-sea core drilling and climate modeling. Temperature data could bring disparate fields, as well as past climates and future models, into conversation with one another. By drawing on Reinhart Koselleck’s notion of the metahistorical and the historical, I argue that this history can be seen as a process in which the boundaries between the natural pre-givens and human history were being re-negotiated. The establishment of deep-sea core “libraries”, which by the 1970s could be used to prognosticate future economic, agronomic and environmental conditions, drew the vast planetary histories of deep-sea core research into the domain of politics and governance. By paying closer attention to the actors, practices and institutions involved in this process, and how they shaped the temporal frameworks of deep-sea core drilling can make the historicity of planetary timemaking visible.

In the next chapter, I will continue to trace the interdisciplinary interest in paleoclimatological times and data. As the deep-sea core community was approaching climate modeling, similar developments were unfolding among ice core drilling researchers. The increased collaboration between and across disciplines would enable new

⁴⁸⁵ Sörlin and Isberg, “Synchronizing Earthly Timescales”.
temporalizations of the planetary-scale environment and the next chapter follows how ice core data – along with other kinds of paleoclimate data – entered Earth System modeling in the 1980s.
In 1975, ice made an appearance in a seemingly strange place. Wallace Broecker, a geochemist at Lamont-Doherty Geological Observatory, had barely engaged with ice or glaciological research previously in his career, but in his article “Climatic Change: Are We on the Brink of a Pronounced Global Warming?” ice served as the empirical foundation for his argument. \(^{488}\) The argument was not about ice either: it concerned global temperature and how it could be rising due to increased amounts of carbon dioxide in the atmosphere. Yet, Broecker relied on the work of Willi Dansgaard and ice core data from Camp Century, Greenland, in order to make his prognostication of the world’s future climate. \(^{489}\) Broecker’s famous article was not unique. By the mid-1970s, drawing on ice core data in order to prognosticate the effects of anthropogenic global warming was increasingly becoming a feature of the growing climate modeling community. \(^{490}\) Ice core data were travelling beyond the geographical, temporal and disciplinary limits of glaciological research.

As we saw in the last chapter, deep-sea cores were in the 1970s also expanding their scope and temporalities, as they became interwoven with efforts to model future climate conditions. Ice core research had a similar development and, as Broecker’s adaptation of ice core data is indicative of, the epistemic status of ice core data was increasingly removed from the geographies from which the data originated.

This chapter follows paleoclimate temperature data, and particularly data originating from the ice core drillings at Camp Century, from the first interactions between paleoclimatologists and climate modelers in the early 1970s to the institutionalization of


Earth System Science (ESS) in the mid-1980s. During this time, paleoclimate data moved further into the intellectual and scientific frameworks of ESS and came to realize particular temporal configurations. The integration of paleoclimatological knowledge in ESS also affected how different strands of paleoclimateology, such as ice- and deep-sea core research, related to one another.

Jennifer Patricia Barton has shown how the Earth System concept emerged somewhat haphazardly in the NASA (National Aeronautics and Space Agency) initiated committee called the Earth System Science Committee in the mid-1980s. Rather than being the natural outgrowth of increased planetary monitoring and enhanced technological means for computer modeling, the emergence of the concept of the Earth System relied on a significant sociological and intellectual component.491 As Francis Bretherton, the chairman of the Earth System Science Committee, put it in a 1986 interview in Science: “Many of the observations we need are already being made for other reasons, such as weather forecasting [...] it’s more an attitude of mind. We want to make sure that we go the extra mile – that we cover everything.”492

However, the Earth System concept was about more than merely assembling data. It was an endeavor fundamentally concerned with time and understanding how the Earth System’s “component parts and their interactions have evolved, how they function and how they may be expected to evolve on all timescales”.493 In other words, the Earth System concept did not just emerge in the mid-1980s, it was also temporalized. This chapter will focus on the latter, and explore how paleoclimate data from ice- and deep-sea cores made its way into the intellectual and conceptual framework of the NASA Earth System Science Committee.

Here, I draw on Reinhart Koselleck’s notion of the temporalization of concepts and how, according to Koselleck, “there is an entire socio-political vocabulary which refers to coefficients of movement and change. All socio-political concepts encounter a temporal tension which assigns the past and the future in a new way.” 494 Even though the Earth System concept transcends the boundaries of the “socio-political”, I argue that its strong anthropogenic component and political ramifications make the notion of temporalization applicable. Furthermore, Anders Ekström and Staffan Bergwik argue that temporalization

needs to be understood differently in the context of anthropogenic climate change, in contrast to the strictly socio-political usage of the concept by Koselleck. Temporalization becomes, in this new setting, “...connected with major recalibrations of scale, and the need to develop alternative ways of imagining and visualizing abstract and multilayered relations between societies and epochs that are separated and yet connected over large distances in time and space.”

The Earth System concept tries to imagine and visualize precisely such multilayered temporal relations. But this process of temporalization did not begin with the invention of the Earth System concept. By tracing the role of paleoclimate data – and ice core data from Greenland in particular – from the early 1970s to the end of the 1980s, the chapter sets out to explore how the temporalization of the Earth System concept built upon a negotiation about how different times – geophysical, paleoclimatological, biological and political – could, and should, be aligned. Rather than passively contributing to a cumulative growth of paleoclimate data, and progressively increasing knowledge about planetary pasts, the practices of synchronization taking place in the Earth System modeling community depended on a broader intellectual and political globalizing epistemology.

In present theoretical discussions concerning the relationship between human and natural timescales in the Anthropocene, the temporalities of ESS are as central as they are contested. Given the close connections between ESS and the Anthropocene concept, the framework stemming from the Earth System Science Committee echoes into the present. But as Deborah R. Coen and Fredrik Albritton Jonsson point out, current theoretical discussions as well as the historiography written by the Earth System scientists, tend to underemphasize the historical contingency of the Earth System concept itself. This chapter is informed by these discussions, and builds on the empirical historical scholarship on the origins of ESS, but seeks to further elaborate on how issues of time and


temporality were addressed in the formative phase of conceptualizing ESS. The Earth
System Science Committee’s implicit emphasis on global governance and technocratic
visions of detached planetary control – “environing from outside”, as Giulia Rispoli puts it
– placed the times of ESS into a particular political framework. Over time, the material
origins of these times – in ice, sediments, trees, pollen, and so on – faded out of view, and
became reified within the ESS machinery.

When Wallace Broecker drew on Dansgaard’s ice core data in 1975, these temporal
alignments were yet to be made self-evident. In the years leading up to the establishment
of the Earth System Science Committee, new disciplinary constellations were being formed,
which brought together scientists from glaciology, geophysics and oceanography, as well as
climate modeling and computer science. The deep pasts of ice- and deep-sea cores met the
planetary futures of climate models and, later, Earth System models.

“From a month to millennia”: Timing proxy records and climate models

In November 1972, a two-day workshop took place at Scripps Institution of Oceanography
in California. It was entitled “Climatic Changes on the Time Scales Ranging from a Month
to Millennia” and gathered leading scientists from oceanography, meteorology, glaciology,
solar physics, dendrochronology and climate modeling. The list of attendees was, as the
organizer and meteorologist Jerome Namias put it, “imposing”. It covered many of the
central figures of postwar Earth sciences: Maurice Ewing, William Nierenberg and Walter
Munk from oceanography, Willi Dansgaard and John Imbrie from paleoclimatology, Hans
Suess from physics, Jacob Bjerknes from meteorology, Edward N. Lorenz and Akira
Kasahara from climate modeling, and Melvin A. Peterson of the Deep-sea Drilling Project,
were all present at the workshop.

The American Meteorological Society’s Committee on Paleoclimatology was the
formal organizer, even though the event took place in Scripps’ facilities. The main purpose
of the meeting was to “provide a forum for exchanging ideas and information among
specialists.” The topics were broken down into the following groups: “(a) Theory of
Climate (modeling), (b) Evidence for past climatic changes (historical and geological), (c)

499 Erik M. Conway has shown that bringing together a wide set of disparate timescales – ranging from
geological and geomorphological, to oceanographic, meteorological and biochemical – was a key
challenge for Francis Bretheron as he was setting up the Earth System Science Committee. Erik M.
500 Giulia Rispoli, “Planetary environing: The biosphere and the Earth system,” in Johan Gärdebo and Adam
501 Jerome Namias, Letter to Maurice Ewing, April 13, 1972, MEP, Briscoe Center for American History,
University of Texas, Austin, Box 350-91, folder 275.
Empirical studies of mechanisms of climatic change (laying stress on ocean-ice-continent influences)." The presentations covered a wide set of topics, as well as a wide set of temporalities. Ice ages, rainfall cycles, solar climatic cycles, year-to-year variabilities, monthly and seasonal sea surface temperature anomalies and future climate prediction were among the topics discussed by the participants. Time was the organizing principle. A perceived need to reconcile temporalities and knowledges that up until this point had been too far apart appeared as the driving force behind the workshop.

All of the involved sub-disciplines had in the last decade seen a rapid development of their epistemological, temporal and political scope. After the successful drilling at Camp Century in 1966, ice cores had proven to be of interest for scientists far beyond the field of glaciology. Oceanography, as discussed in the previous chapter, was in the midst of a temporal expansion, with new technologies for sampling ocean water and the ocean floor and new oceanic timescales emerging. At the same time, climate modeling was entering, according to Matthias Heymann and Amy Dahan-Dalmedico, “a deep cultural shift” by turning towards the risks of anthropogenic global warming rather than focusing on producing an improved scientific understanding of the atmosphere.

This shift in climate modeling had been in the making since the end of World War II. Meteorology and climatology had become increasingly globalized and numerical weather prediction, which had developed rapidly during the war, was being further pushed forward by the rise of electronic computers and their enhanced capability of making large and rapid calculations. The burgeoning Cold War geopolitical order, and its emphasis on science and technology as strategically valuable assets, equipped scientists with ample resources and increased authority and expectations. At the same time, the Cold War came to shape a particular intellectual framework, a “Cold War rationality”, which was systems-oriented and held a strong belief that systems on different scales could be understood, predicted and...

502 Jerome Namias, Letter to Maurice Ewing, February 24, 1972, MEP, Briscoe Center for American History, University of Texas, Austin, Box 350-91, folder 275.
controlled. Numerical weather prediction fitted well in this setting and laid the groundwork for an applied, systems-based and global way of sensing and knowing the atmosphere and the Earth’s climate. By the early 1960s, particularly in the United States, calls began to appear for not only predicting atmospheric conditions, but to control them.

As the first General Circulation Models (GCMs) appeared in the 1960s, the timescales of weather prediction were growing as well. The GCMs were developed to quantify atmospheric processes on monthly or seasonal timescales, not just a few hours or days ahead. As the temporal scope of prediction increased, another kind of time was coming into view as well: the time of anthropogenic impact on the climate system. The boundary between the climatological and the political was dissolving, but, as Mike Hulme argues, this particular scientific organization of climate knowledge production also led to a “hegemony exerted by the predictive natural sciences over human attempts to understand the unfolding future”.

In 1971, one year before the workshop at Scripps, the report *Study of Man’s Impact on Climate (SMIC)* was released by a group of scientists in order to push climate change onto the agenda of the UN Stockholm Conference on the Human Environment, set to take place in 1972. William W. Kellogg, an American meteorologist and the chief organizer of SMIC, argued in the report that the “the haunting realization” that humanity could alter Earth’s climate had become “one of the most important questions of our time”.

Taking on the question of “Climatic Changes on the Time Scales Ranging from a Month to Millennia” was, in 1972, an endeavor with temporal and political qualities that would have been inconceivable just ten years before. The well-studied transformation of climate modeling around this time – its politicization, computerization and global scale – is one central feature, but not the only one. The incorporation of paleoclimatological data, and the vast increase in temporal scope it entailed, was also an important factor behind the subsequent rise of Earth System modeling in the decades to follow. Even though the

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paleoclimatological scientific community was not directly engaged with, for example, the Stockholm Conference in 1972, their data and temporal outlook began making its way into climate prediction and early climate politics.\footnote{In addition to Broecker, 1975, who used Dansgaard's Camp Century data, another example is the usage of CLIMAP's deep-sea core data in the, at the time, controversial and influential 1981 article "Climate Impact of Increasing Atmospheric Carbon Dioxide". See: James Hansen, D. Johnson, A. Lacis, et al. "Climate Impact of Increasing Atmospheric Carbon Dioxide," \textit{Science} 213:4511 (1981): 959–966.}

In the SMIC report, paleoclimatology was sparsely represented. Hubert H. Lamb, a British meteorologist and early champion of using historical climate data, was the only authority cited on pre-industrial climate in the report.\footnote{Given Lamb's skepticism of numerical modeling and physics-dominated ideas of climate, this might appear as somewhat surprising. Lamb was known to use multiple, but non-geophysical, forms of data, such as medieval almanacs and ship logs, and favor locally situated knowledge over global simulations. In the report, however, the references to Lamb are made in passing and his data are not further deliberated on. Pre-instrumental climate records are not central to the arguments of the report, which would explain why Lamb, despite his aversion for modeling, is quoted. For more on Lamb's role in climate science at the time, see: Janet-Martin Nielsen, "Ways of Knowing Climate: Hubert H. Lamb and Climate Research in the UK," \textit{Wiley Interdisciplinary Reviews: Climate Change} 6:5 (2015): 465–477; Elliot Honeybun-Arnold, \textit{Scientising the 'environment': The School of Environmental Sciences, University of East Anglia, 1967–1990} (Ph. D diss., Norwich: University of East Anglia, 2022).} There were no references to either ice cores or deep-sea cores. The field of historical climatology – using documentary evidence as proxy data for climate reconstruction – was at this time in its infancy and was, with the exception of Lamb, not represented either. Astrid Ogilvie has called this period a “renaissance” for historical climatology and pointed to the surging interest in the field in the early 1970s. Yet, it was at the time not as evident were the new historically oriented climate data belonged.\footnote{Astrid Ogilvie, "Historical climatology, \textit{Climatic Change}, and implications for climate science in the twenty-first century," \textit{Climatic Change} 100 (2010): 33–47.} As the concept of climate was becoming increasingly dynamic and political, its temporal boundaries and qualities were in flux. The synchronization of proxy records and computerization of climate modeling were beginning to take shape. The Scripps 1972 workshop is an early example of this, but also shows that it was yet to become formalized in a coherent scientific framework.

During the decade that followed the workshop at Scripps, paleoclimatological data entered the climate change debates, as well as new attempts at even more advanced modeling. In the next section, I will focus on the ice core data from Camp Century and trace how this data became picked up in contexts well beyond the boundaries of ice core science and glaciology.

\textbf{Frozen archives on the go: Ice core data in 1970s climate change discourse}

When the surface-to-bedrock drilling took place in 1966 at Camp Century, a U.S Army Base in north-west Greenland, it marked the end of a technological struggle to reach all the
way through the ice sheet. But it also marked the beginning of a complex infrastructural project of gathering, storing, circulating and quantifying ice cores and ice core data. As the operations in Greenland came to an end, Willi Dansgaard, now back home in Copenhagen, wanted access to his ice cores. They were scattered around multiple research institutions and most of them were stored in Buffalo, NY, at the CRREL (Cold Regions Research and Engineering Laboratory) where Dansgaard’s collaborator at Camp Century, Chester C. Langway Jr., was in charge of the ice core samples. In 1968, Dansgaard was able to get some smaller samples sent to him and his Copenhagen colleague Jorgen Møller travelled to CRREL shortly after and brought back some 1000 samples.

The vertical and portable form of the ice core samples enabled new temporal conceptualizations of ice, but they also enabled a new kind of epistemic geography of the cryosphere. In contrast to the field-based, observational approach favored by glaciologist in the first half of the twentieth century, the material properties of the ice core decoupled the ice from the geographical area from which it originated. In places like Copenhagen and Buffalo, the Greenland ice sheet was materially accessible in large ice core storage facilities, which served as proxies for entire polar environments. Exactly what the ice cores, ordered in neat rows in their freezers and handled by men in lab coats rather than men in down jackets, were proxies for was still somewhat up in the air. Ice core data seemed to be able to reach beyond the glaciological community, speaking to a planetary past rather than glacial developments in Greenland.

In one of the first major publications on the Camp Century ice core, appearing in *Science* in 1969, Willi Dansgaard, Sigfús J. Johnsen, Jorgen Møller and Chester C. Langway Jr. presented a “time scale” that the ice core had made possible. Ice was presented as a...
deep time climate proxy and the visualization of the ice core timescale had thereby entered the visual language of geological stratigraphy as well as the history of timelines itself. Presenting ice as a timescale that stretched into geologic time was a way to transcend the disciplinary reach of glaciology, even though Dansgaard and his colleagues still expressed some cautiousness regarding the planetary reach of their results: “[…] although the complete O\textsuperscript{18} curve is primarily valid for the North Greenland area, the general trend of the curve agrees with known and reported climatic changes in other parts of the world, at least in the course of the last 75,000 years.” They also highlighted the temporal frequency of ice core data – which would later prove to be one of its greatest assets: “It appears that ice-core data provide far greater, and more direct, climatological detail than any hitherto known method.”

Shortly after the ice cores had expanded the temporal boundaries into the deep past, Dansgaard and other ice core scientists were increasingly looking in another temporal direction: the future. In a 1972 article in Quaternary Research, Dansgaard, Sígríður J. Johnsen, Chester. C Langway Jr., and Henrik B. Clausen, used their results from Camp Century to “speculate” about future glaciations. The article raised concern regarding the risk for a new, rapidly developing glaciation and asked whether human impact could be a force comparable with geophysical phenomena such as ice surges or intense volcanic activity. Even though the dataset was the same – relying on the Camp Century ice core – the authors were no longer as cautious to let the ice core speak for the entire planet. The data moved from being used to archive Arctic pasts to project planetary futures, connecting ice with the emergent scientific study of the future.

The ramifications of this quick epistemological expansion – from Greenland to the planet, from the past to the future – was not lost on Dansgaard himself. In a 1973 entry in the Danish scientific periodical Meddelelser om Grønland (Communications on


Ibid., 380.

Although, it should be noted, ice core data did not reach as far back in time as deep sea core data. Christoph Rosol, “Data, Models and Earth History in Deep Convolution: Paleoclimate Simulations and their Epistemological Unrest,” Berichte zur Wissenschaftsgeschichte, 40:2 (2017): 120-139.


Greenland), Dansgaard and two other colleagues from the University of Copenhagen, asserted that: “The development of ice core drilling technique has led to a broad variety of studies reaching far beyond glaciology itself.”  

In the article, they listed a wide range of scientific disciplines to engage with, ranging from meteorology and climatology to solar physics and atmospheric chemistry. Lorraine Daston identifies the work of making data commensurable as one key role of a scientific archive, as “…methods, instruments, records and observers must be calibrated across polities, epochs and genres”. When turning ice into ice core data, this process of translation became possible across a wider set of fields than had been possible before. The shorter distance between ice core drilling and other disciplines, particularly those dealing with human impact on the planet, also came with political implications: were humans interfering with the planetary dynamics visible in the ice cores?

An indicative example of this temporal and political shift in ice core research is visible in a 1975 article in Nature, entitled “Climatic Changes, Norsemen and Modern Man”. Dansgaard was the lead author of the article, which was co-written with colleagues from the Geophysical Isotope Laboratory in Copenhagen. By using ice core data from Greenland, the authors argued that they could track the climatic changes that caused the demise of the Norse settlement on Greenland in the 15th century. Additionally, they went on to connect the climatic changes that they thought caused the settlement to collapse with present day environmental problems. Climate change appear as an exterior force, which can hit societies at different points in time. This way of conceptualizing drastic climatic shifts is common in ice core futurities, and tends to obscure the role of politics and societal organization while giving primacy to climate as an exterior force. But the article also highlights the increasingly complicated epistemologies and temporalities of ice core research at the time. By connecting ice cores to human pasts – the Greenland settlement – and human futures – anthropogenic climate change – Dansgaard and his co-writers were simultaneously expanding the political, temporal and epistemological boundaries of their research.

531 It should be noted that Dansgaard was not the first to ascribe climatic fluctuations as the cause of the settlement’s demise. However, he was to first to utilize ice core data to make this case. See for example Gustaf Utterström, “Climatic Fluctuations and Population Problems in Early Modern History,” Scandinavian Economic History Review 1:3 (1955): 3-47.
The article made a particular appeal to climate modelers: ice cores could be used in “diagnosing the processes that cause climatic changes, and for checking the validity of the models on all time scales between 10 and $10^6$ yr”.533 By this time, Dansgaard had become a vocal proponent for numerical climate modeling and was eager to bring ice core data into the work of creating, as Dansgaard put it in a 1976 lecture at Cambridge, “a general circulation model comprising all climatic parameters and their extremely complicated interplay.”534 He was also becoming a public voice on the dangers of climate change, arguing that the long-term damage of fossil fuel use should be brought forward in the Danish debates on nuclear energy.535 He was constructing a new scientific and public persona, which allowed him to speak with authority in new areas, much like his ice cores.536 The case of the Norse settlement on Greenland – and how ice core data could explain its demise – was invoked by Dansgaard in more settings than the *Nature* article and with even more dramatic framings. When he received the prestigious Vega medal in 1975, awarded by the Swedish Society for Anthropology and Geography, he compared the risk of future warming with that of nuclear armament. The Greenland settlement was invoked as a cautionary tale of rapid climate shifts.537

Even though Dansgaard himself, was trying to expand his epistemic authority to reach beyond the ice sheet, he did not appear as particularly interested in other kinds of proxy records. He did reference the work of deep-sea core researchers – he had, after all, known Cesare Emiliani since the 1950s – but did not engage with the deep-sea cores themselves.538

James Hays, one of the founding members of CLIMAP, recalled that he tried to recruit Dansgaard to the project, and thereby expand the empirical underpinnings of the project

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537 Willi Dansgaard, “Klimatsvångningar och menneskoeden [sic],” Speech held at the Vega Medal award ceremony, April 24, 1975. Willi Dansgaard Papers, CUL, Box 4. In addition to the Vega Medal, Dansgaard also received the Seligman Crystal by the International Glaciological Society in 1976, for having opened “a new page of glaciology”. From these relatively few public appearances it is hard to pin down a coherent ideological reasoning behind Dansgaard’s interventions. Rather, I bring them forward to show how the epistemic authority of ice core drilling also enabled Dansgaard to speak to issues relating to energy, environmental degradation and future planetary conditions.
538 The work of Emiliani and Wallace Broecker is for example referenced in Dansgaard et al., “One Thousand Centuries of Climatic Record.”
to also include ice cores, but Dansgaard turned CLIMAP down. Hays interpretation was that Dansgaard was hesitant to work with other materials than ice.\(^{539}\)

If ice by the 1960s was increasingly conceptualized as an archive, a new kind of temporal imaginary began appearing towards the end of the 1970s: ice as a time machine. The perceived temporal unidirectedness of the archive was turning towards a temporal framework that expanded into the future, and ice cores could serve as proxies for both past and future climates. This is particularly evident in ice core scientist Richard B. Alley’s monography on his work in Antarctica in the 1990s, *The Two-Mile Time Machine: Ice Cores, Abrupt Climate Change and Our Future* (2000), but as Dansgaard’s activities in the mid-1970s show, this idea of ice cores as mediators of the future had by 2000 already existed for decades.\(^{540}\)

It was not only ice core scientists such as Dansgaard who found the Camp Century ice core data useful. Much like Dansgaard had speculated in 1973, his ice core data made its way into a broad range of scientific disciplines that did not directly engage with ice. In the 1970s, data from the Camp Century ice core could be found in articles in geo- and solar physics, meteorology, climatology, oceanography and geology.\(^{541}\)

But it was particularly in matters concerning planetary-scale environmental knowledge that ice cores began to appear more frequently during the second half of the 1970s. In addition to the previously mentioned article on global warming by Wallace Broecker, Camp Century ice core data also showed up in a 1975 report by the National Academy of Sciences entitled “Understanding Climatic Change: A Program for Action”. The report was written by a committee from the Global Atmospheric Research Program (GARP) and the members came from different geophysical, climatological, mathematical and oceanographic institutions. Among its members were Broecker, but also the

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mathematician Francis Bretherton, who would later become the chairman of the Earth System Science Committee, oceanographer Henry Stommel and atmospheric scientist Joseph Smagorinsky.\(^{542}\)

The Camp Century ice core was in this context one of many proxy archives listed by the committee as possible ways of tracking past climatic changes. The authors stated that “Proxy data come from a wide variety of sources; potentially, any biological, chemical, or physical characteristic that responds to climate may provide proxy data useful in the reconstruction of past climates.”\(^{543}\) In comparison to the 1971 SMIC report, paleoclimatology had a much more pronounced position. Ice as a scientific object was defined by its utility in relation to other proxy archives and its contribution to planetary-scale environmental knowledge. The cryosphere in its entirety was perceived as a mechanism in the planetary system rather than an environment in itself. Dansgaard’s work appeared in “Understanding Climatic Change” in a setting that would have been impossible for earlier, field-based research on glaciers and polar environments. He was referenced not along with other glaciologists, but with meteorologists such as William W. Kellogg, palynologist Margaret Bryan Davis, and paleoceanographer John Imbrie.\(^{544}\)

This way of gathering data from a wide array of geographies, materialities and temporalities in order to produce a coherent perception of Earth System dynamics would become commonplace with the institutionalization of Earth System Science in the 1980s, but could also be seen as a continuation of an emergent “vertical glaciology”.\(^{545}\) The verticality of the ice core enabled it to travel beyond glaciology and to translate layers of ice and snow into datasets recording changes over time. Similar enterprises in other fields, such as oceanography, dendrochronology, and palynology, conducted in a similar vertical and stratigraphic manner, rendered a synchronization between different materialities and geographies possible.\(^{546}\) But synchronizing these different climate proxy records – and the sometimes clashing temporal sensibilities of different scientific fields – was not an easy task.\(^{547}\) As Hubert H. Lamb’s skepticism of modeling the future global climate indicates,


\(^{543}\) United States Committee for the Global Atmospheric Research Program, Understanding Climate Change.

\(^{544}\) Ibid.

\(^{545}\) Achermann, “Vertical Glaciology.”


\(^{547}\) As Edelstein et al. puts it: “[...] power and time interface amid intensely competitive temporal formations, and not simply parallel or layered ones. This interface braces these temporal formations and their conflict, sometimes enforcing a particular temporal hierarchy, at others submitting to their
everyone involved with paleoclimatology was not as optimistic as Dansgaard when it came to the prospects of climate models. With its annual temperature data and vast timescales, ice core data were easier to utilize within the global, quantified and temperature oriented “culture of prediction” emerging within the climate modeling community in the 1970s.\(^{548}\)

Seen in this light, the 1972 workshop at Scripps and the subsequent efforts to reconcile the multiple, and occasionally clashing, climate temporalities were laying the groundwork for the more advanced Earth System modeling in the 1980s.

**Earth System modeling and climate proxy records**

Ten years after the 1972 workshop at Scripps a group of scientists – geophysicists, oceanographers, glaciologists, and geochemists – gathered at the Lamont-Doherty Geological Observatory outside New York for the fourth biennial Maurice Ewing Symposium. It was a similar kind of event: the participants came from the same scientific disciplines, it took place at another elite institution within the U.S. geosciences and the main scientific problems concerned matters of time. The theme of the symposium was “Climate Processes and Climate Sensitivity” and the objective was to bring “together the work of modellers and others involved in the analysis of observational data. The approach considered climate variations and climate processes (focusing on climate feedback processes) on a broad range of timescales.”\(^{549}\) Even though the volume which comprise the papers presented at the symposium was divided into different areas, such as atmosphere, cryosphere and ocean, the editors James Hansen and Taro Takashi noted that these areas are bound together through feedback processes and thereby “overlap extensively”:\(^{550}\) Willi Dansgaard, together with his Swiss colleague Hans Oeschger, were part of the participant list.

But despite the many similarities with the Scripps gathering ten years before, everything was not the same. The societal impact of a changing climate system had become a key incentive for hosting the symposium. It was “another feedback process”, which
consisted of the “progressive understanding of the earth’s climate and possible societal adaptation”.\textsuperscript{551} The emphasis on societal effects had also brought in other actors interested in the scientific study of climate processes. E. E. David Jr., president of the Exxon Research and Engineering Company, a subsidiary of the giant oil corporation Exxon Mobil, was invited to speak on societal adaption. Exxon also funded the event. Under the headline “Inventing the Future: Energy and the CO\textsubscript{2} ‘Greenhouse’ Effect”, David JR. outlined a vision for the future, which emphasized market driven solutions and a plurality of energy sources, while simultaneously deeming energy futures without a substantial part fossil fuels difficult to reconcile with the drive for economic growth.\textsuperscript{552} David JR. was critical of centrally planned governance schemes and rather hoped for self-correcting mechanisms of the “world economy”:

To sum up, the world’s best hope for inventing an acceptable energy transition is one that favors multiple technical approaches subject to correction – feedback from markets, societies, and politics, and scientific feedback about external costs to health and the environment. This approach is not easy, or comforting to the uninitiated, because there is no overall neat and understandable plan. But prophecies leading to masterminded solutions that commit a society unalterably to a single course are likely to be dangerous and futile. A good sign is that, without any central plan, the world economy has already adopted conservation technologies that are reducing the rate of CO\textsubscript{2} buildup.\textsuperscript{553}

The concept of the feedback mechanism, and the perceived self-regulatory qualities of the planet as well as the market driven global economy, has been popular within fossil fuel industries, as Leah Aronowsky has shown.\textsuperscript{554} By conceiving of planetary dynamics as self-regulating it became possible to refer to the resilience of these self-regulatory processes as a reason to not decrease usage of fossil fuels. The planetary system would, the logic went, absorb excess carbon dioxide in order to maintain its stability. In a similar fashion, the economy would also self-regulate in order to adapt to the needs of a less fossil fuel intensive mode of production.\textsuperscript{555} The role of large oil corporations in the production of climate

\textsuperscript{551} Ibid.
\textsuperscript{553} E. E. David JR., “Inventing the future,” 5.
\textsuperscript{555} This view of climate governance was not unique to David JR., but can rather be seen as an indicative example of neoliberal approach to the global environment. See: Noel Castree, “Neoliberalising nature: the logics of deregulation and reregulation,” Environment and Planning A 40 (2008): 131-152.
knowledge is well covered by historians and Exxon’s ambition to fund the 1982 symposium is not an anomaly at this time. But the interactions between paleoclimatologists, climate modellers and representatives of the fossil fuel industry at the symposium point to a more intertwined relationship than popular conceptions of fossil fuel climate research often allows for. Academic research and fossil fuel industry research on climate change was not always two separate enterprises and sometimes shared epistemological assumptions about climate change. The scientists themselves were engaging with each other across institutional boundaries.

After David JR. had presented his ideas on the future, the rest of the symposium concerned more familiar territory for the participants. Dansgaard – together with Oeschger and his co-workers at the University of Bern and Chester. C Langway Jr. from the University of Buffalo, who worked with Dansgaard at Camp Century – presented a “Late Glacial Climate History from Ice Cores”. They drew on resent drillings in Greenland, but they were also incorporating data from lake carbonate samples from Central Europe, in order to establish temperature patterns beyond the cryosphere. By using the same oxygen isotope dating method on both the ice and the carbonates, they were able to draw together two geographically separated environments into one coherent timescale.

The vertical outlook and planetary perspective made new connections possible.

Even though Willi Dansgaard and E. E. David JR. would have been an odd match in the early 1970s, their appearance in the same program in 1982 was less surprising. By the late 1970s and early 1980s, it was not only the participants in the Maurice Ewing Symposium who were beginning to draw together societal and geophysical futures in more elaborate ways. In 1979, the World Meteorological Organization (WMO) held its World Climate Conference, which was “a conference of experts of climate and mankind” that

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558 Jenny Andersson, has for example, showed how the scenario tool was developed by Royal Dutch Shell, even though it came to influence future prognostication beyond that particular company. Jenny Andersson, “Ghost in a Shell: The Scenario Tool and the World Making of Royal Dutch Shell,” Business History Review 94 (2020): 729-751.


560 For an overview of these developments in the 1970s, see Emil Flate, When Science Could Not Wait: Climate, Experts, and the Times of Anthropogenic Change, 1945–1979 (Ph. D diss., Oslo: University of Oslo, 2023)
marked the founding of the World Climate Programme.\textsuperscript{561} Similar attempts to gather a wide set of researchers in order to produce “useful” climate knowledge in light of the prospects of a rapidly warming world were happening in other settings around this time as well.\textsuperscript{562} William W. Kellogg, one of the main authors of the SMIC report, summarized epistemological and political shift anthropogenic climate change meant to climate science in 1980: “This can no longer be considered an academic exercise, because along with the


realization that we can influence climate comes a plethora of questions that have never been raised before in the course of the long history of civilized man.”

By the early 1980s, this “plethora of questions”, needed institutional organization. Computer modeling and satellite monitoring enabled increased institutional capacities to empirically study the planet as one interconnected system. Given the enormous scale of this object of study, it also transcended previous large-scale projects on planetary dynamics. GARP had, for example, focused exclusively on the atmosphere, but by the early 1980s, a similar approach was to be conducted on all component parts of the planet.

In the United States, the shift towards a holistic and comprehensive study of planetary dynamics was particularly visible. NOAA (National Oceanic and Atmospheric Administration) and NSF (National Science Foundation) had traditionally been the main state actors in administering and funding climate research, but following the U.S Congress 1977 National Climate Program Act they were also joined by NASA in keeping track of the future of the planet. NASA’s expertise in remote sensing and planetary perspective appeared as relevant in a setting that was new for the space agency: monitoring Earth itself. Eric Conway particularly highlights the impact Dansgaard’s work at Camp Century as well as the CLIMAP project had on conceptions of planetary change at NASA and how these studies rendered planetary stability a much more fragile and contingent phenomenon than had previously been the case.

In 1982, NASA proposed to launch the Global Habitability research program. It was the first major effort from NASA to combine their planetary-scale approaches with the interconnected component parts of the Earth. Notably, this program did not use the Earth System concept, but rather focused on the notion of “global habitat”. The proposed program had a strong international focus, but still failed to consider the geopolitical ramifications of satellite imagery and climate data. Jenifer Patricia Barton argues that the U.S. centered program design was not attentive enough to the global context, especially the concerns of the so called Global South, and therefore had trouble acquiring the support of the international community. Where the U.S. scientists behind Global Habitability saw a

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566 Ibid, 199.
project “for the benefit of mankind”, many other nations saw a political tool for U.S. control of global-scale datasets.\textsuperscript{571}

The year after the failed launch of Global Habitability, a subcommittee of the NASA Advisory Council (NAC) was founded and given the name Earth System Science Committee (ESSC). The ESSC would be active from 1983 to 1988 and become foundational in the establishment and conceptualization of Earth System Science as well as the making of the Earth into a singular scientific object with distinct systemic abilities and properties.\textsuperscript{572} The committee's aim was to promote a somewhat tweaked version of the Global Habitability program, but maintain the aim to launch a large-scale Earth science program led by NAC. The more malleable Earth System concept proved politically useful, while the geopolitical tensions around satellite data were decreasing in the mid-1980s.\textsuperscript{573}

The conceptual framework of the ESSC revolved around time. Given the many components of the Earth System, and their various and intersecting rhythms, speeds and durations, the framework needed to account for vastly divergent timescales. The final report of the ESSC, \textit{Earth System Science: A Program for Global Change: A Closer View} from 1988 put it bluntly: “A deeper understanding of the processes responsible for the evolution of the Earth on all timescales is the ultimate goal of Earth System science.”\textsuperscript{574} As the program came to a close in 1988, the ESSC had divided the times of the Earth System into three main categories: short-term processes ranging from seconds to years (such as global weather systems, volcanic eruptions, atmospheric turbulence, and seasonal vegetational cycles), medium range processes ranging from decades to centuries (such as climate change, variation in carbon dioxide, soil erosion, and ocean circulation) and long-

\textsuperscript{571} Ibid, 134-135. For an overview of data politics and geopolitics during the Cold War, see Peder Roberts and Simone Turchetti (eds.), \textit{The Surveillance Imperative: Geosciences during the Cold War and Beyond} (New York: Palgrave Macmillan, 2014).


Figure 5.2. Chart of the timescales of the Earth System. Earth System Science Committee, Earth System Science: A Program for Global Change, 28.
term processes ranging from millennia to thousands of millions of years (such as atmospheric composition, mantle convection, plate tectonics, soil developments and glacial periods). The final report also made clear that it was the medium-range that, for political reasons, was the most relevant timescale. As the report stated: “It is on timescales of decades to centuries that natural change has major effects on humanity, and that the effects of human activity on global processes are most pronounced.”

But in 1983, when the ESSC began its work, the temporal properties of the Earth System were less settled. They had to be negotiated, synchronized and assembled in ways that were not pre-determined when the program started. In the remainder of this chapter, I will follow the temporal negotiations within the committee and draw on the internal documents of the ESSC.

**Planet Dynos: Modelled futures and clashing temporalities in the ESSC**

The ESSC was led by the Cambridge trained mathematician Francis Bretherton. Born in Oxford in 1935, Bretherton had previously worked models of ocean circulation and ocean-atmosphere interactions. He was also part of GARP in the 1970s, but had his main focus on theoretical models rather than empirical studies of the oceans or the atmosphere. Based in Boulder, Colorado, Bretherton served as the president of UCAR (University Corporation for Atmospheric Research) and director of its subsidiary institution NCAR (National Center for Atmospheric Research) from 1973 to 1980, before stepping down to focus on research. He was known to work interdisciplinary and as a leader who had a “big picture” approach. The all-male committee consisted of 15 members, representing NASA as well as a broad range of fields, including atmospheric chemistry, geophysics, ecology, oceanography, and solar physics. This smaller group served a coordinating role and divided the work up into working groups, which in turn involved a much greater number of

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573 Ibid, 29.


scientists. When the ESSC released its final report in 1988, 239 individual scientists had contributed with their expertise.\textsuperscript{580}

The ESSC used the urgency of understanding planetary dynamics as a motivation for their existence. They emphasized a strong and optimistic belief in accurately predicting the future of the planet while simultaneously not requiring excessive amounts of funding. Much of the data, research and scientific infrastructure were, the argument went, already in place. In his internal presentations, Bretherton combined a sense of existential urgency with visionary ideas of modeling the future of the world (figure 5.3).

In the inner workings of ESSC, paleoclimatology did not have a self-evident representation. There was no designated working group for paleoclimatology in the ESSC, but given the multi-temporal nature of the whole enterprise, questions of time and the usage of climate proxy record data could be found in many different parts of the work of the ESSC.\textsuperscript{581} Managing temporal friction between different working groups, as well as within working groups was a key task and a difficult challenge.

The temporalities of the Cryosphere Working Group is one example. It was led by the glaciologist Wilford Frank Weeks from the Cold Regions Research and Engineering Laboratory (CRREL), who worked with remote sensing of sea ice and glaciers.\textsuperscript{582} The point of departure for the group was the NASA affiliation and the short-term changes that could be expected in the upcoming decade.\textsuperscript{583} Ice core data, or any ice core drilling related activities or temporalities, were not part of the Cryosphere working group. Ice cores had by this time become fully immersed into the planetary scale, being of relevance to the Systems modeling working group rather than the one designated to the cryosphere. The research approach of Weeks and his colleagues were in this sense more similar to the field-based methods of early twentieth century glaciology than the planetary-scale and long-term perspectives of ice core drilling in the postwar decades. However, their interest in ice and snow was motivated by how these scientific objects could be understood in relation to other

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\textsuperscript{580} Earth System Science Committee, Earth System Science: A Program for Global Change: A Closer View.

\textsuperscript{581} The working groups were somewhat loose, and often had overlapping memberships, but the proposed groups as the ESSC was reaching its full working capacity in 1984 were: Systems modeling, Tropospheric Sounding and Imaging, Cryosphere, Precipitation and Wind, Basin Scale Land and Biogeochemical Processes, Geology, Geophysics, Systems Integration, Interagency, Data Management, System Stability and Calibration, International, Associated Benefits and Applications and Interaction with Solar Terrestrial Physics. Francis Bretherton, “The Earth System Science Committee – A Working Framework,” September 1984. Earth System Science Committee Papers, NCAR Archives, Box 3, Folder 83.

\textsuperscript{582} E.g Wilford F. Weeks, A. J. Gow, and R. J. Schertler, Ground-truth observations of ice-covered North Slope lakes imaged by radar (Hanover: CRREL Report 81-19. 1982).

\textsuperscript{583} Wilford. F. Weeks, Letter to the members of ESSC Cryosphere Working Group, January 9, 1985. NCAR, Box 3, folder 83.
component parts of the Earth System the committee had set out to construct. By using remote sensing, in particular by satellite, the Cryosphere working group aimed to align glaciological research goals with those of NASA and to use the vantage point from space to understand the cryosphere as “three distinct entities” (snow, sea and lake ice, and ice sheets and ice shelves).584 These three entities were divided geographically but particularly temporally. “There is an essential difference in timescales between snow and sea (and lake) ice on the one hand and the world’s larger ice sheets on the other”, the working group wrote, noting how the disparate times of different aspects of the cryosphere affected both how they should be studied and how they could impact society.585 The Greenland and Antarctic ice sheets were not deemed to be changing on timescales shorter than “hundreds to thousands of years”, rendering them less interesting for the working group’s focus on the upcoming decades. However, they also noted the ice sheets role as important indicators of climate change on longer timescales, but the “surprisingly poor” state of the knowledge around changes in the ice sheets made it difficult to draw any conclusions.

Another role of cryosphere temporalities concerned the hydrological cycles within the Earth System. Glacial flow, melting and freezing needed to be understood in order to produce a comprehensive model of the global hydrological cycle. Ice and snow appeared as frozen water, along with other types of water (in soil, underground water reservoirs and land-surface). Once again, timescales divided the waters up: “The relevant timescales are days for soil water, weeks for land-surface water and snow, weeks to years for sea ice and years to many decades for underground water and glaciers.”586

Ice and snow emerged with particular temporal and spatial qualities in the ESSC and the Cryosphere working group. In order to synchronize the cryosphere with the scientific and political times of the Earth System, ice and snow had to be conceptualized as input into a variety of planetary processes, such as climate change and hydrological cycles. But the melting of the ice sheets, for example, were deemed too slow and lacking too much data to be of immediate concern and therefore fell out of the Cryosphere working group’s focus. The remote sensing oriented program, enabled by the NASA affiliation, in turn rendered a spatially vast but temporally short framework for the working group that they themselves coined “satellite glaciology”.587 They were self-aware about the sometimes tricky work to translate the material properties of the cryosphere into data and Earth System Science theoretical frameworks. As Weeks jokingly put it in a memo headline: “MOM! There is an ICECUBE in with the acronyms!!”.588

585 Ibid., 8.
586 Ibid., 9.
Figure 5.3. Excerpt from Francis Bretherton's OH-slides from the ESSC meeting June 15, 1984. Earth System Science Committee Papers, NCAR archives, Box 4, Folder 99.

Why?

- Fundamental science
- More accurately predict the earth's future
- We live here
If paleoclimatology didn’t fit within the Cryosphere working group, it played a more prominent role in the Systems modeling working group. The group, led by meteorologist John Dutton, was a central part of the work of the ESSC, as an overarching goal of the committee was to create models of the future Earth System. The group’s main task was to “start a top-down approach of an Earth System model” and assign variables to the various subsystems underpinning the Earth System itself.586 The group gathered for the first time in Washington D.C., October 4–5, in 1984. They identified “interactions of processes or models on different timescales” as a “crucial challenge to be addressed”, even though they were also optimistic regarding the overall potential for modeling the Earth System.587 Francis Bretherton was part of the group, and presented on the topic of “Ocean Dynamic Modeling” at their first meeting.588 The geochemist Paul Crutzen – who would later coin the term Anthropocene – was present as well and talked about geochemical models and processes. Paleoclimatology was represented by John Imbrie, one of the central members of the CLIMAP project in the 1970s (see chapter 3), and he became part of the group in order to bring together different strands of paleoclimatological research. These different ways of measuring change had to be made to fit within the framework of the ESSC. At the first meeting, he gave a talk about “Paleoprocesses and models”.589

Given the emphasis on timescales ranging from decades to centuries in the ESSC, the role of paleoclimatological data was to “illuminate” the short to medium range changes in climate that could be expected. Especially the last 18 000 years – the time passed since the last glacial maximum – was considered the most relevant frame of reference to model future climate change.590 This timeframe coincided with John Imbrie’s previous work in CLIMAP, but the ESSC also cited ice cores from Greenland and Antarctica as potential sources for this particular time period.591

The overarching intellectual goal of the Systems modeling group transcended individual datasets, such as the one from CLIMAP, and instead brought together disparate

586 John Dutton, Memo to ESSC Systems modeling working group, undated 1984, NCAR Archives, box 3, folder 89.
587 John Dutton, Memo to ESSC Systems modeling working group, October 28, 1984, NCAR Archives, box 3, folder 89.
588 This was a topic well familiar to Bretherton and he had published extensively on these matters before, e. g. Francis Bretherton, “Ocean Climate Modeling,” Progress in Oceanography 11:2 (1982): 93–129.
589 ESSC Systems Modeling Group, “Agenda, First meeting, 4 and 5 October 1984,” NCAR Archives, box 3, folder 89.
590 Earth System Science Committee, Draft Report of the Earth System Science Committee, Ch. 1, September 8, 1985, NCAR Archives, box 1, folder 7; Earth System Science Committee, Earth System Science: A Program for Global Change, 87.
forms of data and knowledge. “Our knowledge about the subsystems is greater than our knowledge about the system”, the ESSC wrote in an early draft of their first report.\footnote{ESSC Systems modeling working group, First Draft: Modeling the Future of Planet Earth, 15 May, 1985, NCAR Archives, box 1, folder 21.} The goal was to tie existing models together, rather than making a new one from scratch. Despite the immense scale of this task, the Systems modeling working group remained optimistic about the prospects of succeeding with their task. The systems oriented approach allowed for a progressive integration of subsystems and the coupling of models on different scales into one coherent framework. The “one model to fit all”, as Ola Uhrqvist puts it, sparked great excitement among the scientists, enabling seemingly infinite additions to the system.\footnote{Ola Uhrqvist, “One Model to Fit All? The Pursuit of Integrated Earth System Models in GAIM and AIME,” \textit{Historical Social Research / Historische Sozialforschung} 40.2 (2015): 271-297.} ESS could become, in the minds of its practitioners, a neo-Humbolditian, holistic science. Fueled by computers and satellites it should enable a unified epistemology of the entire planet. The scientists of the ESSC were self-consciously framing their work a return to an earlier ethos of science. As they put it in one draft of their first report: “In a way, this approach harkens back to the era of the ‘Renaissance Natural Scientist’, who strove to generalize knowledge across discipline lines.”\footnote{Earth System Science Committee, Draft of the First Report of the Earth System Science Committee, June 11, 1985. NCAR Archives, box 1, folder 4. These kind of comparisons with perceived historical predecessors have continued after the ESSC, perhaps most famously in H. J. Schellnhuber’s 1999 assertion that ESS could be considered a “second Copernican revolution”. H. J. Schellnhuber, “Earth system analysis and the second Copernican revolution,” \textit{Nature} 402 (1999): C19-C23.}

Going across discipline lines also marked a further step towards environmental governance.\footnote{Heymann Dahan-Daldemico, “Epistemology and Politics in Earth System Modeling.”} The imperative of correct future modeling was framed as a feature of a new kind of human-Earth relationship, in which “Planet Earth has arrived at a point in its history that the people who live upon it could, and probably should, control its future.”\footnote{Heymann and Achermann, “From Climatology to Climate Science,” 621.} Jumping from “could” to “should”, from descriptive to normative, was both a shift towards a more applied idea of what ESS could do and an appeal to a technocratic form of management of the system. It opened the door to “a globalizing reductionism” of quantifiable planetary and social dynamics.\footnote{ESSC Systems modeling working group, First Draft: Modeling the Future of Planet Earth, 15 May, 1985, NCAR Archives, box 1, folder 21.} As the Earth System was coming into being as a distinct scientific object, with its own epistemology and political ramifications, the role of paleoclimatology became less pronounced. The role of proxy records was providing a historical baseline of a now clearly defined Earth System. As the ESSC Systems modeling group put it in one early draft:
“The history of the system appears in the geological record, in fossils, in pollen, in ice and ocean cores”.\textsuperscript{598} Despite the wildly different temporalities of these records, they were, in relation to the vaguely defined temporal properties of the Earth System, synchronized into one unified source of the system’s history.

This temporal flattening of different proxy records is particularly evident in the Bretherton Diagram. The diagram, which aimed to visually represent the Earth System as coupled boxes divided into subsystems, is perhaps the most famous output of the work of the ESSC. It is also an indicative illustration of the role paleoclimatology came to play in the realization of the ESS framework. Ice cores, deep-sea sediment cores and lake/bog cores were all given a box in the system, thereby providing temporal underpinnings to the

\textsuperscript{598} ESSC Systems modeling working group, First Draft: Modeling the Future of Planet Earth, 15 May, 1985, NCAR Archives, box 1, folder 21.
system as a whole. Yet, they were at the same time lacking any distinct temporal qualities, serving only as input to determine the stability of the system of timescales ranging “from decades to centuries”. The utility of proxy records in ESS, and the Bretherton diagram in particular, could thereby be seen as a compression of paleoclimatological temporalities into an intellectual framework that primarily sought to make itself relevant for the political temporalities of environmental governance.

Even though the mission statement in the 1986 ESS Overview report made clear that ESS concerned “all timescales”, the temporal focus narrowed down as the project went along. The solid Earth sciences such as geology and geomorphology were represented in two working groups – Geophysics and Geology – but in the publications of the ESSC, their long timescales didn’t fit with the overall research agenda. In the final report, Bretherton solved the problem with too divergent timescales by placing “changes on hundreds to millions” of years in a separate section, with a separate wiring diagram. C. Barry Raleigh, the chair of the Geophysics working group, wanted Bretherton to add a caveat in the Closer Look report, which would mention the short-term focus of the report, but Bretherton did not think it would fit.

The clashing temporalities – and competing temporal sensibilities – were not foreign to the ESSC members themselves. On the contrary, they were self-aware about the somewhat arbitrary nature of constructing an Earth System and, particularly, finding a temporal framework that could include all timescales. In a memo entitled “Contact made with life in outer space”, sent from John Imbrie to the rest of the System modeling working group, he jokingly outlined the fictive finding of the Planet Dynos. A planet with intelligent life, the same size as Earth, inhabited by Dynapeople who spoke a strange kind of English and had a much faster metabolism than humans. This fast metabolism meant that Dynapeople lived only for 40 hours. A fictive Bretherton, interviewed in Imbrie’s saga, said that he first felt sorry for them, but later realized that many other things went faster – 22 000 times faster – than for humans: “hunger, menstruation, telephone billing and so on”. This also had implications for the climate scientists and meteorologists on Dynos, as the relatively slow pace of weather and climate – which was roughly the same as on Earth – meant that most Dynapeople experienced only one weather during their lifetime.

As the story goes on, Imbrie identifies the most hard-working scientists on Dynos: the paleoclimatologists. They have to establish weather patterns reaching several months into the past, and present their findings to the skeptical meteorologists. The

600 Earth System Science Committee, A Closer View, 26.
602 John Imbrie, Memo sent to the ESSC Systems modeling group, October 11, 1984. NCAR Archives, box 3, folder 89, 2.
“Dynacongress” funded a Deep-sea Drilling Project, and their cores showed indications of an ice-covered Dynos ten months before. What human scientists would consider as a regular winter, the Dynapeople would consider as an ice age. Imbrie asserted that the planets were quite similar, but the temporal frames through which we understood them were different. He ended the story with what appears to be a comment on the perilous status of paleoclimatologists in the ESSC and the epistemic status of proxy records in their work. In the last lines of the Dynos saga, John Dutton, the leader of the Systems modeling working group, commented on the Dynos discovery and the trouble of understanding the temporal qualities of their climate system. Even though the fictive Dutton confessed they had “a long way to go” in understanding the difference between the climates of Dynos and Earth, he also thought that shouldn’t be to alarming as “after all, we are talking about paleoprocesses, not real processes”. 603

Although the Dynos story is written in a jokingly manner, it points to the tricky nature of making the Earth System’s temporalities function within a multi-disciplinary framework. For paleoclimatologists, ESS and adjacent projects such as the IBGP (International Geosphere Biosphere Program) marked an opportunity to use their expertise within a new epistemology of the Earth System. 604 But it simultaneously meant that some epistemic authority was lost, as the temporalities and practices of 1980s Earth System modeling favored other modes of knowledge production and shorter timescales. Some, like Imbrie, took on a role as a mediator between paleoclimatological temporalities and ESS, and adopted the logics of the planetary-scale system approach onto his own work. Among ice core scientists, Hans Oeschger is a similar example. He joined the IGBP in the mid-1980s and later also be a lead author in the first IPCC Assessment Report. 605 While Dansgaard, by the mid-1980s was approaching his retirement, Oeschger took further steps in incorporating the systems approach to planetary dynamics with ice core research.

603 Imbrie, Memo to ESSC, 4; Barton, Wiring the World, 169-175.
In a 1985 entry in the volume “Greenland Ice Core: Geophysics, Geochemistry, and The Environment”, which was edited by Hans Oeschger, Chester C. Langway Jr. and Willi Dansgaard, Oeschger discussed ice core timelines in relation to the “environmental system”, in a way that was very similar to the contemporary work of the ESSC. The goal with this practice, Oeschger explained in the article, was to use ice cores to produce coherent and systematic knowledge about “the environmental system” as a whole. This system encompassed “the entirety of physical, chemical and biological processes action in the earth’s surface and in the atmosphere.” Further emphasizing the practical qualities of the concept, Oeschger asserted that the environmental system should be seen as a “research approach”. Working with environmental systems, in the way Oeschger defined them, was

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in other words as much about defining an object of study as it was a way to conduct research, a scientific practice.

The chapter also included his own visualization of the “environmental system”, which resembles the Bretherton Diagram, although it is not a wiring diagram and encompasses a more multi-faceted array of “human activities” (figure 5.5). His own work in ice core research can in this diagram be incorporated into larger framework of both the planetary scale and the emergent realization of human impact on fundamental aspects of Earth’s processes. The diagram simultaneously brings together phenomena that had previously not self-evidently belonged together – nuclear weapons tests, volcanic eruptions, ocean sediments – into one image, but also flattens the political aspects of these phenomena. A nuclear bomb going off appears to be as natural as a volcano erupting. Both Oeschger and Bretherton can in this way be said to represent an ambition to incorporate human affairs into a pre-existing systems approach made for the study of natural phenomena. As Castree et. al puts is, they are risking complicity in the processes they seek to understand by “refusing to explore the full range of values, means and ends that might guide human responses to GEC [Global Environmental Change], researchers may implicitly endorse the societal status quo by neglecting to question it fundamentally.”  

Oeschger’s embrace of Earth System modeling was, however, not the only way to go. The particular way of using climate proxy record in ESS, and Oeschger’s own environmental system approach, was tied to broader historically contingent developments in climate modeling and environmental politics.

In the second chapter of this thesis, I mentioned Margaret Bryan Davis, a palynologist and paleoecologist who began her career as a visiting graduate student in Copenhagen in 1953–1954. She studied for Johannes Iversen, who would become Denmark’s state geologist, and frequented the scientific environment around the Isotope Colloquium. In 1953, she went to Greenland to study pollen samples found along the island’s coasts. At the same time, on the icebergs outside Greenland, Willi Dansgaard was beginning to develop his isotope dating method of glacial ice, which would later serve as the foundation for modern ice core research.

By the mid-1980s, the graduate students doing fieldwork on Greenland had become prominent voices in their fields. They held professorships and positions in international scientific societies. Margaret Bryan Davis was in 1986, the year of the first ESSC report, a

609 I elaborate more on this in chapter three.
professor at the University of Minnesota, former president of the American Quaternary
Society and the president-elect of the Ecological Society of America. Her research on how
pollen samples could reveal how plant communities responded to climatic changes had
made her an authority on the study of biodiversity in changing climates.\(^{610}\) However, the
proxies she was using – mostly pollen and plant fossils – and the metrics she was working
with – biodiversity, the historical habitat of individual species on regional levels – did not
fit as easy as global temperature data into the climate modeling regime emerging in the
1970s.

In 1989, Bryan Davis held her presidential address to the Ecological Society of
America. She cautioned against getting too carried away with the planetary-scale models
that had taken hold in the last decade. Regional vegetation histories did not necessarily fit
in this framework and, she argued: “This result from the past means that species can be
expected to respond individualistically to climatic changes in the future. We must not build
models that predict the future by shifting existing communities or biomes around on the
surface of the globe.”\(^{611}\) The following year she made a similar point, but explicitly
addressed the International Geosphere-Biosphere Program, and cautioned against
oversimplifying ecological processes. Asserting that “the paleorecord argues against such
simplification” she brought the tensions between global models and regional and local
responses to climate change to the forefront.\(^{612}\)

At this time, the global climate modeling framework had “seized hegemonic status”
of the ways in which planetary dynamics were conceptualized scientifically and
politically.\(^{613}\) Some 35 years after Bryan Davis searched for pollen on Greenland while Willi
Dansgaard sampled icebergs for temperature data just off the coast, the epistemologies and
utilities of paleoclimatology had been transformed. The times they sought to make visible,
in fossilized plants and air bubbles, had, by the end of the 1980s, become conceptually
synchronized with an emergent understanding of humanity as a driving force of planetary
dynamics.\(^{614}\)

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\(^{610}\) E.g Margaret Bryan Davis, “Palynology and Environmental History During the Quaternary Period,”
*American Scientist* 57:3 (1969): 317–332; Margaret Bryan Davis, “Climatic instability, time lags, and

\(^{611}\) Margaret Bryan Davis, “Address of the president: Insights from paleoecology on global change,” *Bulletin

\(^{612}\) Margaret Bryan Davis, “Biology and paleobiology of global climate change: Introduction,” *Trends in


\(^{614}\) Sverker Sörin and Erik Isberg, “Synchronizing Earthly Timescales: Ice, Pollen, and the Making of Proto-
Anthropocene Knowledge in the North Atlantic Region,” *Annals of the American Association of
This process, as the divergent fates of palynology and ice core drilling show, was not just about progressive increase of scientific knowledge, but rather the political, conceptual and scientific alignment of particular temporal and spatial scales. It was also, drawing on Dan Edelstein, Stefanos Geroulanos and Natasha Wheatley, an example of chronocenosis, competing temporal sensibilities and non-reconcilable ways of understanding time. Earth System Science, and particularly the Earth System Science Committee, became able to function as a powerful arena in which the temporalization of planetary dynamics could take on a particular form.

Conclusion

This chapter has traced the relationship between paleoclimate temperature data and the rise of Earth System modeling. I have argued in this chapter that if we are to understand how Earth System Science became the dominant mode of researching and conceptualizing planetary dynamics, we must also pay attention to how different planetary and political times have been – or have not been – synchronized throughout its history. This has both empirical and, given the current historiographical debates around the relationship between history and ESS, theoretical implications.

Finding a temporal framework for the Earth System was a central aspect of constructing a coherent scientific object and the issues of synchronizing disparate timescales into climate models preceded the rise of ESS with more than a decade. This development happened both within the climate modeling community, but also, as the chapter shows, among paleoclimatologists themselves. Willi Dansgaard appears in this context as a scientist who realized the potential of expanding ice core research beyond the boundaries of glaciology and the cryosphere, but at the same time remained committed to work with ice as his main scientific object. The integration of paleoclimate data in climate modeling that occurred in the mid-1970s meant an epistemological and political expansion of paleoclimate temporalities into new domains. But it also meant a loss of epistemic authority of the content, meaning and scope of the climate proxy records for the scientists who directly engaged with them. Other scientists as well as other societal actors, such as E. E. David JR. of Exxon, became part of the production of planetary temporalities and the attempts to produce and predict integrated planetary-human futures.

When the Earth System Science Committee began their work in 1983, they had to navigate political timescales and the disparate timescales of a variety of scientific fields. Paleoclimatology did not have its own working group. Instead, it was represented by paleoceanographer John Imbrie in the System modeling group, which is indicative how the times of paleoclimatology had become fully globalized and immersed in a scientific and political framework of future modeling. Ice core scientists such as Hans Oeschger decided, like Imbrie, to join the Earth System modeling framework and developed their own “environmental system” research approach as an interpretive framework for paleoclimatological research. Paleoclimatologist Margaret Bryan Davis on the other hand did not fit so well into this framework, as she operated mostly on a regional scale and with other metrics than temperature. She urged caution towards the Earth System ontology that would seize hegemonic status in global climate politics with the rise of the IPCC in the 1990s.

The synchronization – and non-synchronization – of these phenomena between 1970 and 1990 depended on particular institutional formations and negotiations between alternate temporal sensibilities in scientific disciplines as well as political and economic interests. Contrary to the description of paleoclimate records as “natural archives” underpinning ESS and global climate politics, this history rather points towards the work of synchronization by a few key actors and institutions which enabled the ESS temporalization to take hold. Despite the ambition to work “on all timescales”, ESS was a selective enterprise, that focused on changes that occurred on “decades to centuries” as well as paleoclimate temperature records that could function on a global scale.616

According to Dipesh Chakrabarty, ESS has enabled the emergence of a “new regime of historicity” which brings together “the history of the planet, the history of life on the planet, and the history of the globe made by the logics of empires, capital, and technology.”617 It is, however, less evident in his writings how this new regime of historicity came about in practice. Under which conditions did the history of the planet encounter the history of the globe?

In this chapter, my aim has been to show a possible history of one such encounter. The chapter shows the historically contingent factors that affected how it could unfold in this particular instance. The history of the planet, as it has been configured through ESS, is more temporally multifaceted than it might first seem. Following the proxy records from the ice sheet and the ocean floor to their “boxes” in the Bretherton diagram reveals a history of standardization and synchronization. But it also shows a history in which planetary times

have been reconfigured and rearranged over time. The times of ESS were never as stable as they would appear in the final publications of the ESSC. Following the histories of these times can open up, I have suggested in this chapter, more nuanced ways to approach this “new regime of historicity”.
Forty years passed between the first Copenhagen Isotope Colloquium in 1948 and the publication of the final report of the NASA Earth System Science Committee in 1988. In historical terms, this is not a long time. Yet, for the scientists who measured, conceptualized and negotiated the temporal properties of planetary dynamics, these two events, separated by a mere forty years, were worlds apart. In this thesis, I show how, despite how disparate they may seem, the Isotope Colloquium and the Earth System Science Committee belong in the same history.

This history, I demonstrate in the chapters above, is a history of how the temporalities of postwar environmental knowledge were configured. Disparate disciplines, technologies, practices and temporal sensibilities were drawn together, as tracking and forecasting planetary dynamics became a pressing scientific and political objective. Paleoclimatology became a key site in which the boundaries between geological, environmental and political times were continuously re-negotiated. These times did not appear out of nowhere. In order to make the planet a coherent environmental object, its temporal properties had to be defined. But how this should be done, and with what means and organizing logics, changed over time. Planetary times had to be made, not merely discovered. Or, in other words, the times themselves were historical.

Paleoclimatology, and the practices of ice- and deep-sea core drilling, can therefore be situated in a longer history of negotiating the boundaries between human and natural histories. The divide between natural and human history has, since a few centuries, become increasingly deep and characterized by epistemological and temporal differences between various historically oriented disciplines.\textsuperscript{618} The holistic \textit{historia naturalis} was, from the

eighteenth century and onwards, gradually replaced by distinct disciplines with their own
temporal frameworks and research methods. In recent years, however, the rise of the
Anthropocene concept has led to calls for finding new ways of bridging this divide once
again. If human activities are a geological force, this line of argument goes, then the
division between natural and human histories collapses. The rise of the Anthropocene
concept in the last two decades is often taken as a point of departure: it marks an abrupt
break with the modern division of natural and human history and opens the door to a new
kind of historical sensibility.

If we instead view this development from the vantage point of the history of postwar
paleoclimatology, a different story emerges. The Anthropocene concept appears not so
much as a radical conceptual innovation as the outcome of decades of scientific production
and negotiation of widely disparate timescales. Just like any kind of environmental
knowledge, the notion of the Anthropocene is a product of historical processes. This is
in itself not a new idea. The history of paleoclimatology provides a way to study the
processes in practice. It is a way to make visible how the production and synchronization of
time is a material, situated and historically contingent process, not just the outcome of
abstract theorizations. Given that the environment is a fundamentally temporal concept,
the ways through which phenomena in nature acquire a temporal dimension and become
intertwined with historical and political conditions, is an important but often hidden aspect
of how we come to understand the environment.

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Helge Jordheim, “Natural Histories for the Anthropocene: Koselleck’s Theories and the Possibility of a

1997), Brian W. Ogilvie, *The Science of Describing: Natural History in Renaissance Europe* (Chicago: The
University of Chicago Press, 2006); Helen Anne Curry, Nicholas Jardine, James Secord and Emma C.

Dipesh Chakrabarty, *The Climate of History in a Planetary Age* (Chicago: The University of Chicago
Press, 2021); Julia Adeney Thomas and Zoltán Boldizsár Simon, “Earth System Science, Anthropocene


5-32.

Etienne Benson, *Surroundings: A History of Environments and Environmentalisms* (Chicago: The
University of Chicago Press, 2020); Paul Warde, Libby Robin, and Sverker Sörlin, *The Environment: A

For an overview of the historical genealogies of the Anthropocene concept, see: Deborah R. Coen and

Alessandro Antonello and Mark Carey, “Ice Cores and the Temporalities of the Global Environment,”
I begin my story in the 1940s. During this time, new ways of measuring time in the natural world were emerging. In particular, the invention of radioisotope and stable isotope dating methods, stemming from nuclear physics, produced a temporally oriented research program in a wide range of natural sciences, as well as archaeology and paleontology. At the same time, new technologies which aimed to access previously unexplored vertical depths of the ocean floor and glacier environments were also beginning to be developed. The geophysical, climatological and geochemical dynamics of different parts of the planet – atmosphere, hydrosphere, cryosphere – were becoming scientific objects in their own right. We may say that they were temporalized, hence put into motion in ways that were more quantifiable and possible to monitor than they had ever been before.

One such environment that gained new temporal qualities during this time was the ocean floor. The combination of isotope dating and enhanced drilling capacities made it possible to access sediments further down, thereby expanding the ocean sciences vertically below the seabed. I zoom in on this history by following the work conducted at the Oceanographic Institute in Gothenburg in the late 1940s and early 1950s (chapter 2). Even though it was a comparatively small research environment, it came to have a big influence on the development of deep-sea core drilling and historically oriented research concerning the ocean floor. The invention of the “Kullenberg piston corer” and the 1947–1948 circumnavigation with the Albatross expedition – which was focused on deep-sea core drilling – lay the groundwork for later, large-scale drilling operations in other contexts beyond Sweden and Gothenburg. Even though the scientists themselves spoke about deep-sea core drilling as a process of excavation and discovery, the work of rendering the ocean floor into a temporally coherent space necessitated conceptual as well as disciplinary leaps.

While it stood clear to the Albatross scientists that they were part of making a new history of the oceans, the temporal and spatial scales of this history were far less evident. Börje Kullenberg, who invented the corer that motivated the expedition, worked in the 1940s to synchronize his deep-sea core chronologies with Swedish quaternary geology and postglacial archaeological history. He brought his corer to Ångermanälven in northern Sweden, where he worked alongside the future state geologist of Sweden, Erik Fromm,

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within a nationally oriented research framework. Hans Pettersson, who led the expedition, was on the other hand keen to present the deep-sea core chronologies as part of global geological knowledge, which he could connect to the globalizing ambitions of the recently founded UNESCO. The Oceanographic Institute collaborated with scientists from the Institute of Nuclear Studies in Chicago in order to develop isotope dating of deep-sea sediments while simultaneously utilizing the recently established Swedish Board for Computing (Matematikmaskinnämnden) to process the vast amount of data the cores enabled. As the temporal qualities of the ocean floor changed, so did its epistemic geographies. New sites of knowledge production and new technologies were brought together by the cores.

Ice core drilling, despite its different materiality and disciplinary origin, emerged as a scientific practice in a similar way in the early Cold War period. In comparison to interwar glaciology, ice core drilling enabled a temporal and epistemological leap, from the localized and horizontal to the global and vertical. The vertical element was one important aspect, but far from the only one. I trace the early career of Danish ice core scientist Willi Dansgaard and show how he was part of a turn towards thinking of ice as a proxy for atmospheric conditions, rather than ice as a scientific object in itself. He did so before it was technologically feasible to drill deep into the ice sheet (chapter 3). Dansgaard’s research was animated by his geophysical background and his interest in using stable oxygen isotopes to establish temperature trends over time. He searched for “ancient atmosphere” in water, both as droplets in the air and in the deep trenches of the Pacific Ocean, before he began working in Greenland. In turning ice – or more precisely the preserved atmospheric bubbles embedded in its crystals – into a proxy, a stand in for past atmospheric conditions, Dansgaard contributed to an epistemological reorientation in which ice was studied in order to study something else. This “something else” was soon translated into “climate change”.

While Dansgaard himself would later talk about ice cores as “frozen annals”, the chronologies of early ice core drilling eluded such language. In order to translate ice into time, air bubbles into “ancient atmosphere”, Dansgaard and his collaborators in the 1950s relied on an interdisciplinary framework which encompassed geophysics, glaciology, nuclear physics and physiology. Dansgaard collaborated with the physiologist Per Scholander in a multi-scalar research framework: they were interested in gaseous exchange

between bodies in the Arctic, ranging from lichens and seals to icebergs and glaciers. They applied a systems-oriented approach to their object of study with the goal to create generalizable knowledge about how the interaction between carbon dioxide and oxygen should be understood, beyond local conditions and individual species or objects. In this setting, the planetary timemaking lacked a disciplinary framework and developed in idiosyncratic ways before the means to drill deep enough were in place. Scaling up a sample of ice to the planetary level depended on technologies, such as drills and mass spectrometers, but also on an epistemological framework in which ice was a proxy for past atmospheric conditions. Drilling itself was never enough.

By the late 1960s, new temporal registers were opening up in paleoclimatology. In both ice core and deep-sea core research, the temporal orientation had been directed towards making sense of past climatic events in order to get a fuller picture of how planetary dynamics operate. Around this time, with concerns about environmental degradation rising on the political agenda and climate modeling emerging as a promising scientific endeavor, the future appeared as a new temporal category in paleoclimatology. The turn towards prediction also led to a need to synchronize different proxy records with each other, in order to meet with the strong global focus of emergent environmental governance efforts.

Deep-sea core drilling, which in Pettersson’s iteration was an activity directed towards the past, acquired a strong future component as the practice became a part of the research program of the UN International Decade of Ocean Exploration (1971–1980). I show (chapter 4) how American deep-sea core drilling came to be enrolled in the imperative of “prediction and rational utilization” of the ocean. In large-scale research projects, such as the Deep-sea Drilling Project (DSDP) and CLIMAP, the scientists navigated a multi-temporal landscape, and actively sought to make their temporal frameworks usable for actors beyond the ocean sciences. Translating sediments into time was, despite the seemingly straightforward stratigraphy of the cores, increasingly complicated as the times of the cores could be synchronized with models of future planetary conditions. A close reading of the correspondence between scientists in these projects reveals how they became aware of the political nature of their work, and how they struggled to find a clear-cut usage for their data in economic, agronomic and climatological forecasting. The 1970s appears as a decade in which the boundaries between the times of deep-sea core drilling and the times of political and economic prognostication were continuously renegotiated.

It was also a decade in which new connections were made between different strands of paleoclimatology. Willi Dansgaard, who by the late 1960s had emerged as a central figure for a climate oriented ice core science, was keen to connect his work to questions about future climatic conditions. His publications and engagement from this period give the picture of a scientist who wanted to remain in his field of ice core research, but nevertheless sought to expand the temporal, epistemological and political scope of his work. Throughout the 1970s, he attended the same workshops as scientists from the DSDP and CLIMAP, bringing together ice cores with other proxy records, as well as with climate models. He published widely about climate modeling, the demise of the Norse settlement in 15th century Greenland and the fate of humanity in a changing global climate. At the same time, his work began to be cited in articles and policy documents that had nothing to do with Greenland or the properties of the ice sheet. Ice core data was increasingly treated as a ready-made planetary-scale object, with its spatial and temporal scales already defined to fit within a burgeoning infrastructure for climate forecasting and goverance. His ice core research was reaching, as he put it in 1973, “far beyond glaciology itself”.

Dansgaard’s work in the 1970s, as well as the DSDP and CLIMAP, are examples of how a change in temporality – directing the research focus towards the future rather than strictly the past – also changed institutional frameworks and who would consider paleoclimate data a relevant resource. The cores did indeed grow, both materially, spatially and quantitatively, as they became more numerous and went further back in time. They augmented in other ways as well, becoming those very special objects through which a small and exclusive group of scientists could speak to the future predicament of the entire planet. In this sense, the transformations of paleoclimatology in the 1970s became indicators of a larger shift in how natural and political times related to one another. To make sense of this profound transformation, I draw on Reinhart Koselleck’s distinction between the historical and the metahistorical – what is perceived as unfolding within human history and what are the “pregivens” that lie beyond history – and his argument that the relationship between

the historical and metahistorical changes over time. This distinction makes it possible to view the discussions around the societal utility of proxy records in the 1970s as about something much more fundamental: the boundary between what should count as historical and what should be termed metahistorical. Times that had previously been regarded as distinctly outside the domain of history were becoming synchronized with phenomena such as economic and agricultural forecasting, climate modeling and environmental politics.

I end my story with the NASA Earth System Science Committee, which operated between 1983 and 1988 (chapter 5). In particular, I focus on the role of proxy records in the Committee’s work to assemble a coherent Earth System. In this setting, ice- and deep-sea cores were fully mediated and quantified into planetary-scale datasets. Any trace of the local and contextual origins – from Copenhagen, Gothenburg, Greenland, the floor of the North Atlantic, or any other location that had been part of making the records – were at this point invisible. The ESSC’s planetary timemaking was not material in the same sense: they had all the data they needed, but they had to assemble it in ways that rendered their framework useful for both science and policy. This came to guide the ESSC, as they zoomed in on relatively short timescales, and decided which kind of metrics they would primarily focus on. The geochemical and geophysical data would prove more useful in this highly scaled-up and aggregated view of the planet than, for example, data on vegetation or biodiversity.

The ESSC saw themselves as operating on “all timescales”, but their temporal multiplicity should not be seen just as an expansion. In order to compute what they considered a complete view of planetary dynamics with limited computing capacity, the making of Earth System Science was also a reduction of relevant metrics and temporalities. The Bretherton Diagram (figure 5.4), launched in 1986 as an attempt to visualize a quantifiable Earth System, is an indicative example of how Earth System Science (ESS) created a multi-temporal understanding of the planet, while at the same time flattening the involved temporalities into one chart. It allowed for a synchronization of different proxy records and human activities, but it did so in a way that also rendered other ways of temporalizing and scaling natural and human times impossible. I use the work of palynologist Margaret Bryan Davis to highlight this tension. It was not obvious how her pollen samples, and historical data on regional vegetation variabilities and responses to


climate change, would fit in the Earth System framework. Her objections to, in her view, overly generalist models show there was no unified idea about how the planetary past should be assembled in Earth System models.639

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In the beginning of this thesis, I listed two main research objectives. One was empirical, the other theoretical. Empirically, my aim was to place time and temporality as the central categories of postwar planetary-scale environmental knowledge. Paleoclimatology, and ice- and deep-sea core drilling in particular, were during the postwar era activities that came to shape environmental knowledge by providing temporal frameworks through which the environment could be studied. In this sense, the thesis has situated paleoclimatology in environmental history, by highlighting how the environment is a fundamentally temporal concept that has to be understood as being produced both in space and in time. It is a history that shows how paleoclimatology enabled a particular temporalization of the environment. It also demonstrates how it did so, and why it matters under what conditions science produces time.

By closely following key actors in ice- and deep-sea core drilling in the North Atlantic region, it becomes possible to see how a small group of scientists, technologies and practices could begin to speak for the entire planet. In the early postwar period, when the times of paleoclimatology were still not immersed in an environmental framework, the timemaking relied on Cold War scientific infrastructure and the introduction of isotope dating technologies in field sciences such as glaciology and oceanography. By turning the ocean floor and the ice sheet into conversation with each other and using their respective information as proxies for past climatic conditions, it became possible to decouple the paleoclimate data from its geographical origins. By the 1970s, when the future appeared as a temporal category for paleoclimatologists to engage with, things got more complex. When CLIMAP members John Imbrie and James Hays, in 1974 discussed how they perhaps could “translate our maps of ancient equilibrium into humanistic terms” they were putting into words the larger temporal shift: paleoclimatological times were intersecting with human history.640 Imbrie, Hays and their colleagues saw an opportunity to connect their deep-sea core research to technocratic visions of forecasting and planning economic, agricultural and climatological futures. Their temporal negotiations are indicative of how

640 James D. Hays and John Imbrie, Memorandum to the CLIMAP Executive Committee, undated 1974. NSP, box 4.
ice- and deep-sea cores, despite their status as unequivocal “natural” archives, never spoke for themselves.

My second research objective concerned the theoretical implications of my research. By drawing together environmental history and history of science with the theory of history, my aim was to bring forward the mutually beneficial aspects of combining these different fields. As environmental knowledge is dynamic and, at a very basic level, concerned with how various phenomena change over time, it appears as an important task for environmental history and history of science to understand how these changes become known. Yet, histories of environmental knowledge have typically been more interested in space than in time, and given rise to an elaborate conceptual apparatus about ways of measuring, mapping and delimit environments. By bringing concepts from the theory of history – such as synchronization and temporalization – which are mostly used for sociopolitical rather than scientific or environmental times, I have suggested a way to empirically study how environments gain their temporal qualities.

On the other hand, as many theorists of history are now directing their gaze towards the Anthropocene and times beyond human history, I believe the theory of history would benefit from a closer engagement with the historical processes that produced the knowledge which underpins their discussions. Ice- and deep-sea cores bring forward how time is not an abstract, immaterial category, but something that is material and the outcome of, in the case of environmental times, scientific work. Not taking for granted the temporal framework of Earth System Science, and subsequently also the scientific articulations of the Anthropocene concept, could open up for a more nuanced and critical discussion around the status of historical time in the proposed new geological epoch.

When Paul Crutzen in 2000, at a conference in Cuernavaca, Mexico, stood up and declared that we had entered the Anthropocene, his intervention was understood as a new conceptual framing of the human-Earth relationship. It sparked a wide range of debates – around environmental degradation, global justice, the legacies of colonialism and perils

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641 In her work on contemporary geology, Sophia Roosth shows how studying how geologists produce time is a way to both see how geological time becomes globalized and how geologists constantly negotiate and navigate the relationship between local idiosyncrasies and planetary frameworks. She writes: “Geologists themselves do fieldwork precisely to critically evaluate the synchronicities and disjunctions of the rock record, to tether contingent temporal objects into something approaching a global timescale.” Sophia Roosth, “The Sultan and the Golden Spike; or What Stratigraphers Can Teach Us about Temporality,” *Critical Inquiry* 48:4 (2022): 713.

of capitalism – which are still ongoing today. Geochronological charts have ended up at the center of these debates because they show, in Heather Anne Swanson’s words, “how seemingly banal time charts are a form of infrastructure that shapes environmental management practices, research agendas, and policy negotiations.”

For historians today, Earth System temporalities are being invoked in calls for a new “Anthropocene history”. It aims to bring together the knowledge provided by Earth System Science with the historicities of historical scholarship. While this field is still “unsettled” and lacking a clear institutional framework, it is a recent example of how scientific timemaking is moving closer to historical time. While these developments are new, and operate on far larger temporal scales than historians are used to, they are also, as Sebastien Felten and Renée Raphael recently pointed out, in line with previous work in environmental history, which has relied explicitly on scientific concepts for explaining historical processes. The conceptual language of the Anthropocene thereby poses a challenge for historians of science: scientific knowledge produced in ESS is foundational in the calls for a new Anthropocene history, while historians of science also are committed to historicizing scientific knowledge. To what degree can the concepts from ESS help us writing a new kind of history? Are they too indebted to the contingency of their origins?

In this thesis, my aim has not been to provide a definitive answer to these questions. Instead, I have suggested that an increased attention to how scientists – in their practices – produce and conceptualize time can be a way to better approach the planetary times that are moving towards the domain of history. The aggregated, quantified planetary-scale is


necessary for accounting for anthropogenic pressures on the Earth System. However, it is not the only way of conceptualizing how natural and human times intersect and entangle. The history of postwar paleoclimatology is a history of continuous negotiation of the relationship between times, materials, practices and politics. It makes it possible to study environmental times in situ, as they emerge, form, and travel. This appears, I argue, as one way of approaching the “genuine conceptual challenge” of thinking historical time in the plural.\textsuperscript{648} There are more ways, alternate epistemic foundations, which can help us conceptualize the intersection of human and planetary time than the ones provided by planetary-scale models.\textsuperscript{649} The history I have outlined in this thesis is far from the only possible one. On the contrary, my aim has been to provide some tools to approach environmental times as material and situated, rather than abstract and de-spatialized, in other historical settings.

Zooming in on the particular postwar genealogies of the planetary turn, with its conceptual and institutional legacies very much still with us, makes it possible to distinguish a phase of proto-Anthropocene knowledge production.\textsuperscript{650} During this period, particular practices of making and synchronizing time gained institutional and political momentum. But this history also shows how notions of planetary time were continuously reconfigured, made to speak on different scales and on different temporal registers. Taking the historicity of planetary time seriously – and not treating it as a ready-made for theorizing about history in the Anthropocene – opens up for more multi-facetted approaches to historical time.

Crutzen’s sudden epiphany in 2000 appears not so much as a moment of brilliance, but as the crystallization of a decades-long process of bringing together and drawing apart a wide set of times stemming from a range of scientific practices and geographies. I end this history where many other texts on the Anthropocene concept begin. The decades of scientific timemaking that preceded the conference in Cuernavaca show us how the recent turn towards planetary times has a history. This history reveals, as all histories do, that what is taken for granted today was once unknown.


\textsuperscript{649} For a rich example of how this can be done in a wide range of historical settings, see: Alison Bashford, Adam Bobbette, and Emily M. Kern (eds.), \textit{New Earth Histories: Geo-Cosmologies and the Making of the Modern World} (Chicago: The University of Chicago Press, forthcoming 2023).

Den här avhandlingen handlar om paleoklimatologins historia under efterkrigstiden. Paleoklimatologi är studiet av klimatförändringar som ägde rum innan det fanns meteorologiska instrument att mätta dem med. För att få kunskap om sådana förändringar måste man istället använda sig av olika typer av proxyindikatorer i naturen och på så sätt rekonstruera klimatets historiska svängningar. Det kan röra sig om analyser av gaser som finns kvar i luftbubblor i glaciärer, årsringar på träd eller sediment på botten av sjöar och hav. Idag är de här praktikerna en självklar del av modern klimatforskning och används till exempel i IPCC:s rapporter och som input till klimatmodeller. Men som allt annat har de här praktikerna också en historia, och det har inte alls varit självklart hur de skulle komma att bli en del av modern klimatforskning.


Avhandlingen avser på så vis att dels belysa en tidigare underutforskad del av miljövetenskapernas historia och dels bidra med ny teoretisk förståelse kring relationen mellan geologiska, geokemiska, ekologiska och historiska tider. I den samtida


Den svenska Albatrossexpedition var ett försök att just hitta den här typen av material. Genom en ny typ av djuphavsborr – kolvlodet – tänkte sig oceanograferna Hans Pettersson och Börje Kullenberg, som var initiativtagare till expeditionen, att det skulle bli möjligt att
borra sig ner i havsbottnen och på så sätt komma åt de sediment som fanns där under. Sedimentens stratigrafi, alltså deras skiktning, kunde i sin tur användas för att datera havsbottenens historia och de geologiska och klimatologiska skiften som den hade genomgått.


Det tredje kapitlet tar också sin utgångspunkt i Levis isotopkollokvium. Men istället för att följa isotopdateringens funktion i havsforskningen, följer kapitel tre framväxten av isotopdatering av is på Grönland och iskärneforskningens tidiga historia. Willi Dansgaard blev under 1960-talet en av ledargestalterna inom iskärneforskning och paleoklimatologi och var en flitig deltagare vid kollokvierna i Köpenhamn. Han intresserade sig för möjligheterna att använda is för att studera hur klimatet förändrats över långa tidsrymder. I kapitlet följer jag Dansgaards tidiga karriär och visar hur iskärnan inte bara var ett resultat av att man borrade djupt i Grönlands istäcke. Istället argumenterar jag för att Dansgaard företrädde en ny typ av ”isepistemologi”, som behandlade isen som ett medium för klimatet – en proxy – snarare än som ett studieobjekt i sig själv. Han var inte främst intresserad av isen som sådan, utan av den ”uråldriga atmosfär” han tänkte sig fanns i luftbubblorna inne i isen. Detta gjorde att han samarbetade med forskare från andra fält än glaciologi för att genomföra sina studier.

utbytet av syre och koldioxid hos djur och växter i Arktis och skalade tillsammans med Dansgaard upp sina studier till att omfatta gasutbyten i glaciärer och i hela atmosfären. De strävade efter en generaliserbar, systemorienterad kunskap om relationen mellan syre och koldioxid som kunde förstås bortom lokala förhållanden.


allt oftare betona borrkärnornas roll som garanter för framtida resursutvinning och korrepta prognostiseringar av klimat, energi och jordbruk.


När Hans Pettersson och Willi Dansgaard påbörjade sitt arbete var det inte alls självklart vad de olika tiderna som de studerade hade med varandra att göra, och inte heller om dessa tider hade några politiska implikationer. I avhandlingen utforskar jag hur detta var en process som utvecklades i relation till vetenskaplig infrastruktur, politiska prioriteringar och en allt större oro för mänsklig miljöpåverkan. Att studera efterkrigstidens miljötider som något i sig självt historiskt, som skapats, omförhandlats och standardiserats i särskilda kontexter, öppnar upp för vidare studier kring relationen mellan vetenskap, tid och miljö.

Att förstå miljötider som historiska studieobjekt har också teoretiska implikationer. Om planeten är, som Chakrabarty menar, en ”ny humanistisk kategori” och dess tidskalor något som även angår historia och politik, blir det samtidigt viktigt att förstå hur den här kategorin har uppkommit. Ofta förstas tid som något abstrakt och teoretiskt, eller rentav gåttfullt och gäckande som redan Augustinus menade. Men paleoklimatologins historia visar hur miljöns – och planetens – tider snarare är materiella och skapade på vissa platser genom vissa praktiker. Den samtida vändningen mot det planetära kan spåras bakåt genom efterkrigstiden, från dagens abstrakta modeller och teorier till konkreta materialiteter som
is, sediment och pollen. Genom dem blir en annan typ av planetär historia synlig. 
Paleoklimatologins historia visar hur relationen mellan mänskliga och planetära tider kontinuerligt omförhandlats sedan 1945 och gör det möjligt att placera de senaste årens debatter om antropocen och historisk tid i en längre historia. Det öppnar upp för en mer mångfacetterad förståelse av miljöns tider, som inte enbart utgår från jordsystemvetenskap och de senaste årens teoretiska debatter. En sådan är nödvändig för att förstå vad historia är, och kan vara, i en ny planetär tid.
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