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Research agendas in climate studies: the case of West Antarctic Ice Sheet research

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Abstract Concern over anthropogenic climatic change has been the major driver behind the rapid expansion in climate studies in recent decades. However, research agendas revolving around other intellectual or practical problems motivate much of the work that contributes to scientific understanding of present changes in climate. Understanding these agendas and their historical development can help in planning research programs and in communicating results, and it can often elucidate the sources of disagreements between scientists pursuing differing agendas. This paper focuses on research agendas relating to the possible glaciological instability of the West Antarctic Ice Sheet (WAIS). For much of the history of this research, which dates back to International Geophysical Year traverses, WAIS instability was thought of as innate rather than climatically triggered, even as a growing program of intensive field research was heavily motivated by tentative links drawn between WAIS instability and concerns over anthropogenic climatic change. Meanwhile, climate models for many years did not countenance instability mechanisms. It is only over the past fifteen years that field glaciological research has been integrated with other forms of empirical research, and that empirical studies of WAIS have been more closely integrated with the broader body of climate studies.

Keywords West Antarctic Ice Sheet · ice streams · glacial cycles · sea-level rise · history

1 Introduction

The imperative to understand and forecast anthropogenic climatic change and to assess its impacts has led to the increasing institutional and intellectual orchestration of the work of a large and diverse community of scientists. However, this broad community also comprises sub-communities who often pursue research agendas tangential to that imperative. These agendas can be instrumental in motivating individual researchers, and researchers' pursuit of them can lead to the discovery of unanticipated ideas and facts pertinent to understanding climatic change and its impacts. For this reason, funding agencies and research managers

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must strike deft balances between supporting overarching and local agendas. Similarly, synthetic writings, such as review articles and scientific assessments, must locate and draw on work done in the service of a variety of agendas. Generally, those undertaking such syntheses are forced, for the sake of brevity and to promote clarity for particular audiences, to neglect mention of the various goals that work was originally intended to further.

Understanding the nature of, and differences between, research agendas almost necessarily entails understanding their history. The goals and methods of different agendas are shaped by researchers' education, by their past research experience, by their established interests in certain practical or policy problems, and by their contact (or lack of contact) with various other scientists and scientific communities. Because researchers' goals and the presuppositions underlying their methods are only stated sporadically, and are often only vaguely articulated, historical study offers a means of recovering and clarifying them, and making them more widely appreciated. In this way histories of scientific research can make more visible the different perspectives that have contributed to scientific understanding, which would be beneficial, for instance, to graduate students learning to navigate a complex methodological terrain. It would also elucidate the often opaque nature of disagreements between researchers pursuing different agendas. Finally, it would help research managers articulate the rationales underlying the balances they strike between agendas, and possibly help them to strike better ones.

This paper discusses in detail how various research agendas have shaped research on the West Antarctic Ice Sheet (WAIS) over the past forty years. In that time all researchers interested in WAIS have agreed the subject should be of general interest because it rests on bedrock that is below sea level (Fig. 1), and may therefore be glaciologically unstable in spite of its deeply frigid environment. If WAIS were to disintegrate, the world's sea levels would rise by at least three meters on a time-scale potentially on the order of centuries (Joughin and Alley 2011). This case is particularly interesting for the purpose of highlighting the interaction of research agendas in climate studies because WAIS's instability was initially considered to be innate rather than climatically triggered. Yet, the growth of research on the subject beginning circa 1980 was clearly prompted by links drawn between the issue and anthropogenic warming. The agendas of glaciological research and climatic study remained only sporadically integrated for perhaps twenty years thereafter. Even in recent years authors of scientific assessments of anthropogenic climatic change have struggled to incorporate knowledge about WAIS instability into assessment frameworks (O'Reilly et al. 2012). Given the tensions between research agendas, and between research and assessment, it is unsurprising that examinations of the evolution of WAIS research framed exclusively from the perspective of assessment have produced disjointed portraits. As O'Reilly et al point out, uncertainty regarding WAIS actually appeared to increase between the third and fourth IPCC assessments. Five years ago, Vaughan (2008) suggested that the history of WAIS was characterized by strong shifts in "paradigm," which first anticipated, then played down, then again anticipated WAIS disintegration. Oppenheimer et al (2008) have characterized these oscillations in terms of "negative learning." I would suggest that shifting focus between agendas can provide more comprehensible portraits of the evolution of scientists' actual knowledge of WAIS. As the sorts of tensions between agendas that have shaped WAIS research surely have analogies in other areas of climate research, a firmer understanding of the history of that research can yield insight into the more general patterns in which agendas can diverge and converge over time.

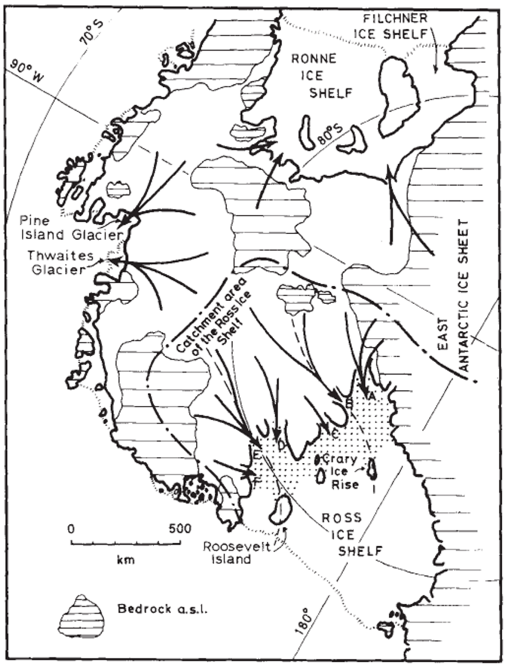

Fig. 1 Map of West Antarctica; A, B, C, D, E, and F, mark the ice streams around the Siple Coast region; “a.s.l.” stands for “above sea level”; light dotted lines indicate the reach of floating ice shelves. Reprinted by permission from Macmillan Publishers Ltd: (Thomas et al. 1979), copyright 1979.

2 Early agendas in WAIS research

Recent historical studies of the expansion of post-World War II field research in the earth sciences (Doel 2003), and in ice-sheet research in particular (Belanger 2006; Naylor et al 2008; Turchetti et al. 2008; Martin-Nielsen 2012, 2013), emphasize that the sorts of large-scale research projects that would later be supported out of environmental interest owed their support to the technological developments and political agendas of the early Cold War. Knowledge of WAIS’s marine topography dates to this period, when Charles Bentley and Ned Ostenso traversed West Antarctica as part of America’s International Geophysical Year research program, taking seismic soundings across West Antarctica’s bed (Bentley and Ostenso 1961; Belanger 2006). However, for fifteen years speculation concerning the glaciological implications of WAIS’s topography was limited to academic theorizing. The increased propensity of such marine ice sheets to disappear was quickly suggested by John Hollin (1962), and Gordon Robin and Raymond Adie (1964). A few years later the glacial geologist John Mercer (1968) suggested that WAIS’s ice might have been the source of higher sea levels during the Sangamon Interglacial 120,000 years ago. He further noted that WAIS might disintegrate in the future due to climatic warming caused by increasing “industrial pollution of the atmosphere,” but this was an offhand remark in an otherwise geological paper.

Early suggestions that WAIS might be innately unstable were soon elaborated upon by Mercer’s colleague at the Ohio State University, the young glaciological researcher Terence

Hughes. Hughes's approach to glaciology is unusual in that it involves synthesizing diverse sets of theory and data into a coherent network of often bold, but ultimately speculative, narratives of natural processes, which are intended to serve as explanations for macro-scale geological phenomena. Hughes has long cultivated a self-image as a radical outsider, priding himself on his "outside-the-box" thinking (Hughes 2008 interview with the author). However, his commitment to finding general explanations for macro-scale phenomena might be more specifically linked to his first exposure to earth science in the form of a "heterodox" theory of ice ages (Hooker 1958), as well as to the prevalence at that time of more legitimate speculations surrounding ice ages (Imbrie and Imbrie 1979), and the then-fresh memory of the spectacular ascendancy of plate tectonics in geology (Oreskes 2001; Hughes 2009).

Hughes's interest in WAIS originally derived from his interest in an ice-age theory proposed by geologist Alex Wilson (1964), suggesting that "surges" of ice from Antarctica could trigger global glaciations. In a series of privately circulated "ISCAP Bulletins" (Ice Stability Coordinated Antarctic Program), also published in the peer-reviewed literature (Hughes 1973, 1975, 1977), Hughes proposed that an intrinsic instability in WAIS might be able to cause such surges. He pointed to evidence submitted by glacial geologists, including Mercer, which indicated that the Ross Sea portion of WAIS had already retreated from the edge of the continental shelf over the previous several thousand years, creating the Ross Ice Shelf as it presently stands. Further, measurements of ice elevation and flow patterns, and the concave profile of the ice sheet, suggested that it had not arrived at a point of dynamic stability. These issues, he urged, required focused field research.

Hughes's arguments concerning WAIS's role in glaciation cycles attracted little notice. However, he was able to repurpose the basic idea when he joined a branch of the large, multi-institutional, National Science Foundation (NSF)-funded CLIMAP (Climate: Long-Range Investigation, Mapping, and Prediction) project at the new Institute for Quaternary Studies at the University of Maine. One of the founding members of the institute, glacial geologist George Denton, had published work on Antarctica's glacial history (Denton et al. 1971), which Hughes had marshaled into his original argument about WAIS. Hughes and Denton, joined by visiting Russian geologist Mikhail Grosswald, posited a "simplification and synthesis" of existing studies of the late-Würm glaciation 18,000 years ago (Hughes et al. 1977). They supposed the existence of a single Arctic ice sheet with marine portions that were buttressed, like present-day WAIS, by floating ice shelves. This configuration allowed them to identify both the source of the stability of the various ice domes in the northern latitudes, and the mechanism of their apparently rapid collapse (Fig 2). This argument not only meshed with Hughes's idea about WAIS instability, it retained his synthetic macro-explanatory approach by purporting to "[harmonise] geological and glaciological problems associated with standard reconstructions."

Given the propensity of grand synthetic approaches to produce suggestions that are both heuristically productive and beyond the pale of ordinary standards of rigor, it is difficult to characterize claims deriving from them in properly scientific terms. The scientific community's usual response to synthetic macro-explanations is not to engage with their overarching logic, but to assess the plausibility of their various component claims as quickly as possible. One of the first figures to respond to Hughes's ideas about WAIS instability was his first mentor in glaciology, the theorist Johannes Weertman. Having already seen fit to apply his analytical tools to Wilson's Antarctic surge theory (Weertman 1966), Weertman undertook a simple theoretical treatment of the forces acting on marine ice sheets, which affirmed the plausibility of their instability (Weertman 1974), and recast Hughes's macro-explanatory agenda into a more straightforward problem of glaciology.

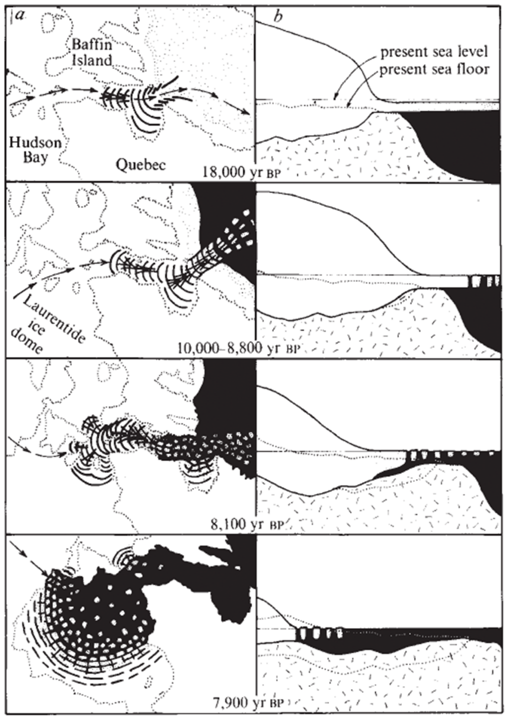

Fig. 2 A diagram of the disintegration of the Laurentide Ice Sheet through its marine portions. Reprinted by permission from Macmillan Publishers Ltd: (Hughes et al. 1977), copyright 1977

3 Glaciological investigations

Marine-ice-sheet disintegration's shift from being a macro-explanation for paleoclimatic phenomena to being a question of glaciology shifted the central focus of research on WAIS from academic theorization to field investigations of the behavior of ice sheets and especially the ice streams that drain them. The shift also set the stage for the proliferation of researchers interested in the subject. Following Weertman's theoretical analysis, the first glaciologist to take up the question of WAIS's stability was Robert Thomas, who was in charge of the glaciological component of the then-ongoing NSF-funded Ross Ice Shelf Geophysical and Glaciological Survey (RIGGS). Because the Ross Ice Shelf represented the largest outlet of ice from WAIS, any evaluation of WAIS's future would necessarily take its behavior into account. Using data from RIGGS and an associated drilling project, Thomas (1976, Thomas and Bentley 1978a, MacAyeal and Thomas 1979) began to address the empirical question of the present-day equilibrium state of the ice shelf. Following RIGGS, he also worked alongside Hughes and Denton at Maine for several years and developed theoretical treatments of the past retreat of marine ice sheets in Antarctica (Thomas and Bentley 1978b) and North America (Thomas 1977). However, this line of research did not flourish more broadly until the initiation of the Siple Coast Project (SCP), which formally lasted from 1983 to 1987.

Where RIGGS had surveyed the Ross Ice Shelf itself, the SCP began a long program of research on the ice streams that drain ice from the interior of WAIS into the ice shelf. One of the central figures in both RIGGS and the SCP was Charles Bentley, who, in the years following his work on the IGY traverses, had become a senior figure in the American polar research community, and served as chairman of the National Research Council's Polar Research Board from 1981 to 1985. Bentley had contributed to Thomas's early work on the prospect of WAIS disintegration, and was certainly aware of Hughes and Denton's arguments concerning the paleoclimatological importance of marine ice sheets. In his mainly positive review (Bentley 1981) of a book they edited about their CLIMAP work (Denton and Hughes 1981), he found heuristic value in their macro-explanation of past deglaciation patterns, since it was impossible to build reasonable models of past glacial behavior out of rudimentary glaciological principles. However, he objected to the book's "failure to distinguish between well-supported statements and those that are speculative," and was unconvinced that a solid case existed for WAIS disintegration. Nevertheless, he was sufficiently interested in the possibility that he switched his own focus from geophysics to glaciology at that time (Bentley 2008 interview with author).

A major goal of the SCP was to move beyond speculation and rudimentary theoretical understanding of marine-ice-sheet behavior through an intensive program of field work, undertaken by three teams. Bentley led one team, which comprised mainly his graduate students (including Richard Alley and Sridhar Anandakrishnan, who would become important figures in the study of ice sheets). The other two SCP teams were led by Ian Whillians, an experienced glaciologist who had studied the interior of WAIS in the 1970s (Whillans 1973), and Robert Bindschadler, who had completed his PhD in 1978 and had recently been placed in charge of a small field glaciology program at NASA. Following the completion of these teams' work in 1987, work on the Siple Coast not only continued but expanded with NSF support. Thus from the mid-1980s through the 1990s, field work relating to the WAIS disintegration problem was characterized mainly by the accumulation of understanding of ice-stream behavior, including discoveries made concerning the nature of the bed beneath ice streams, the friction along their sides, and the ability of ice shelves to impede their flow. These results have been well chronicled in the review literature (Oppenheimer 1998, Alley and Bindschadler 2001).

4 The impetus of anthropogenic warming

To understand the importance attributed to work along the Siple Coast, it is important to understand that the development of this work coincided with the linkage of the problem of WAIS disintegration to a growing interest in anthropogenic climatic change. This link was drawn by the same person who first suggested it in 1968, John Mercer, who now articulated it much more forcefully in a *Nature* paper entitled "West Antarctic ice sheet and CO_2 greenhouse effect: a threat of disaster" (Mercer 1978). That paper received great publicity, as well as attention from the broader scientific community. In 1980, the American Association for the Advancement of Science sponsored a conference at the University of Maine dedicated to the link between WAIS and CO_2-induced warming, which would be followed by many others. The WAIS issue also began to be discussed regularly at conferences dedicated to anthropogenic warming, beginning with one sponsored by the U. S. Department of Energy at Berkeley Springs, West Virginia in 1982.

There can be no question that finding answers to the questions of whether and how fast ice streams could plausibly transport ice from the interior of WAIS is critical to making informed projections about WAIS's future. At the same time, it is important to note that, while research conducted along the Siple Coast was motivated by the problem of possible WAIS disintegration, it was not designed to provide a full assessment of that possibility. For example, the work was not directly coordinated with efforts to assess the overall mass balance of WAIS. For that matter, the work's concentration on the Siple Coast ice streams remained predicated on Hughes's and Weertman's original idea that WAIS might be inherently unstable. Thus little effort was made at that time to investigate means by which WAIS behavior might couple to a rapidly changing climate, such as at the intersection of oceans and ice shelves. This conjunction of WAIS disintegration as a problem requiring assessment with the investigation of the Siple Coast as an agenda relevant to such an assessment was the context in which a new and historically large generation of glaciologists and climate modelers became exposed to WAIS as an object of scientific and policy interest.

5 WAIS and climate modeling in Europe

In the 1980s, computer modelers became increasingly important in developing projections of future climatic change. WAIS behavior began to be modeled within the glaciological community by the long-established glaciologist William Budd and collaborators in Australia, as well as by the younger modelers James Fastook and Craig Lingle in the United States. But perhaps the most influential modelers of ice and climate, coming out of the University of Utrecht, were not centrally concerned with physical glaciology. Johannes Oerlemans received his PhD at Utrecht in 1980, having completed a project that used models to examine the ability of Milankovitch cycles in insolation to account for the geological record of global glaciation and deglaciation (1979, 1980). Remaining at Utrecht, he then began applying his models not only to the paleoclimate, but to CO_2-induced climatic change as well (1982a, 1982b).

Oerlemans's models accounted for the cryosphere's response to climatic change through established mechanisms: precipitation, melting, iceberg calving, lubricated basal sliding, and the depression and rebound of bedrock. He was also aware of the discussions concerning the stability of marine ice sheets, but did not regard the phenomenon as sufficiently established to incorporate into his models. However, his first student, Cornelis van der Veen (1985), did investigate the issue as part of his PhD work. According to van der Veen, the then-prevailing model, developed by Robert Thomas, simply posited the existence of a marine ice sheet and then demonstrated its instability. His model, by contrast, accounted for the ice sheet's initial creation and its subsequent behavior. He also treated the interaction of ice sheets and ice shelves differently, arguing that Thomas's model had contained "a natural built-in instability." Van der Veen's model was itself far from a definitive statement of the behavior of marine ice sheets, but, for him and Oerlemans, by virtue of its improvements over precedent, it did throw the supposition of instability into doubt. As he wrote, "either the formation or shrinkage of a marine ice sheet is not described adequately by the present model, or a marine ice sheet is more stable than is generally believed." Subsequent theoretical models, e.g. (Hindmarsh 1993), would reach similar conclusions about the inadequacies of early models.

Adjudicating whether a physical mechanism is well established is critical in numerical modeling. It protects the integrity of the model by determining whether the model's behavior is forced by an arbitrary input.[1] Oerlemans and van der Veen were especially keen to

[1] Edwards (2010) discusses the history of ideas related to the legitimacy of numerical modeling in much greater depth.

protect the integrity of climate models from the canonization of mechanisms that had potentially been overpublicized on account of their perceived implications for humanity. In their monograph, *Ice sheets and climate* (1984), they pointedly remarked, “The world’s major problem is the arms race, not the next ice age or the possible collapse of the West Antarctic Ice Sheet.” As late as a 1989 paper discussing future sea levels, Oerlemans (1989) referred to the “great deal of public attention [that had] been focused on sea-level rise” in the wake of Mercer’s 1978 warning. Not entirely dismissing the possibility, he observed that there was “a long way to go before we can properly model the marine ice-sheet dynamics.”

In spite of climate modelers’ admissions that their models were incomplete, glaciologists concerned about the future of WAIS had reason to worry that models could marginalize the topic they were busy investigating. In 1990, Philippe Huybrechts, a doctoral collaborator of Oerlemans’s, published results from a three-dimensional, climate-coupled model of ice flow in Antarctica, which continued to indicate WAIS’s stability (Huybrechts 1990, Huybrechts and Oerlemans 1990). Huybrecht’s model was still inhibited by limited computing power and physical understanding from simulating ice-stream behavior. Nevertheless, it set a new standard for sophistication that would prevail for many years. Meanwhile, the first IPCC assessment report was also issued in 1990, with Oerlemans as one of two lead authors of the chapter on sea level rise (Warrick and Oerlemans 1990). The chapter acknowledged the range of available models dealing with WAIS, as well as the empirical studies being done on ice streams, but it was constrained by the IPCC’s terms of reference from evaluating the longer timescales on which any major disintegration might occur. It, therefore, tersely concluded that “future warming should lead to increased [snowfall] accumulation and thus a negative contribution to sea level change [from Antarctica].”

6 Divergent research agendas

In the early 1990s, interest in WAIS was beset by divergences in the various agendas behind that interest. The prospect that WAIS might disintegrate was predicated on the speculative mechanism of marine-ice-sheet instability as a macro-explanation for the paleoclimatic record, as well as on rudimentary glaciological theory. The impetus underlying more widespread concern surrounding the prospect of an imminent disintegration was provided by the link between it and general concern over the near-term consequences of anthropogenic climatic change. However, the climate modelers responsible for assessing those consequences were unconvinced of the plausibility of the phenomenon, and their computer models, in any case, were incapable of simulating it. For their part, glaciologists were aware that whatever risk there might be was tied to phenomena they were yet to discover about the behavior of ice streams. Some among them, such as Charles Bentley, were reluctant to overstate the dangers (Bentley 1997, Bentley interview with author 2008). At the same time, though, unexpected discoveries such as the lubrication of the bed beneath one of the ice streams (Kamb and Engelhardt 1991), or, later, the near-instant responsiveness of an entire ice stream to oceanic tides (Bindschadler et al 2003), tended to heighten glaciologists’ conviction that the uncertainties surrounding their work should not be taken as a license to discount their concerns (Bindschadler and Bentley 2002, Bindschadler interview with author 2009).

Meanwhile, among those with a strong interest in WAIS, the concentration of NSF resources on research along the Siple Coast suggested a growing dominance of one particular agenda. From the perspective of Terence Hughes and George Denton, for instance, the prioritization of that work did not reward what they regarded as their fundamental scientific insight concerning the role of marine ice sheets in the paleoclimate. According to Denton (2008 interview with author), “Whether [WAIS, in particular] actually collapses sometime is an engineering question,” a detail-oriented issue certainly worthy of study, but of comparatively minor importance for general scientific understanding of the earth’s history. According to Robert Thomas (2009 interview with author), who did not actively contribute to WAIS-related research for two decades following his initial contributions in the late 1970s, such long-term projects as the Siple Coast research “tend to become Christmas trees and everyone hangs their pet project onto them...” For this reason, they can become “unfocused,” and cease to make major contributions to the questions they are supposed to resolve. This view would not necessarily be denied by the people who have worked on the Siple Coast. Douglas MacAyeal (2008 interview with author), who worked on RIGGS as a graduate student and later on the Siple Coast, observes that, while it is useful to include the question of WAIS disintegration on grant proposals, his own research agenda mainly follows his glaciological interests. Robert Bindschadler (2009 interview with author), by contrast, has long been deeply motivated by the WAIS disintegration problem, but he, too, acknowledges that it is challenging to ensure that researchers continue to work actively to solve the larger questions that ostensibly drive their work. A clearer articulation of agendas might have made it more apparent what agendas the Siple Coast research program was and was not supposed to serve, and how well it served them.

7 Convergent research agendas

While the divergences in research agendas running through WAIS-related research have been pronounced, their severity should not be overstated. Generally speaking, researchers have been aware of others’ perspectives, and the limitations of their own, which has aided in the reconciliation of agendas. If the concentration of work on the Siple Coast represented the dominance of a particular agenda, the group of people who did that work also served as a nexus where various disciplinary perspectives could be brought together. After the conclusion of the SCP, ice-stream research was coordinated through an annual workshop called the WAIS Initiative. NASA’s Robert Bindschadler (2008 interview with author), who chaired the Initiative for two decades, credits both it and NASA’s and the NSF’s commitment to the integrated research framework of “Earth System Science” for helping to foster more interdisciplinary approaches. Even still, much of the impetus behind the reconciliation of WAIS-related agendas arose from beyond the bounds of the research context in which it initially developed, and had to gain acceptance by established insiders. For instance, in the past ten years attention to WAIS has turned to the rapid drainage of ice not into the Ross Sea, which is likely to remain blocked by the Ross Ice Shelf, but the Amundsen Sea through the large Thwaites and Pine Island Glaciers. The prospect for drainage through those outlets was raised early by Hughes (1981), whose perspective held that the question was not whether WAIS would disintegrate, but what was to stop it from doing so. Yet logistical difficulties kept the region from being investigated closely. The first evidence of rapid ice drawdown in that sector of WAIS was provided in the late 1990s by analysis of data gathered over several years by the European Remote-Sensing Satellites. That work was not actually motivated by any strong prior interest in the WAIS issue (Wingham et al 1998, Rignot 1998, Wingham personal communication, Rignot personal communication). However, by the early 2000s the importance of the results had been recognized by the WAIS-research community.

In the past ten to fifteen years, disciplinary agendas have been more actively reconciled, and methodological weaknesses limiting the ability of one agenda to constructively contribute to others have begun to be addressed. More sophisticated glaciological theories, e.g. (Schoof 2007), have clarified physical problems that raised questions about the validity of early models of marine-ice-sheet behavior. Numerical climate models have been developed, e.g. (Pattyn 2003), which can couple small-scale ice-stream behavior to large-scale changes in climate. Oceanographic studies of the interaction of ocean waters with ice shelves and the grounding line of WAIS have become an integral part of field research programs that tie the question of WAIS instability more closely to climate research. Institutionally, the increased prominence of assessment exercises, particularly the IPCC, has created an impetus to ensure that results arising out of various research agendas are capable of contributing to assessment frameworks. Some organizations, notably the British Antarctic Survey, manage the various types of research done under their auspices so that they actively contribute to pressing questions, such as assessing future rises in sea level. This recent phase of the history of WAIS research should be subjected to more intensive historical analysis.

These observations should not be construed as an unqualified endorsement of the virtues of the present moment. No doubt in the future it will be easier to judge just how successful present reconciliations of various research agendas have been. Further, there exists the danger that benefits accruing from the independence of research agendas could be at risk by pressing them increasingly into the service of the overarching agenda of assessment. Nevertheless, it would be difficult to deny that once-divergent agendas have been brought together in ways that allow them to function more complementarily, and that permit deeper and broader discussions of the prospect of WAIS disintegration to be developed.

8 History and the articulation of agendas

Using historical research to understand research agendas can substantially clarify the nature of scientific claims, and disagreements over them, by placing them in the context of the goals they were initially intended to serve. For instance, understanding the motivations behind the early claims of scientists such as John Mercer and Terence Hughes that WAIS might be unstable would help to clarify the degree to which the apparent accord between their work and present understanding is simply fortuitous, and the degree to which it is based on some ill-articulated intellectual virtue in their work. By the same token, if the limitations of the research agendas their work served had been better articulated originally, their speculations might have been treated more circumspectly by the science journalists and the public press. In turn, it might have been less necessary to treat the prospect of WAIS disintegration with the guarded conservatism that a number of scientists regarded as a necessary countermeasure to perceived alarmism. Moreover, articulating research agendas would help elucidate the rationales that initially informed research policy, such as the Siple Coast Project, and help determine whether and when those rationales require rebalancing. More positively, articulating agendas would clarify what concrete steps have already been taken to overcome divergences between agendas, which could serve as a model to researchers in other fields of climate studies facing analogous situations.

If the articulation of research agendas is a virtue, the question remains as to what form that articulation should take. A simple strategy would be to produce occasional histories comparing agendas and discussing the evolution of their interrelations. These contributions could be placed in publications, such as the present one, where they would visible to the broader specialist audiences who could most benefit from them. Such publications can discuss research results, but their primary function would be to make explicit the goals and assumptions of research that would otherwise remain tacit and so be subject to misunderstanding. Such publications would then no doubt be met with clarifying responses. In addition to whatever general edifying effects such discussions may have on the decision-making of research managers, the added clarity they can lend to discussions of those decisions could inform statements concerning research strategy, instruct students how better to navigate the scientific literature, and deepen engagement between specialist and non-specialist audiences.

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