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Climate state: Science-state struggles and the formation of climate science in the US from the 1930s to 1960s

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Abstract

This article has two aims: first, to understand the co-production of climate science and the state, and second, to provide a test case for Pierre Bourdieu's field theory. To these ends, the article reconstructs the historical formation of a US climate science field, with an analytic focus on inter-field dynamics and heterogeneous networking practices. Drawing from primary- and secondary-source materials, the historical analysis focuses on relations between scientists and state actors from the 1930s to the 1960s. The account shows how actors with positions linking scientific and bureaucratic fields constructed critical nodes and 'hinges' that co-produced war-making and state expansion on the one hand, and a relatively autonomous climate science field on the other. The analysis explains the emergence of climate science by focusing on the WWII-era transformation of meteorology and oceanography into distinct disciplines, the emergence of 'basic' research as a central principle of post-war government, and the formation of a climate science field by the 1960s centered on computerized modeling and populated by an interdisciplinary scientific elite. The article concludes by indicating how these processes led to the subsequent development of climate change as a science–state conundrum that has reorganized the climate science field in recent decades.

Keywords

Bourdieu, climate change, co-production, Cold War, scientific fields

Introduction

With climate-change researchers consistently facing organized efforts to polarize reception of their work (Farrell, 2016), accounts of climate science tend to sharpen boundary-work

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among those involved, emphasizing tensions between scientists and policymakers as a matter of science versus politics and ideology. In a world where some crudely decry climate science as ‘unsettled’, climate and social scientists have responded in defense of truth, objectivity, and consensus, and historical accounts have followed suit by establishing that climate change research emerged as a cumulative development of facts, discoveries and technological innovation (Bolin, 2007; Le Treut et al., 2007; Weart, 2008). Yet by reifying distinctions between truth and politics, this narrative paints an inaccurate picture of the historical context in which climate science emerged, and it fails to adequately consider what Jasanoff (2004) calls ‘co-production’, in the case at hand, of political and climate science worlds.

This study both draws upon diverse work in STS that problematizes the construction of climate change knowledge (e.g., Demeritt, 2001; Edwards, 2010; Livingstone, 2012) and develops a different historical account. By critically engaging Bourdieu’s approach to ‘fields’, I analyze climate knowledge as a formation that emerged through struggles that linked scientific fields and the state prior to the partisan and regulatory battles that have raged since the 1980s – the decade when many claim climate change began ‘breaking into politics’ (Weart, 2008: 138). The emergence of what we can call a *climate state*, organized around governmental apprehension of climate dynamics, preceded the 1980s. My analysis conceptualizes relations between state actors and scientists from the 1930s to the 1960s as *matched struggles*, where scientific and government actors have linked conflicts concerning how to integrate atmospheric and related research into war- and state-making. Tracing these struggles, the paper helps historically situate subsequent developments in the climate state. By conceptualizing climate state formation as a matter of matched struggles, the article develops conceptual resources for studying the emergence and co-production of scientific fields and the state in other historical and contemporary contexts.

Into the 1960s, atmospheric scientists were routinely involved in weather and climate ‘modification’, indicating how efforts to understand atmospheric dynamics were frequently shaped by goals to control them technically (Harper, 2017). Yet by having secured jurisdiction within the state and gaining a degree of autonomy, the post-1970 climate science field – increasingly organized around anthropogenic climate change – instigated a *state–science conundrum* among scientists and state actors. In other words, as I will argue, mid-twentieth century efforts to govern climate helped to produce a scientific field that in turn undermined the prior relationship between climate knowledge and state power.

Theoretical and analytic approach

Bourdieu and scientific fields

Field theory provides one way to trace social action among scientists and state actors over time. Bourdieu conceptualizes a field as a patterned space of social relationships and individual positions variously oriented to rules of practice, which become embodied in individuals’ *habitus*. Social action within any field involves participants accumulating a field-specific form of capital, which functions through accumulation and exchange with other forms of capital. Bourdieu conceptualizes a field as ‘autonomous’ insofar as field-specific interests form a hierarchical order that produces a clear set of positions and orientations. Necessarily, autonomy is always related to a broader space of multiple and often unsettled fields. Even autonomous fields therefore contain axes of struggles over field-specific versus other forms of capital. For example, Bourdieu (1996a) emphasizes that the field of artistic production emerged through the opposition of the cultural (‘art for art’s sake’) and economic (‘art for money’). Heteronomous, or peripherally-oriented, positions and action may therefore inspire conflict, or ‘boundary-work’ in Gieryn’s (1983) terms, which aims to differentiate legitimate from illegitimate production.

By situating field autonomy within the context of inter-field dynamics, I trace the emergence of field-specific struggles within climate science in relation to other fields, especially the state (addressed below as a ‘bureaucratic field’). As Albert and Kleinman (2011) and Camic (2011: 284–285) suggest, inter-field relations remain an under-developed focus of existing scholarship on scientific fields. Scholars in STS have recently begun drawing from Bourdieu’s concepts to understand scientific controversies and credibility, specifically using his concept of ‘scientific capital’ to explain power struggles in and over particular research fields, sites, or laboratories (Hess, 2006, 2011; Hong, 2008; Panofsky, 2011; Ruget, 2002). Such studies, however, have mostly treated fields as relatively isolated spaces, rather than contingent on inter-field dynamics. It remains to be seen how such analyses might intersect with others showing science and the state to be co-produced.

Bourdieu’s own work does not provide a sufficient account of relations between scientific and other fields. Bourdieu, (1975, 1991, 2004) broadly theorizes the scientific field as a social struggle over objectivity, emphasizing that this field, when autonomous, can transcend its field-specific ‘game’ to advance the ‘progress of reason’. Work in STS mostly has either rejected his general view of scientific practice (Mialet, 2003) or else narrowly engaged the concept of scientific capital, often assuming, rather than tracing historically, the autonomization process (e.g. McGuire, 2016). Thus, a significant way to productively formulate field-theoretical accounts of science is to focus on inter-field dynamics as opposed to a monolithic scientific field or simply *intra*-field processes. Indeed, scholars studying expertise outside the natural sciences have recently begun such a movement by conceptualizing ‘spaces between fields’ occupied, for example, by knowledge brokers or expert interventions in formal politics (Eyal, 2013; Medvetz, 2012). Approaches emphasizing field structure and political interventions (Dezalay and Garth, 2002; Mudge and Vauchez, 2012) argue that inter-field dynamics structure actors’ career trajectories and modes of expert intervention. By bringing inter-field relationality to the study of science, in particular, Panofsky (2011) argues, we ‘might put to rest the assumption that field theory only applies to the kind of well-bounded, highly integrated, autonomous ideal-typical fields that science studies are skeptical even exist’ (p. 313).

In light of these central issues of field autonomy and power in science, climate science – as interdisciplinary, politically intertwined, and transected by wider networking practices – provides a test case through which STS scholars may further evaluate the import of field theory to the study of science. Towards this end, my analysis develops concepts organized around a theory of matched struggles. Struggles, in this conceptualization, are either strategies for accumulating and valuating field-specific capital or are inter-field strategies that integrate networks of practices, objects and goals. Logically, struggles may take place within specific fields, through inter-field dynamics or through heterogenous network-building strategies (Law, 1992) apart from delineated fields. Struggles are *matched* when actors with previously unrelated, undeveloped, or even contradictory concerns construct 'hinges' between fields or assemble critical nodes to link networks.

Abbott defines 'hinges' as issues that secure professional jurisdictions by providing distinct yet compatible rewards for involved actors. For example, he (Abbott, 2005) analyzes medical licensing as a 'hinge' (p. 255) between the medical profession and the state that emerged by providing 'payoff not only for either the doctors or their "irregular" competitors, but also for some political group against its political competitors'. Bourdieu (1996a) develops a similar term, 'homologies', to designate matching positions across different fields that benefit from alliance and exchange. In a different vein of research, by symmetrically integrating nonhuman action, actor-network theory (ANT) argues that power struggles proceed through the enrolment of objects and actions towards particular strategies. In this analysis, hybrid actors order diverse critical nodes among objects, people and practices. Field theory does not account for such processes, yet when actors order nodes in this way, it may help stabilize a distinct logic and set of orientations within a field. In my study here, *critical nodes* therefore denote strategic networking of humans and nonhumans, *homologies* relate to the positions of individuals, and *hinges* are those issues sustained through multiple and often contradictory meanings for those involved. A conceptualization of matched struggles integrates the concepts – hinge, homology, and critical node – as a basis for clarifying the import of field theory in a world transected by inter-field and network processes that confound Bourdieu's own account of 'the scientific field' as a social world-unto-itself. It also provides a way to reconstruct how the climate science field emerged as an autonomous source of power in virtue of the embeddedness of this field in broader networks of power, especially within the state.

Climate knowledge and state/science co-production

Historians of science have addressed the relationship between climate research and the state primarily by focusing on patronage and particular administrations, with a special emphasis on the Cold War (Doel and Harper, 2006; Dörries, 2011; Hart and Victor, 1993; Weart, 1997). Such scholarship emphasizes that Cold War military and technocratic priorities transformed American science (Leslie, 1993; Wang, 1999). In particular, military patronage affected directions in geosciences (Doel, 2003; Hacker, 2000).

Sociological and STS theories of state formation extend analysis of scientific fields beyond patronage sources, political administrations or single historical periods. Jasanoff (2004) and Shapin and Schaffer (1985) argue that science, state power and modern social order are intertwined over the *longue durée*. Mukerji (2009) and Rueschemeyer and Skocpol (1996) show that modern states developed alongside forms of knowledge that constituted state intervention in society. State power, as Scott (1998) and Carroll (2012) argue, forms a governable world only insofar as state actors assemble or make 'legible' complex geographies, environments, and social practices.

Field theory provides an entry point into analyzing legibility in state-making. Bourdieu (1994, 2014) argues that the state, as a 'bureaucratic field', has two features. First, it is an 'instrument of unification', formalizing a given social order though a monopoly of coercive and symbolic power (Bourdieu, 2014: 226; Loveman, 2005). Second, and unlike Scott, Bourdieu views the state as organizing a 'division of labor of domination' both between formal government organizations and within society writ large (Bourdieu, 1996b: 165). As a field, the state involves struggles over 'bureaucratic capital', the capacity to claim to govern correctly. The accumulation and exchange of this capital valorizes a particular distribution of bureaucratic authority, backed explicitly by policy or law and, at least implicitly, by legitimated performance of national interest.

Although providing a way to investigate the distribution of power within states, Bourdieu's theory of the bureaucratic field, like that of the scientific field, inadequately considers science–state relations. Despite conceptualizing science as the 'arbitration of the real' (Bourdieu, 2004: 70) and the state as the 'monopolization of the universal' (Bourdieu, 2014: 99), Bourdieu's theory evaluates neither the relationship between the 'real' and the 'universal' nor the related struggles to monopolize them. Bourdieu also does not trace the significance of natural environments to securing state power and bureaucratic capital. The case of climate science shows, however, that the state – as a field of administrative action and as a territorialized social order – increasingly stabilizes through harnessing the epistemic authority and technical capacity of science to form governable environments. In other words, science is critical to extending administrative reach and symbolic power, especially power to designate the terms of security and progress amidst uncertain futures and public threats such as climate change. In these terms, the state clearly fails to monopolize power as a 'meta field', as Bourdieu (2014: 222) claims, for scientific practices have the capacity to stabilize or destabilize the value of bureaucratic capital. The field approach utilized here therefore views the state as a bureaucratic field that is co-produced with climate science. In other words, state-making in part proceeds through the matched struggles with scientists and their networks that in turn make the state a terrain of their struggles.

Data and historical periodization

In order to reconstruct the matched struggles that characterize the formation of the climate science field, I draw on diverse data, including biographical material (memoirs, speeches, letters, and papers), scientific papers and professional proceedings, government reports and Congressional hearings, and existing histories of climate-related sciences. I employ scientific texts to reconstruct a historical understanding of developments in meteorological analysis, atmospheric modeling and associated technologies. Government and organizational reports, alongside secondary historical texts, provide insights into the institutional structure of climate research and the primary logics through which it expanded. Biographical material yields an understanding of how actors' strategies, discourses and practices overlapped along individual career trajectories. To be clear, using biographical narratives to anchor the account assumes not that historical change is a matter of individual ingenuity but rather that fields may be traced through biographical analysis. Table 1 presents my periodization of science–state relations discussed in greater detail in the analysis. The table offers both a convenient reference point and a tabular summary of how the emergence of climate science can be explained in reference to the co-production of scientific fields and the state.

Table 1. Periodization of climate science–state relations.

Time period	Stage of science–state relation	Key features	Major outcomes
c. 1890–1930	1. Meteorology as 'weather services'	US Weather Bureau, private weather services Descriptive climatology, synoptic forecasting	Climate research weakly organized
1930s–1940s	2. The academic turn and World War II	Bergen-school meteorology imported First meteorology departments in US universities Military meteorology	Disciplinary boundaries in meteorology and oceanography War-time 'science policy' widely engages geophysical research
1950s–1960s	3. National security and climate science autonomization	Numerical Weather Prediction International Geophysical Year Weather modification Institutionalization of climate dynamics research	'Basic science' component of state-making First wave of climate scientists Boundary-making in climate science
1970s-present	4. Climate science and power *over* the state?	General Circulation Modeling Global warming	Science–state conundrum Fragmentation of climate expertise Securitization of climate futures

Disciplining the atmosphere? American meteorology and state-making

The origins of climate science can be traced to state–science dynamics that first emerge in the changing meteorological profession, roughly from 1930 to 1950. Prior to the 1930s, questions about climate dynamics, including anthropogenic global warming, had been raised by a dispersed array of individuals with no organizational center. As Koelsch (1996) summarizes, studies of climate in the nineteenth century 'were stillborn' (p. 529). Only with hindsight have scientists and historians argued that these early scientists' work initiated a shared trajectory (Bolin, 2007; Weart, 2008). 'Climatology' and 'climate science' are today often discussed interchangeably (Barry, 2015; Nuccitelli, 2015), yet through the mid-twentieth century climatology remained rooted in descriptive statistical methods that generally treated climate as a normalized, relatively static characteristic of a given area. The topic of climate was basically a subspecialty among geographers and a subordinated concern among meteorologists (Bryson, 1997; Landsberg, 1941; Leighly, 1954). Drawing boundaries between 'climate science' and 'climatology', for example, meteorologist Peter Lamb (2002) recollected that the new 'climate science', largely advanced by meteorologists, was nothing short of a 'scientific revolution' (p. 4). By expanding the ways in which researchers constructed climate, the 'revolutionaries', Lamb concluded, 'rescued the moribund "climatology" of the first two-thirds of the twentieth century'. It is such developments in the scientific status of climate dynamics and the professional location of its 'revolutionary' spokespersons that makes for the boundary-making I now trace, initially through meteorology.

Carl-Gustav Rossby: Overturning American meteorology

Atmospheric scientists (Bolin, 1959, 1999; Phillips, 1998) and historians (Cox, 2002: 179; Harper, 2008) identify the Swedish scientist Carl-Gustav Rossby (1898–1957) as a founder of modern American meteorology, carrying Scandinavian-based developments in meteorology, often ascribed to Vilhelm Bjerknes, to the US (Friedman, 1989). We can explore Rossby's career trajectory to see how a new form of atmospheric research emerged because he was able to successfully match struggles within meteorology to those of state actors, especially around the time of WWII.

After completing a Norwegian college education in 1918, Rossby travelled to Bergen, where he studied under Bjerknes at the Geophysical Institute. There, researchers developed theories of atmospheric patterns, especially 'polar fronts', using the novel method of air-mass analysis, rooted in Bjerknes's 'primitive equations' of atmospheric motion (Friedman, 1989). Particularly in Norway, meteorology was a prestigious scientific vocation based in state-supported weather services and university-based research programs (Friedman, 1989; Phillips, 1998). As meteorologist Horace Byers (1960) remarked, 'new characteristics of the atmosphere were revealed nearly every day'.

Across the Atlantic, US meteorology lacked such academic or disciplinary loci (see 'Stage 1' in Table 1). Climate research remained strongly tied to regional agricultural interests. Few options for careers, training, or credentialing in atmospheric research existed outside of the Weather Bureau or private weather services. In these circumstances, Rossby arrived from Norway in 1926 with a fellowship to work at the Bureau. In 1927, a conflict between Rossby and Bureau personnel prompted him to leave for MIT, where he organized the first US meteorology department, in 1928. Rossby had allegedly frustrated Bureau superiors by providing aviator Charles Lindberg with an unauthorized weather forecast using air-mass analysis. This Bergen method conflicted with Bureau practices, derided by meteorologists a decade later as amounting to a 'guessing science' (Koelsch, 1996; Rossby, 1934).[1]

By attempting to affirm the legitimacy both of the imported Bergen meteorologists' methods and of his own training, Rossby adopted, in Bourdieu's (1975) terms, a 'subversion strategy' (p. 31) within meteorology. At the time, other practicing meteorologists were content with 'conservation strategies' that valorized atmospheric knowledge established through conventionalized means. Their typical accounts of weather patterns consisted of mapping observations and drawing statistical probabilities based on forecasters' interpretation of past and current maps. They often lacked college degrees, and they gained forecasting skills primarily through apprenticeship (Harper, 2008). US academic scientists at the time, compared with those in Bergen, had less contact with forecasters or climatologists, a situation that Jerome Namias, recalling his position as a 'salesman for the Bergen School', interpreted as 'scientific backwardness' only to be overcome by geophysics-trained 'upstarts' located in a 'hotbed of resistance' (Namias, 1983: 746, 734, 741). Under these circumstances, to secure a fragile monopoly in meteorology, senior Bureau meteorologists shunned oppositional loci of professional meteorological practice.

By the beginning of WWII, Bureau meteorologists remained insular in organization and ambiguous in epistemic authority concerning atmospheric dynamics, thus proving weak relative to the Bergen-school meteorologists, who had already gained access to university departments and administrators better positioned to train military meteorologists. Bureau-based meteorological practice failed to provide officials with convincing promises of better forecasts. But for military officials building an aggressive aerial military strategy in uncharted areas of the globe, progress in meteorology was an urgent, high-stakes issue.

Mobilizing meteorology

During the 1930s and 1940s (see Stage 2 of Table 1), meteorologists and state officials followed one another into the messiness of war, transforming the relationship between meteorology and the bureaucratic field. As Mitchell (1998) argues in the case of economics and Latour (1988) for microbiology, scientists often gain power by enrolling diversely interested actors and attaching them to scientists' professed ability to solve *their* problems. In the case of military meteorology, hybrid actors such as Rossby adeptly solved military problems while also institutionalizing their visions for meteorology as a geophysical discipline.

As a first issue for military meteorology, worldwide aviation in WWII required new forms of meteorological knowledge, including high-altitude climatology and detailed predictions of precipitation, winds, cloud cover, and other phenomena (Bates and Fuller, 1986). The geopolitical and military struggle to wage air-based missions in new battle scenarios matched meteorologists' goals of studying atmospheric circulation dynamics, especially in the upper atmosphere and in 'new' areas of the globe, including what Byers (1960) labeled 'the neglected tropics' (p. 267). In addition, mobilizing meteorology simply called for an enormous increase in practicing meteorologists.

The issue of how to establish meteorological training therefore became a 'hinge' between war planners and those seeking to transform meteorology into an academic geophysical discipline. During WWII, between 7,000 and 10,000 Americans were trained as meteorologists and another 20,000 as observers and technicians (Koelsch, 1996). Harper (2008) calculates a 1500% growth in meteorologists from 1941 to 1945. UCLA, the University of Chicago, Caltech, MIT and NYU emerged as the 'Big Five' institutions for meteorological training (Allen, 2001; Byers et al., 1946). Scandinavians, who taught Bergen School theories and methods, directed all programs other than Caltech's. Rossby, the first established Bergen meteorologist in the US, often facilitated the recruitment of Norwegian colleagues to these programs.

The meteorologists, methods, and training programs that Rossby assembled became critical nodes within 'military meteorology', which had expansive effects on meteorological training. Rossby's path-breaking MIT program was replicated first in 1940 at the University of Chicago, which established a formal department in 1942 under direction of Rossby's MIT student Horace Byers. Harry Wexler, like others of Rossby's MIT students, worked as a military meteorologist, then taught at the Chicago department; he was among the first at the Weather Bureau with a PhD in meteorology. Helmut Landsberg also worked with Rossby at Chicago on military training programs while seeking to elaborate a geophysical basis for a ‘physical’ (as opposed to descriptive) climatology (Landsberg 1941).

The network of department participants forged close links between state efforts to integrate meteorology into war-making and a new, university-based science (Byers, 1970). Especially through the University Meteorological Committee that organized military training programs beginning in 1940, Rossby and other European meteorologists (many of whom had fled to the US in the late 1930s) helped to solidify a military-academic-meteorology complex. Professional training and instruction, war-time data collection, and funding contracts could therefore be tailored to the directors’ visions of meteorology.[2]

Rossby became a central spokesperson for meteorology not only by importing new forecasting and training techniques, but also by engaging and translating the interests of state officials. Especially significant was Rossby’s relationship to Francis Reichelderfer, who became the Weather Bureau Chief in 1938. Previously a Navy officer, Reichelderfer had also helped to organize the initial military training programs at MIT. Reichelderfer and Rossby held homologous positions within the state bureaucracy and academic meteorology, respectively. Reichelderfer invested his career in reorganizing the military weather services (Namias, 1991). Rossby, on the other hand, stood to maintain a dominant position in the meteorological field if war could provide a means of expanding Bergen-school, academic meteorology. Their matched struggles – Reichelderfer against existing military and Bureau organization, and Rossby in support of an academically-centered discipline – converged, thereby transforming American meteorology and its place in American government.

These matched struggles co-produced a state territoriality that extended vertically into the atmosphere. Wartime meteorology also effectively altered the bureaucratic field by establishing a basis of power in atmospheric knowledge. Bergen-school meteorology linked science and the state insofar as its tools and organizers helped to make a world that, to be governable, required meteorological capacity to translate otherwise unwieldy atmospheric phenomena and potential recruits into a stable network of training programs, standard methods, and, ultimately, weather forecasts. In addition, the field-struggle waged by Rossby and his colleagues succeeded by strategically organizing these new meteorological practices within university programs that could then reproduce efforts to study atmospheric dynamics utilizing mathematics, physical principles and, later, digital computing.

Postwar meteorology: State–science co-production through Numerical Weather Prediction

As WWII came to a close, a central struggle that marked the bureaucratic field involved defining and institutionalizing national scientific dominance (Steelman, 1947). Science remained ‘the endless frontier’, as Vannevar Bush (1945) famously declared. Scientists and policy-makers succeeded in organizing that frontier in government through a discourse of ‘basic science’, constructed in opposition to both ‘pure’ science allegedly trapped in abstractions and 'applied' science of technical development (Bush, 1945; Calvert, 2006; Eisenhower, 1954). Basic science was an important, novel formation in the bureaucratic field because it invested the future of government directly in the work of scientists.

The rise of basic research, still embroiled in a vacillating politics of federal science policy, allowed elite meteorologists to settle the 'endless frontier' in a way that promised relevance to national security while also settling the boundaries of their discipline (Needell, 2000; Sapolsky, 1990). Rossby and his colleagues feared that postwar meteorology might fall back into being a practice of 'amateurs', rather than a professional one rooted in scientific progress. As President of the American Meteorological Society (AMS) from 1944 to 1945, Rossby undertook reform efforts that reveal his strategic boundary-work aimed at securing his vision of the profession. Before WWII, the AMS was open to anyone who paid $3.50 annual dues (Harper, 2008: 84). Rossby called the AMS 'the National Geographic Society of meteorology', suggesting it was problematically geared towards amateurs and therefore unfit to serve as the organization for a more professionalized field (quoted in Harper, 2008: 85). In 1944, Rossby reorganized AMS publications, especially by founding the *Journal of Meteorology* to advance meteorology's reputation as a bounded science.

Boundary-making in meteorology also developed within the Weather Bureau insofar as meteorologists lamented that the Bureau was suffering an acute 'personnel problem' because of conflicts between senior officials and the new, academically-trained recruits (Advisory Committee on Weather Services, 1953). In effect, the kind of meteorology that initially made Rossby, as Byers (1960) recollected, '*persona non grata* at the Weather Bureau' in 1927 had by 1945 become dominant within many university departments and weather services.

To maintain atmospheric research as a critical node between postwar administrators and meteorologists, however, Rossby and colleagues retained a focus on weather forecasting linked to emerging military and commercial priorities, especially in aviation. In the late 1940s, Rossby and colleagues' research increasingly turned towards Numerical Weather Prediction (NWP), combining developments in atmospheric fluid-dynamics theory, mathematics, and digital computing (Harper, 2008; Nebeker, 1995). By tying progress in forecasting to physical theory as well as mathematics and computing, and centering meteorology in specialized university research programs, NWP assembled critical resources that later would become central to climate modeling and research efforts.

In 1946, Rossby and mathematician John von Neumann negotiated a contract, administered by the newly organized Office of Naval Research (ONR), for what became called the Meteorology Project. This project held different meanings and goals for various participants, and succeeded to the degree that the meanings supporting weather prediction became a hinge between them. For Rossby, the project provided inroads into what he considered the central problem of meteorology – 'general atmospheric circulation' – which entailed representing atmospheric dynamics in a system of mathematical equations (Rossby, 1959). For government approval, securing accurate, longer-range forecasts was the major justification. Von Neumann, however, was most interested in developing an electronic computer, because the differential equations that meteorologists were formulating matched his technical goals in computing (Harper, 2003). Von Neumann also emphasized 'weather modification' as an important aspect of NWP scientists' promises.

Overall, by the early 1950s, the multiple but compatible meanings of 'weather prediction' facilitated the technical apparatus that became numerical models. As a critical node, NWP further reorganized the field of meteorology and durably linked variously-situated actors such that NWP research could survive the decade it took for its methods to become operational in routine forecasting.

NWP is well documented as having transformed weather forecasting (Harper, 2008; Nebeker, 1995). Most important for the present analysis is how NWP and the broader transformation of meteorology through WWII affected the genesis of a climate science field. Scientists during the war clearly began to place new stakes in the study of physical atmospheric dynamics. Through NWP efforts, the atmosphere became a new kind of object for meteorologists. It became a global physical system that could be rendered calculable, in addition to being observed and mapped. Although NWP did not run climate change models at the time, meteorologists, and those recruited from computing, mathematics and physics, organized an epistemic and institutional basis upon which competing atmospheric models, climate theories and social positions could be built.

Several counterfactuals suggest that alternative possibilities of climate science became foreclosed in this period. First, NWP meteorologists faced the challenge of making equations of atmospheric processes compatible with complex, delicate computing technologies (Charney et al., 1950; Haigh et al., 2016). Such material struggles were only later contained, with relative success, within atmospheric models. A parallel struggle involved securing political justifications for meteorologists' ambitious research in its early stages. In particular, the link between NWP and weather control remained a central logic for government support of 'basic' atmospheric research into the 1960s, especially because of von Neumann's influence (Fleming, 2010; Harper, 2017). In the last publication of his life, Rossby (1959: 50) favorably reviewed von Neumann's vision of weather control as a central component of the discipline of meteorology. Goals of modifying atmospheric processes, by providing additional meaning behind NWP, sustained weather prediction as a hinge and shaped the possibilities for atmospheric research, despite failures in modification programs.[3]

To take a second counterfactual, the academic turn during WWII could have failed to build close ties to the postwar discourse around basic science. Had meteorologists not forged new critical nodes apart from war-making, the science might have failed to address distinctive problems later critical to a climate science field, especially regarding general circulation models (GCMs, addressed below). Instead, beginning in this period, meteorologists succeeded in expanding the bureaucratic field to embrace their research goals by promoting the benefits of weather prediction (and control).

Success in altering the bureaucratic field often proves critical to the formation of scientific fields. Research on the formation of disciplinary economics shows that one of its basic achievements was to reimagine the world in economic terms and to make that vision effective by building jurisdiction inside the state. Just as 'the atmosphere' became something different in meteorology, Mitchell (1998) argues that economists succeeded from the 1930s through the 1950s in constructing 'the economy' via 'a new language in which the nation-state could speak for itself and imagine its existence as something natural [and] subject to political management.' (p. 90) Economists were thus able to construct a world in which it was taken for granted that government required their expertise.

Scientific success, whether meteorological or economic, may depend on how struggles within a field can produce effects within the state.

As a third counterfactual, meteorologists could have failed in borrowing from – and building critical nodes with – other sciences. Meta-theoretical and methodological innovations are particularly relevant boundary objects that link scientific fields (Fujimura, 1992). Again, economics is a telling comparison. It is unclear how 'the economy' could become an object of science and government if the discipline had not initially borrowed the methods of physicists in the 1870s (Mirowski, 1989), developed econometric analysis in the 1930s (Mitchell, 1998), and integrated mathematics and computing thereafter. Similarly, in meteorology, forecasts until the 1930s had little to do with existing disciplines. The academic turn opened the door for scientists in other fields to recognize meteorologists in virtue of how they represented atmospheric dynamics in the languages of mathematics and physics. Had physicists been able to continue to write off meteorological practice as unscientific, or had atmospheric analysts not quantified their work in numerical models, it is unclear how a field of interdisciplinary actors could or would have invested in later decades such immense financial and personnel resources in explaining atmospheric circulation and climate dynamics.

Overall, the hinges, nodes and homologies that matched the struggles of meteorologists with those of state actors around WWII effectively transformed the social and epistemic foundations of atmospheric science. Rossby's hybrid position helped to link the tools and logics of state agencies with those of academic science. Military meteorological training and NWP linked academic meteorologists to war-planners, generating opportunities for all involved. For military officials, battles could be won. For others, accurate forecasts and weather modification could secure a more governable future. For leading scientists, training and numerical modelling formed a new 'climate' for their work.

Making waves: From oceanographic to climate state

Climate science is an interdisciplinary field, marked by direct appeals to cross-disciplinary engagement (Schneider, 1977; Weart, 2013). Analysis of oceanography over roughly the same period as that addressed above (Stage 2 in Table 1) shows how it is not possible to reduce the nodes and hinges that co-produced war-related interests and climate science to developments in meteorology. The significance of geoscience–state relations to climate science is especially legible through developments in oceanography. Oceanographer Roger Revelle (1909–1991), who is at the center of the following narrative, helped pioneer physical oceanography in the US, integrate oceanic and atmospheric research, and later construct 'the greenhouse effect' as a public issue in the 1970s. In parallel with Rossby, Revelle occupied a distinct position from which to shape academic science, the postwar science policy bureaucracy, and the Navy. Along with meteorology, the matched struggles among oceanographers and state actors helped form the social and epistemic basis for a climate science field.

Revelle is a towering figure in US oceanography and climate science, credited with numerous major discoveries (Malone et al., 1998; Morgan and Morgan, 1996). From the mid-1930s to 1950, he was variously an overlooked researcher aboard Navy ships, a Navy Commander, and a director of classified weapons research and oceanic expeditions. During WWII, Revelle negotiated contracts for Scripps Institution of Oceanography (which he later directed) through the federal National Defense Research Committee (NDRC) and the Navy. In 1942, the Navy Bureau of Ships charged Revelle with 'formulating a wide-ranging program in oceanographic research applied to wartime needs and translation of the results into naval terms,' chiefly sonar techniques and weaponry (Malone et al., 1998: 6). Oceanography thus developed through war-making, reflected in drastic changes at Scripps in the early 1940s, when, as Rainger (2001: 342) calculates, half of each incoming class were uniformed cadets.

Revelle's involvement in establishing oceanographic research was a hinge to war-making that matched a particular set of struggles within the bureaucratic field over how to integrate academic research in government. Because proposals for a national-level science program remained unsettled and many of the technical aspects of war-making were unevenly distributed across formal military branches and among researchers, military contracts before and during WWII allowed Revelle and others like him to benefit from military–academic alliances (Hamblin, 2005: xx, 57). The bureaucratic field structure aligned with Revelle's strategies within oceanography: Uniquely, he insistently promoted Naval research contracts as grounds for scientific progress, even as colleagues shunned military surveillance and related strictures (Mukerji, 1989; Wang, 1999). After WWII, the Navy was sharply divided between those who favored in-house research focused on direct military operations and those favoring university-contracted 'basic' science. By 1946, the latter became dominant through the Office of Naval Research, which Sapolsky (1990) and Hamblin (2005) describe as an outcome of Navy officials' rivalry as well as an explicit strategy for the Navy to compete with the other military services by investing in the work and prestige of scientists.[4]

Revelle effectively linked this Navy struggle to that of oceanographers. In effect, by linking the Navy to Scripps, Revelle established a near-monopoly on the tools with which oceanography could be conducted as a field-based science – military surplus ships, instruments, and worldwide oceanographic data. As with meteorology, Naval research and training became hinges that allowed Revelle and others to begin constructing 'basic science' as a national issue that, in effect, valorized their own career trajectories over others. Revelle's work was particularly oriented to building 'physical oceanography' as a discipline. Like Rossby's success in elevating the Bergen School over other approaches in meteorology, Revelle's leadership at Scripps privileged physical oceanography to the detriment of marine biology (Hamblin, 2005: 12, 24; Rainger, 2001). Revelle fulfilled his advisor Carl Eckhart's dictum in 1950, 'At the present time it is one of the responsibilities of the Scripps Institution … to define the limits [of] oceanography, and to stimulate the formation of a unified profession' (cited in Rainger, 2001: 342). As in meteorology, Revelle's capacity to stabilize war-relevant research as a hinge co-produced a distribution of power within military and science policy bureaucracies on the one hand and oceanography as a discipline on the other. Oceanographic research allowed state officials to integrate oceanic processes into military operations and strategies while grounding national security in a discourse of basic research. By the mid-1950s, such research yielded opportunities for oceanographers to pursue problems of oceanic circulation and climate, which organized climate science and later helped to reorganize the climate state.

Changing tides: Investing in climate science

Reflecting on the oceanographic side-projects that he had conducted during classified sea expeditions in the 1950s, formally devoted to nuclear-weapons testing, Revelle stated, ‘we felt we were revolutionizing the world because of what we were finding …. It’s quite obvious now that 1948 to 1965 [was] one of the great ages of discovery of the earth’ (Revelle, 1975; cf. Rainger, 2000). Capitalizing on the ‘revolutionary’ potential of existing oceanography-state alliances, in the mid-1950s Revelle and his collaborators began to conduct research on global warming through analysis of atmospheric and oceanic carbon dioxide (Keeling, 1960; Revelle and Suess, 1957). However, their scientific findings, which became seminal works in climate science, do not in themselves explain the development of a climate science field. Others in prior decades had linked carbon dioxide to climate change. Rather, the structure of bureaucratic and scientific fields, by then linked durably through matched struggles over how to shape wartime basic research, had created a social world in which immense resources and many careers might be oriented to research on climate dynamics.

With Revelle as an elite spokesperson, in the early 1960s oceanographic research provided grounds for climate change to emerge as a legitimate object of scientific knowledge (Revelle et al., 1965). Revelle and his colleagues’ research would eventually intersect with meteorologists’ atmospheric-circulation research to provide the first global climate models (Manabe and Bryan, 1969). Such models became critical nodes, linking diverse scientists, objects, and organizations together, such that accounts of climate dynamics increasingly had to depend on them, especially because they integrated equations of atmospheric and oceanic circulation. Model construction and use, as in the case of NWP already considered, enrolled diverse scientific tools and specialties, ranging from mathematics and computing to chemistry, physics and atmospheric and oceanographic sciences. To further understand how global climate modeling succeeded and how climate science became a distinct field, the following section specifies a series of matched struggles between climate researchers and state actors during the Cold War.

The climate science field

An autonomous climate science field took shape during the Cold War through the capacity for climate science to shape the national security state (Stage 3 in Table 1). As the Cold War unfolded, the strategic focus on satellite and ballistic weapons technologies, conflicts over airspace sovereignty, the Space Race, and weather modification programs all reformed power, geopolitical controversy and science in relation to the atmosphere. Earth was emerging as a novel object of standardized measurement and abstract analysis, just as it was becoming a new object of especially global military strategy. Scientists continued to build credibility by claiming that basic research fortified pillars of government – economic development, national security, and international scientific superiority. Furthermore, international scientific collaboration hinged US foreign policy to the goals of geophysical researchers. In this context, climate science emerged as a field unto itself rather than as a shaky profession or a ‘stillborn’ research pursuit.

To start with one example, atmospheric scientists' participation in weather 'modification' in the 1950s and 60s shows how they engaged Cold War politics in a way that benefited their positions in the emerging climate science field and, in turn, began to remake the climate state. Through scientific advising, state officials argued that national defense demanded modification programs, and scientists helped formulate such programs as issues of basic research. Navy Commander Paul Jorgenson testified before the US Senate (1966), 'we regard the weather as a weapon' (p. 33), a perspective echoed by politicians such as Senator Alan Bible (p. 42), who claimed, 'I can well recall the warning of Dr. John von Neumann that weather manipulation, not the intercontinental ballistic missile, would be the ultimate weapon for the protection of the free world.' In response, National Science Foundation (NSF) Director Leland Haworth (p. 91) testified that progressing beyond the 'infant art' of previous modification techniques demanded investment in 'basic' atmospheric research. Within this discourse of 'modification' and 'basic' science, scientists raised issues of anthropogenic climate change, which, in a turn of phrase, they categorized as '*inadvertent* modification' (National Science Foundation, 1965).[5] This development illustrates more generally that insofar as issues of 'climate' could be collaboratively constructed, basic climate research could successfully co-produce scientific and bureaucratic fields. Let us turn to the structure and resulting 'conundrum' of this co-production.

The 'first wave' of climate scientists

Many climate researchers in the 1950s and 1960s had professional origins in the academic departments that Rossby, Revelle, and their colleagues had reorganized. Those pioneers, who had opened up space for climate research by virtue of their matched struggles with state actors around WWII, were followed by a distinct *first wave* of climate scientists. Some in this wave were cadets or technicians during WWII, but few were military officers. Many gained their PhDs in the late 1940s and early 1950s. Jule Charney, Joseph Smagorinsky, and Bert Bolin were notable figures in this first wave who continued to organize climate science through the 1960s and beyond.[6] Smagorinsky (1991) recalls that Norman Phillips, among other atmospheric modelers, emerged as one of 'a new breed of young turks never before connected with the field of climatology' (p. 31). Like Charles Keeling and Hans Suess, who worked with Revelle at Scripps, these first-wave researchers engaged government agencies less directly and their support was more likely to come from NSF grants than from military contracts. As a result, they advanced novel professional orientations marked by field-specific stakes and struggles.

In one first-wave career trajectory, Jule Charney entered the UCLA military meteorology program in 1941. There, Charney studied under Jacob Bjerknes and Morris Neiburger, the former brought by Rossby to UCLA and the latter trained by Rossby at MIT. As a mathematician who encountered Rossby's work, Charney was less oriented to the war effort than to developing equations of atmospheric circulation (Lindzen et al., 1990; Phillips, 1995). By 1946, Rossby introduced Charney to von Neumann, who had just begun planning the NWP Meteorology Project (see above). Such networks facilitated Charney's (Charney et al., 1979) advances in NWP and, later, in climate change modeling. This is one early example of a career trajectory in climate science that became more widely possible during the 1950s. In effect, participants became increasingly organized around general circulation models (GCMs) as a means to reconstruct and explain climate dynamics through an intricate system of standardized observations, models, and digital computation (Edwards, 2010; Phillips, 1956).

The International Geophysical Year as 'hinge event' for climate science

By the mid-1950s first-wave climate scientists recognized a major barrier to progress: At the time no standardized data was being collected in vast areas of the globe, limiting how GCMs could be developed or tested against the 'real' atmosphere (Edwards, 2010; Weart, 2008). However, Cold War international relations and the positions that scientists had achieved by the mid-1950s in government agencies made global data collection more plausible (Miller, 2004). In these circumstances, scientists effectively organized the meteorological and oceanographic branches of the International Geophysical Year (IGY) of 1957–1958. Participants exploited new technologies, especially military rockets and satellites, to gather atmospheric data, and they assembled an international observational network and global data centers to organize vast stores of data.

The IGY was a key moment in the co-production of the Cold War state and a climate science field. It was a 'hinge event' (Abbott, 2005: 260), linking individuals in bureaucratic and scientific fields by infusing their efforts with disparate but compatible meanings. For US officials, IGY gave foreign policy an objective, humanist face. IGY planner Lloyd Berkner (1954) declared: 'Tired of war and dissension, men of all nations have turned to "Mother Earth" for a common effort on which all find it easy to agree' (p. 575). Yet, as Needell (2000) shows, Eisenhower's administration supported IGY primarily to develop military technologies. Various US government agencies proffered different justifications for involvement, backed by a taken-for-granted discourse that basic science could enhance national security. Administrators who straddled the concerns of geoscientists and government officials, such as NSF Director Alan Waterman, proved particularly instrumental in constructing the mutuality of diverse interests served by the IGY.[7] In effect, national security and scientific internationalism coalesced around a broad program of rational mastery, illustrated in Figure 1. IGY provided an unprecedented opportunity for those invested in achieving accounts of global atmospheric dynamics. In addition to specific gains of IGY-supported work in atmospheric modeling and global warming (Revelle, 1958; Revelle and Suess, 1957), the pursuit of global knowledge that undergirded IGY became an administrative and technological foundation for global climate modelling (Edwards, 2006). Efforts to secure global data thus amounted to a formative struggle for a climate science field oriented to expansion and to recognition as a basic science.

Making boundaries, stabilizing climate science

Around the time of IGY, climate researchers in the US worked to institutionalize the field, notably through the National Center for Atmospheric Research (NCAR). Founded in 1960, NCAR emerged as the result of a matched struggle waged by atmospheric researchers and state allies, especially Alan Waterman and National Academy of Sciences

Figure 1. IGY Commemorative Stamp Issued May 31, 1958. Designed by Ervine Metzl.
Image retrieved from https://commons.wikimedia.org/wiki/File:Geophysical_Year_3c_1958_issue_U.S._stamp.jpg.

(NAS) president Detlov Bronk, for both of whom directing such research had become a governmental priority.[8] In a high-profile report entitled 'Research and education in meteorology' (NAS, 1958), scientists argued that meteorology retained a frustratingly inferior position among the sciences. In the mid-1950s, 90% of meteorologists were still government employees, and Platzman argued that meteorology continued to suffer the 'trade school blues' (quoted in Weart, 1997: 331; cf. Mazuzan, 1988). Concerned scientists and policy-makers organized the NAS Committee on Meteorology, directed by Rossby and Lloyd Berkner, and including Rossby's former colleagues Horace Byers, John von Neumann, and Jule Charney. They held several conferences in 1956 to support atmospheric research, which they reported was 'on the threshold of a truly exciting and productive era – one in which man's understanding of his environment is about to increase at a rapidly accelerating rate' (NAS, 1958: 6). The report pronounced the 'inescapable conclusion' that 'our nation's best economic interests require a more active attention to the meteorological problem' (p. 4).

Discourse about the 'meteorological problem' successfully established that national progress and security demanded an elite center devoted to atmospheric science. With NSF funding, NCAR was established in 1960 in Boulder, Colorado. It is one of the few centers that continues to generate global climate models, and thus remains a critical node within climate science. Other centers that stabilized US climate research have similar origins. The reputation of NWP had yielded opportunities for participating researchers, opening possibilities to expand atmospheric research in the mid-1950s. For example, Weather Bureau meteorologists, including Francis Reichelderfer and Harry Wexler, worked with the Air Weather Service and Naval Aerological Service to establish the Joint Numerical Weather Prediction Unit (Fawcett et al., 1956). During the 1950s, Harry

Wexler invested Bureau resources into researching climate control (Fleming, 2011; US Advisory Committee on Weather Control, 1957), and in 1955 Joseph Smagorinsky exploited new opportunities within the Weather Bureau to open its General Circulation Research Section. Smagorinsky (1983) recollected: ‘Reichelderfer undertook to fund the whole [Research Section] from Weather Bureau resources … even though it was still considered somewhat improper for the Bureau to involve itself in research.’ In 1963, the section became the Geophysical Fluid Dynamics Laboratory (GFDL), which, like NCAR, remains a critical node that makes ‘global climate’ socio-technically stable and computationally possible. Under Smagorinsky’s directorship in the 1960s, GFDL scientists effectively bridged oceanographic and atmospheric sciences and began to develop the first coupled oceanic-atmospheric climate models – a breakthrough in climate science (Lewis, 2008).[9]

Around 1960, those who secured global data and institutionalized climate modelling intervened to transform the possible orientations, audiences, and networks of future climate scientists. The field became hierarchically organized around climate modeling practices. Attracting the engagement of physicists, chemists, mathematicians, and computer engineers, climate science integrated an increasing number of physical and chemical parameters. Researchers in the field could exchange ‘local’ scientific capital with dominant disciplines and occupy positions in which their specific investments – in modelling climate dynamics – could be validated by the ‘rules’ of those disciplines.

Those who missed the ‘first wave’, particularly non-academic meteorologists and climatologists, lacked relevant credentials, the resources of the field’s pioneers, and field-specific career investments in explaining climate dynamics. Even prominent climatologists with training in geophysics remained concerned that climate science – especially research into anthropogenic climate change – might no longer be acceptable as climatology. For example, with the rise of climate change as a major public issue, Helmut Landsberg (1972, excerpted in Henderson, 2014) commented:

> Alarming tales have been spread, many of them by persons whose standing as climatologists may well be questioned. And just as the competence of a cardiologist in neurosurgery may be doubted so may the judgment of atmospheric physicists or dynamicists in climatology. (p. 73)

Landsberg, like geographers who treated climate as a statistically normalized average of recorded weather, clearly saw the boundaries of climate knowledge shifting beneath his feet. Furthermore, although climate modelling generated field-specific capital for some researchers via peer recognition and scientific discovery, the general product, global models, did not generate the scientific capital and public prestige of weather prediction, modification, and descriptive or agricultural climatology.

Nevertheless, whereas actors at the origins of climate science built it by accruing and exchanging forms of scientific and bureaucratic capital, dominant positions in the climate science field were by the 1960s structured around what Bourdieu (1996a) calls ‘restricted production’ – climate knowledge for its own sake. Overall, therefore, the ‘hinge event’ of IGY and subsequent institutionalization co-produced a new climate science and a new form of state. Climate science stabilized as a global science thanks to parallel efforts amongst Cold War state officials. Computer-based modelling emerged as a critical node that linked disciplines and grounded climate research in physics and mathematics. Through the work of climate researchers, state jurisdiction inherited a new atmospheric territoriality as well as a national security discourse rooted in technical mastery and scientific progress. In effect, the state became partially marked by climate-scientific struggles that, in turn, transformed the climate state in subsequent decades.

Climate science off its hinges? Expert interventions and the state–science conundrum

We have seen that the formation of climate science was deeply contingent on the capacity for scientists to construct critical nodes that hinged scientists' struggles with those of state actors variously positioned within the bureaucratic field. From the 1930s through the mid-1950s, meteorologists and oceanographers expanded their disciplines by implicating 'basic' research in techniques of statecraft. This accomplishment, and the institutions that upheld it, then provided novel opportunities for scientists to orient their work and careers toward one another through the critical node of global climate models.

Two major issues – one substantive, the other methodological – remain to be considered. The first concerns the implications of the historical analysis for understanding subsequent state–science developments concerning global climate change. The second concerns the use of field theory as an approach to understanding science–state co-production and the dynamics of scientific fields.

Into the 1960s, in almost Hobbesian fashion, the truth of science had closely resembled the truth of the state. However, one result of this state–science relationship was that some climate scientists began to confront the state with predictions of catastrophe, grave uncertainties, and calls to regulate industries – all of which threatened to destabilize the constructed hinge of scientific progress and state sovereignty. Formation of the climate science field, in effect, came to haunt the politics of mastery that dominated the foundations of that science, producing a *state–science conundrum* by the 1970s. State actors risked losing the power to police the meanings of natural environments that they sought to integrate into the post-WWII state. Despite this potential tension, from the 1940s into the 1960s many state actors stood to gain from alliances with scientists, precisely because scientific capital – i.e. objective truth-claims and predictions – could generate bureaucratic capital for state actors. In this light, subsequent interventions by climate scientists in formal politics, especially concerning environmental regulation, are significant not only as 'internal' scientific consensus-building and environmental consciousness-raising, but also as a historical inversion of prior science–state relations. Seeing like a *climate* state, to extend Scott's (1998) terms, had initially entailed making atmospheres legible, even predictable and controllable. Yet by the 1970s, those actors whose very expertise co-produced this climate state came to envision risky and possibly catastrophic climate futures.

To the extent that bureaucratic struggles disparately engage scientific experts over how to understand and govern climatic futures, science–state matched struggles remain important. The present analysis therefore suggests further lines of investigation. Ties between scientific controversies and legislative politics, for instance, suggest that constructions of climate change from the 1970s onward were structured by the prior positions that actors held in political and climate science fields, as Lahsen (2013) shows concerning critics of climate modeling. Industrial elites have secured interpretive spaces through which climate change 'denial' remains plausible (Oreskes and Conway, 2010), while efforts to *dis*integrate climate dynamics from state concerns continue to challenge the basic authority of science.

However, in less visible ways relations between climate experts and state actors may be changing towards absolving the conundrum of contested climate futures. Notably, a recent discourse of national security involves *securitization* of climate change impacts through which scientists and state actors render its disruptive effects legible and amenable to containment in a way that preserves the current geo-political and economic order (e.g. CNA Military Advisory Board, 2014; White House, 2015; cf. Mahony, 2014). This recent development may valorize particular forms of climate expertise over others in order to support or reconfigure existing distributions of material and symbolic power. Climate 'adaptation,' as a logic of government that projects climate states into the future, is likewise infused with power relations (Taylor, 2015). Analysis of the climate state could also examine the recent resurgence of 'geo-engineering' and related approaches to technically manipulate climate dynamics. What struggles, otherwise disconnected, may such political and technoscientific efforts be linking together? Diverse public interventions, whether conservative, radical, or technocratic, are likely to develop not on the basis of scientific reason or raw expressions of social interests alone, but also on the basis of historical and emerging field structures and networking practices.

The second issue to consider in conclusion is the relevance of field theory for STS. Using climate science as a test case, I traced the formation of its field autonomy. Given that this narrative brought into view diverse entanglements and alliances, accounts of scientific fields should be more attentive to hinges, homologies and networking practices that escape the singular logic of a well-bounded social field. In the case of climate science, inter-field structures and networks, developed through what I have termed matched struggles, generated the capacity for early climate researchers to garner resources, represent atmospheric dynamics and their relevance to government affairs, and stabilize a 'vast machine' (Edwards, 2010) of people, objects and different sciences to produce climate models. Such processes may apply more broadly to scientific fields, which may be relatively autonomous in their production to a point, but also held together by broader socio-technical structures than a single field and form of capital. Approaches that assume the autonomy of fields – either by not historicizing such achievements or by failing to situate field dynamics outside the sociologically-reductive 'game' that Bourdieu (2004) outlines – may be missing critical elements of the very dynamics that scholars seek to understand.

What applies to conceptualizing scientific fields may likewise apply to the state. The present analysis shows that it may be difficult to explain aspects of state-making without also tracing the strategies and trajectories of scientists. For field theory to inform investigation of science–state co-production (Jasanoff, 2004), clearly we must break with Bourdieu's (1994: 376; 2004, 2014) theory of the scientific field as a strongly autonomous sphere and of the state as the 'central bank' of symbolic power. Power struggles and settlements in science transpire through both field structures and heterogeneous networks, meaning that field theory will benefit from being complemented by ANT analysis. Climate science succeeded through the work of those who could create practical *assemblages* of geophysical phenomena and, as spokespeople for those phenomena, enroll state goals and actors into what over time became critical nodes within climate science. The emergence of such assemblages affirms Latour's (1988) view that scientists generate 'new source[s] of power with which to conquer the state' (p. 56) by bringing particular order to actor-networks. Yet ANT must be reconciled with field theory, because actors' practices clearly can construct a novel world of positions and career trajectories and a new source of power-as-capital.

Finally, further research can investigate the present theorization of climate science as a field generated through science–state matched struggles. First, historical comparisons outside the geosciences could be pursued. A comparison to the development of economics, addressed above, can be extended. Like climate science, neoliberal economics arose in a way that altered power distributions within states, and both sciences challenge the degree to which social and natural processes are governable by previously orthodox means – whether technical mastery or Keynesian intervention. Second, given the time period and geographic scope of the present analysis, an important extension would be to analyze climate knowledge as a state–science formation over a much longer historical timeframe and from transnational perspectives.

Today, the problem of anthropogenic climate change draws climate scientists into seemingly ever more vexed relations of power. Alignments and tensions between scientists and state actors will undoubtedly continue to take various forms in dialectical relation with the structure of the climate science field. Even in political contexts marked by movements against climate science, the will to know and govern future climate 'scenarios' is intensifying, and it remains unclear what climate expertise will look like as these projected futures emerge as objects of government. The degree to which the climate science field may give rise to a powerful basis for public interventions concerning climate change remains a profoundly open question. Yet, as analysis of the formation of the field demonstrates, this question is a historically sensitive one that can be addressed through critical engagement with field-theoretical inquiry in STS.

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Notes

1. This dispute became a mythic tale among Rossby's students. In Byers' (1960) view, 'the Bureau was headed by unimaginative administrators who had no interest in Rossby's scientific brilliance but rather found the young Swede, with his schemes for revitalizing meteorology in the United States, a great nuisance' (p. 252).
2. As Cushman (2005) shows, Bergen-trained meteorologists' vision during WWII extended transnationally, where 'Pan-Americanism' linked Bergen meteorologists' goals with US state interests in Latin America.
3. On 'modification' research in this period see US Advisory Committee on Weather Control (1957), National Science Foundation (1965), and US Senate (1966).

4. Not until the ‘Sputnik crisis’ in 1957 did scientists effectively secure NSF expansion. Although located within the Navy, ONR presented a break from centrally controlled military R&D, organizing broad geophysical research (Sapolsky, 1990). Yet as Doel (2003) shows, military-contracted research at this time favored physical environmental sciences, such that geophysics emerged over the biological environmental sciences.
5. The applied-basic distinction organized the boundary-work atmospheric scientists performed. Atmospheric research might be mapped onto a general axis of opposition in postwar scientific fields between so-called ‘applied’ and ‘basic’ poles.
6. I address Charney and Smagorinsky below. Bert Bolin became the first director of the Intergovernmental Panel on Climate Change in 1988.
7. For these science policy perspectives, see Waterman (1954), Kaplan (1954), and Berkner (1954). For Commerce and Defense Department perspectives, see Weeks (1954) and Quarles (1954).
8. Primary data on NCAR was collected from the UCAR Digital Archives, available at archives.ucar.edu.
9. Smagorinsky’s career parallels other ‘first wave’ climate scientists. Smagorinsky enrolled in the Army Air Force meteorology program at NYU during WWII. In 1950, Jule Charney, whom Smagorinsky met at MIT, recruited him to the NWP Project at Princeton. Smagorinsky’s modeling work and recognition by von Neumann prepared him for later opportunities through the GFDL.

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