AN ABSTRACT OF THE DISSERTATION OF

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American meteorology was synonymous with subjective weather forecasting in the early twentieth century. Controlled by the Weather Bureau and with no academic programs of its own, the few hundred extant meteorologists had no standing in the scientific community. Until the American Meteorological Society was founded in 1919, meteorologists had no professional society. The post-World War I rise of aeronautics spurred demands for increased meteorological education and training. The Navy arranged the first graduate program in meteorology in 1928 at MIT. It was followed by four additional programs in the interwar years. When the U.S. military found itself short of meteorological support for World War II, a massive training program created thousands of new mathematics- and physics-savvy meteorologists. Those remaining in the field after the war had three goals: to create a mathematics-based theory for meteorology, to create a method for objectively forecasting the weather, and to professionalize the field. Contemporaneously, mathematician John von Neumann was preparing to create a new electronic digital

computer which could solve, via numerical analysis, the equations that defined the atmosphere. Weather Bureau Chief Francis W. Reichelderfer encouraged von Neumann, with Office of Naval Research funding, to attack the weather forecasting problem. Assisting with the proposal was eminent Swedish-born meteorologist Carl-Gustav Rossby. Although Rossby returned to Stockholm to establish his own research school, he was the *de facto* head of the Meteorology Project – providing personnel, ideas, and a publication venue. On-site leader Jule Charney provided the equations and theoretical underpinnings. Scandinavian meteorologists supplied by Rossby provided atmospheric reality. Six years after the Project began, meteorologists were ready to move their models from a research to an operational venue. Attempts by Air Force meteorologist Philip D. Thompson to co-opt numerical weather prediction (NWP) prompted the academics, Navy, and Weather Bureau members involved to join forces and guarantee that operational NWP would remain a joint activity not under the control of any weather service. This is the story of the professionalization of a scientific community, of significant differences in national styles in meteorology, and of the fascination (especially by non-meteorologists) in exploiting NWP for the control of weather.

Boundaries of Research: Civilian Leadership, Military Funding, and the International Network Surrounding the Development of Numerical Weather Prediction in the United States

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Kristine . Harper, Author

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CHAPTER 1
INTRODUCTION

Meteorology has witnessed significant disciplinary changes from the beginning of the twentieth century. Indeed, meteorologists from the early 1900s would view with wonder the practice of their science at the start of the twenty-first century. Far from being an art dependent upon a lifetime of experience in one locality, meteorology today is a sophisticated theoretical science. The data it draws upon are no longer limited to what can be transmitted over telegraph lines. Instead, the data are global – available from remote sensing devices such as satellites and radar – and transmitted via high-speed data links all over the world in a matter of minutes.[1]

But the availability of large amounts of data is not what has advanced meteorology in the twentieth century. From the beginning of the century to the immediate post-World War II period, weather forecasters had increasingly large amounts of data to work with, but were able to process only so much information in

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[1] See James Rodger Fleming, *Meteorology in America, 1800-1870* (Baltimore: The Johns Hopkins University Press, 1990) for a detailed account of the early days of meteorology in the United States, including a discussion of theoretical disputes of the period, the Smithsonian's Meteorological Project, cooperative observation networks, and the impact of weather telegraphy. Gisela Kutzbach, *The Thermal Theory of Cyclones: A History of Meteorological Thought in the Nineteenth Century* (Boston: American Meteorological Society, 1979) focuses on the development of early meteorological theory up until the 1920s. For a discussion of early theories concerning the role of water vapor in the atmosphere, see W. E. K. Middleton, *A History of the Theories of Rain and Other Forms of Precipitation* (New York: F. Watts, 1996). A general history of meteorology in the twentieth century remains lacking.

a short period of time. The extra data, no matter how valuable, would be discarded. And those who would create an atmospheric theory could use that data in the solution of their newly developed equations, but even with calculators those solutions would take months or years to solve. As Lewis Fry Richardson had determined during his abortive attempt at numerical weather prediction during World War I, "[64,000] computers would be needed to race the weather for the whole globe."[2] The ability to develop new theory was hampered by the inability to quickly solve non-linear equations. Therefore, many advances in meteorology, particularly in the latter half of the twentieth century depended on one technological innovation: the computer.[3]

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[2] Lewis Fry Richardson, *Weather Prediction by Numerical Process* (London: Cambridge University Press, 1922), 219. The computers were *human.*

[3] Frederick Nebeker's *Calculating the Weather: Meteorology in the 20th Century* (San Diego: Academic Press, 1995) addresses the growth of calculations as a tool of weather prediction from the 19th century to the introduction and acceptance of computers in the advancement of numerical weather prediction (NWP) – the forecasting of weather by the objective solution of equations defining atmospheric behavior. However, in Nebeker's account – the Meteorology Project, which developed the requisite atmospheric theory and operational models from which NWP arose – is von Neumann's project. On the contrary, it was an outgrowth of Carl-Gustaf Rossby's meteorological research school. While Rossby was not on-site, his influence made him the *de facto* head of the Meteorology Project – he arranged for funding, found personnel, provided a publication venue for results, and was actively involved, albeit from Stockholm, in model development. In addition, Nebeker missed an important element of the story: the potentially divisive battle over control of operational NWP that arose between the Air Force's Philip D. Thompson, and the other participants in the Meteorology Project. Similarly George P. Cressman's "The Origin and Rise of Numerical Weather Prediction," in *Historical Essays on Meteorology, 1919-1995: The Diamond Anniversary History Volume of the American Meteorological Society,* James Rodger Fleming, ed. (Boston: American Meteorological Society, 1996) makes note of the major participants and timing of NWP product introduction in the period from the early 1950s to 1965, but makes no mention of the contributions from Navy modelers. There is also no discussion of developments beyond the primitive equation model and the decision-making that drove the parameterization of those models. William Aspray, *John von Neumann and the Origins of Modern Computing* (Cambridge, Mass.: MIT Press, 1990) provides excellent coverage of the development of von Neumann's computer, but again, treats the Meteorology Project from the computational viewpoint and as von Neumann's project. The meteorologists, in particular, the Scandinavian meteorologists so critical to numerical weather prediction efforts in the immediate post-World War II period, are absent from Aspray's telling.

This dissertation describes the transformation of meteorology from a discipline that was more art-form than science at the beginning of the twentieth century through two world wars to just before the dawn of space exploration in the mid-1950s. In the early twentieth century, meteorology in the United States was under the virtually total monopoly of one government agency: the U.S. Weather Bureau.[4] According to a 1911 article in *Scientific American,* "[The Weather Bureau's] personnel include all of the professional meteorologists in this country – with a few notable exceptions."[5] This situation was slow to change. That a single government agency would employee very nearly every practitioner in a given discipline was unique to the meteorology community. Certainly no other scientific discipline came to see every member of its community (at one time or another) employed by one governmental agency. This situation of governmental dominance fed, and was fed by, a chronic lack of funds for both applied and theoretical research, a severely deficient mathematically-based general circulation theory, a dearth of academically trained practitioners, and a reputation among other scientists that meteorology was "unscientific." But the rise of aviation during the Great War, a development which had sparked renewed demands for military meteorology, did

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[4] For histories of the U.S. Weather Bureau, its predecessor, and its successor, see (in chronological order) Phyllis Smith, *Weather Pioneers: The Signal Corps Station at Pikes Peak* (Athens: Swallow Press/Ohio University Press, 1993); Gustavus A. Weber, *The Weather Bureau: Its History, Activities and Organization* (New York and London: D. Appleton and Company, 1922); Donald A Whitnah, *A History of the United States Weather Bureau* (Champaign, IL: University of Illinois Press, 1961); and Patrick Hughes, *A Century of Weather Service: A History of the Birth and Growth of the National Weather Service, 1870-1970* (New York and London: Gordon and Breach, 1970).
[5] "Curiosities of Science and Invention: Meteorology in American Universities," *Scientific American* CV (14 October 1911): 343.

not abate in the 1920s and 1930s.[6] As a consequence, academic programs in meteorology arose in the late 1920s and continued to grow, albeit slowly, throughout the 1930s. With political disharmony in Europe in the late 1930s, the possibility of another major war loomed. Military planners recognized that this new war would put even heavier demands on aviation. No longer just a means of observation, in the next war aircraft would be both a formidable weapon and a means for ferrying large amounts of material. To keep those assets safe, the military would need meteorologists in large numbers. The thousands trained to fill this need in the early 1940s came from mathematics and physics backgrounds.[7] They would change the face of meteorology forever. Armed with the necessary mathematics and physics (and significantly more surface and upper air observation stations and techniques than before the war) to fully describe the atmosphere, equipped with the computer necessary to solve these systems of equations, the post-World War II meteorologists opened the door to a radically new way of approaching both atmospheric theory and weather forecasting: numerical weather prediction.

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[6] Charles C. Bates and John F. Fuller, *America's Weather Warriors: 1814-1985* (College Station, TX: Texas A&M University Press, 1986) concentrates on the military weather services themselves and the effect of the weather on military operations, but fails to address military contributions in advancing the discipline (particularly in relation to numerical weather prediction) in any detail. The story is told from a decided Air Force viewpoint. John F. Fuller's, *Thor's Legions: Weather Support to the U.S. Air Force and Army, 1937-1987* (Boston: American Meteorological Society, 1990) provides a history of the Air Weather Service and its personnel, but does not address the development of Air Force computer models and their impact on the meteorology community. This history only hints at the political maneuverings that took place between the competing Air Force and Navy weather services for funding.

[7] Charles F. Sarle, 15 January 1942 (Library of Congress, Manuscript Division, Harry Wexler papers, B1, Gen. Corr. 1942) [Hereafter **Wexler papers**].

QUANTIFYING THE ATMOSPHERE

The atmosphere and its visible products – weather elements – have been studied, or at least wondered about, since humans first walked the earth. Once Aristotle (b. 384 B.C.E.) penned his *Meteorologica*, meteorology became a permanent fixture of natural philosophy.[8] However, the Renaissance – which would ultimately lead to the transition from qualitative description to quantitative theory and a completely new way of knowing in physics, chemistry, and astronomy – did not extend to meteorology. Indeed, meteorology remained largely a descriptive science until the nineteenth century despite improvements in meteorological instrumentation. Unlike the other physical sciences, the availability of quantitative measurements – temperature, pressure, humidity, wind velocity – did not, in turn, lead to an atmospheric theory which could describe the movement of air and the changes in atmospheric conditions.[9] In contrast to physics and chemistry, meteorology was not a laboratory science – the global atmosphere could not be contained in a flask. And

[8] For an overview of this early period in the history of science, see David C. Lindberg, *The Beginnings of Western Science: The European Scientific Tradition in Philosophical, Religious, and Institutional Context, 600 B.C. to A.D. 1450* (Chicago: University of Chicago Press, 1992); Liba Taub, *Ancient Meteorology* (London: Routledge, 2003). Of course, Aristotle's meteorology also included meteors, rainbows, and other objects and phenomena in the sky, not just what today are considered weather elements.

[9] H. Howard Frisinger, *The History of Meteorology: to 1800* (Boston: American Meteorological Society, 1983) addresses the study of the atmosphere from before the time of Aristotle until the early nineteenth century, including brief descriptions of the development of meteorological instrumentation. W. E. Knowles Middleton has written extensively on the development of meteorological instruments and their uses. See W. E. Knowles Middleton, *The History of the Barometer* (Baltimore: The Johns Hopkins Press, 1964); W. E. Knowles Middleton, *The History of the Thermometer and Its Uses in Meteorology* (Baltimore: The Johns Hopkins Press, 1966); and W. E. Knowles Middleton, *Invention of the Meteorological Instruments* (Baltimore: The Johns Hopkins Press, 1969). See also S. K. Heninger, Jr., *A Handbook of Renaissance Meteorology* (Durham: Duke University Press, 1960); Theodore S. Feldman, "Late Enlightenment Meteorology," in *The Quantifying Spirit in the 18th Century*, Tore Frängsmyr, J. L. Heilbron, and Robin E. Rider, eds., (Berkeley: University of California Press, 1990).

in contrast to astronomy, another science that had been mathematically quantifiable for centuries, meteorology did not study objects that moved along paths that could be predetermined. The entire atmosphere – an uncontrollable, i.e., controlled experiments are not possible, largely immeasurable, constantly moving mass of air influenced by topography, heat sources and sinks, the relative availability of moisture, and the earth rotating beneath it – still was the laboratory for meteorologists as it had been for Aristotle some 2000 years before. Whereas other physical sciences could be studied and probed with experimental data gathered at a single location, meteorology required data gathered globally from the earth's surface to the top of the atmosphere. While the physicists, chemists, and astronomers in the early twentieth century could use analytically solvable equations in theory development, the atmosphere was an extraordinarily complicated system that resisted such "simple" descriptions. The riddle of atmospheric motion would not be so easily solved. The atmosphere's secrets, still not fully revealed today, would only slowly come to be exposed with the advent of numerical analysis techniques capable of solving the non-linear equations which defined atmospheric motion. Thus, scientists would not be able to crack the atmosphere's secrets until mathematically and physically-based meteorological thought joined forces with computerized numerical analysis techniques. Both of these ingredients emerged in the aftermath of World War II. From that point on, meteorology took a discontinuous leap into its disciplinary future.

While this singular achievement that came to be known as numerical weather prediction moved from the research phase in 1946 to operational reality in 1955, it was actually decades in the making. The story of the development and implementation of objective meteorological techniques is just a small part of the vast story that tells of the rise, and indeed, the very creation of a disciplinary identity in United States meteorology. It is a tale of struggles: the struggle to break free of the grasp of governmental domination, the struggle to gain scientific legitimacy, the struggle to control its own research agenda, and the struggle to be professional.

In the early 1900s, meteorology in the United States was synonymous with the Weather Bureau. Although the Smithsonian Institution had led research efforts in meteorology in the middle of the nineteenth century, and the Army Signal Corps had become the *de facto* national weather service in the late 1800s, in 1891 all weather-related activities were placed under the Weather Bureau in the Department of Agriculture. And virtually all meteorologists in the country worked for the Bureau. Thus, in the United States, meteorological knowledge was, with rare exceptions, created by the government. A service organization concerned with the safety and welfare of persons at sea, ashore, and later in the air, the Bureau concentrated on preparing and transmitting forecasts that aided agriculture and transportation industries.[10] Therefore, its "research" was skewed not only to the applied side of the science, but to agricultural meteorology in particular.

The role of the state in producing knowledge is an important consideration in understanding the ecology of knowledge in modern America.[11] Certainly through its many scientific agencies, one would reasonably expect government-funded research to concentrate on matters of direct interest to the government and its provision of services. Those services are generally applied. However, applied research was typically accompanied by more basic scientific research conducted in

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[10] For a contemporary history of the Weather Bureau, see Weber, *The Weather Bureau*

[11] For an introduction to the role of the U.S. government in scientific development and science policy from the beginning of the country to the 1940s, see A. Hunter Dupree, *Science in the Federal Government: A History of Policies and Activities* (Baltimore and London: The Johns Hopkins University Press, 1986). Dupree briefly addresses the role of the Smithsonian, the Signal Corps, and lastly, the U.S. Weather Bureau in the provision of weather information. See Hugh Richard Slotten, *Patronage, Practice, and The Culture of American Science: Alexander Dallas Bache and the U.S. Coast Survey* (Cambridge, England: Cambridge University Press, 1994) for insight into the importance of the U.S. Military Academy's role in science and engineering education in the nineteenth century and how Bache used his military connections to further the work of the Coast Survey. For the period after 1940, see James P. Baxter, III, *Scientists Against Time* (Boston: Little, Brown and Company, 1946); Irvin Stewart, *Organizing Scientific Research for War: The Administrative History of the Office of Scientific Research and Development* (Boston: Little, Brown and Company, 1948). For accounts directly linking military patronage to scientific policy and development, see David H. DeVorkin, *Science with a Vengeance: How the Military Created the U.S. Space Sciences after World War II* (New York: Springer, 1992); Harvey M. Sapolsky, *Science and the Navy: The History of the Office of Naval Research* (Princeton, NJ: Princeton University Press, 1990); Gary E. Weir, *An Ocean in Common: American Naval Officers, Scientists, and the Ocean Environment* (College Station, Texas: Texas A&M University Press, 2001); and Alex Roland, "Science and War," *Osiris* Second Series 1 (1985): 247-272. For an introduction to the literature on Cold War funding of research, see Jessica Wang, "Liberals, the Progressive Left, and the Political Economy of Postwar American Science: The National Science Foundation," *Historical Studies in the Physical and Biological Sciences (HSPS)* 26 (1995): 139-166; Robert Kohler, *Partners in Science* (Chicago: University of Chicago Press, 1991); Paul Forman, "Behind Quantum Electronics: National Security as Basis for Physical Research in the United States, 1940-1960," *HSPS* 18 (1987): 149-229; Larry Owens, "Science in the United States," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood Academic Publishers, 1997), 821-838; Michael A. Dennis, "Historiography of Science: An American Perspective," in *Science in the Twentieth Century*, ed. John Krige and Dominique Pestre (Amsterdam: Harwood Academic Publishers, 1997), 1-26; Rebecca Lowen, *Creating the Cold War University: The Transformation of Stanford* (Berkeley: University of California Press, 1997).

the laboratories of university and industry. Thus, for any scientific discipline, one would expect to see a mix of applied and basic research being conducted at any given time (as it was, for instance, in many branches of the Department of Agriculture, and in the National Bureau of Standards). But meteorology did not fit the pattern of other sciences. Its few underpaid, under-trained practitioners all had the same mission: to prepare forecasts. Its "customers" expected that service to be "accurate" and on time. The general public did not see meteorology as a science. Neither did other scientists.

So why did both the general public and other scientists fail to see what, along with astronomy, must be one of the oldest sciences *as* a science and its practitioners as scientists? One critical reason: lack of funding based on lack of predictability. Astronomy had been a patronage-rich endeavor from the very beginning. The positions of the moon, sun, stars, and planets were regular and predictable. They determined calendars, planting seasons, and influenced, it was thought, human affairs. As much as astronomy was predictable, weather was *unpredictable* – for centuries subject to the will of the gods. By the twentieth century, science-savvy societies no longer considered that the gods, weather or otherwise, played a role in meteorology. On the contrary, by the twentieth century many, if not most, people considered themselves to be just as knowledgeable about the weather as the Weather Bureau meteorologists. Indeed, *everyone was a meteorologist*. In contrast, very few people were astronomers. And in the twentieth century, money poured in to fund astronomical observatories and the work of astronomers.[12] Meteorology did not experience an equivalent influx of funding.

Moreover, meteorology did not make the "cut" as an academic discipline in America's colleges and universities in the late 1800s. Meteorology was a frequently-orphaned discipline, existing at the margins of established scientific disciplines whose outer boundaries shifted rapidly. Early in the twentieth century, it was most often found in geography departments, where it took on a distinctive climatological look, or in physics departments, which sometimes considered atmospheric physics as a problem worth researching. Sometimes meteorology was found in astronomy, geology, and chemistry departments. It was not, however, found in its own department. As University of Southern California graduate student Woodrow C. Jacobs explained in his master's thesis, "[The] study of meteorology as a pure or applied science seems to have been relegated to the background."[13] Indeed, while chemistry, physics, and botany departments were producing hundreds of Ph.D.s between 1919 and 1930, there were *six* meteorology Ph.D.s awarded in the same period.[14] Three of the meteorology Ph.D.s were really in

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[12] Literature on this subject (in contrast to the history of meteorology) is vast; for an entry see Owen Gingerich, ed., *The General History of Astronomy, Volume 4A: Astrophysics and Twentieth-Century Astronomy to 1950* (New York: Cambridge University Press, 1984); and David H. DeVorkin, *Henry Norris Russell: Dean of American Astronomers* (Princeton: Princeton University Press, 2000). If disciplines are political institutions, as Robert E. Kohler argued in the introduction to *From Medical Chemistry to Biochemistry: The Making of a Biomedical Discipline* (Cambridge, England and New York: Cambridge University Press, 1982), 1-8, it is clear that meteorologists had little such power.

[13] Woodrow C. Jacobs, "A Survey of Instruction in Meteorology in the Colleges of the United States" (Master's thesis, University of Southern California, 1934), 50.

[14] Unpublished table of the National Research Council, Table III, Doctorates Conferred According to Subjects (1923) (National Academy of Sciences Archives, Research Information Service, 1920-

climatology – a discipline in its own right. Consequently, the building blocks of an academically-defined discipline – doctoral students and their advisors – were just not available. And without them, there could be no viable meteorological research agenda.

Lacking disciplinary and professional authority, meteorologists often faced challenges from members of neighboring disciplines. Indeed, without a "scientific" research agenda of their own, and without sufficient numbers to reinforce each other, the small, but active theoretical community of the 1800s had disappeared from the scene in the United States in the early 1900s. However, the need for meteorological research remained. American physicists, at the top of their game in the 1920s as Robert A. Millikan picked up the country's first Nobel Prize in physics, stepped into the vacuum.[15] While the Weather Bureau meteorologists' sense of the physical factors involved in the atmosphere had led them to concentrate on improvements to instrumentations in an effort to obtain more accurate data, which they believed would in turn lead to more accurate forecasts, the physicists were convinced they had better tools. Instead of looking at the atmosphere itself, the physicists and their astrophysicist brethren looked to the stars – or at least one star – the sun. The Smithsonian's Charles Greely Abbot successfully argued for funding for his study of the variation in solar output,

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1923, Information Files Doctorates Conferred); "Four New Doctors of Philosophy in Meteorology or Climatology," *Bulletin of the American Meteorological Society (BAMS)* 10 (1929): 166-167.
[15] See Daniel J. Kevles, *The Physicists: The History of a Scientific Community in Modern America* (Cambridge, Massachusetts: Harvard University Press, 1987); Thomas P. Hughes, *American Genesis: A Century of Invention and Technological Enthusiasm, 1870-1970* (New York: Viking, 1989).

contending that those slight variations were singularly responsible for changes in the weather. Therefore, measurements of the solar constant would lead to more accurate short- and long-range weather forecasts.[16] Weather Bureau leaders argued, to no avail, that instrumentation error alone could account for the variations, and that changes in the atmosphere itself would affect the resulting influence of incoming solar radiation. Indeed, Weather Bureau Chief Charles Marvin had become so exasperated with questions about Abbot and his long-range forecasting claims, that the Secretary of Agriculture finally forbade Marvin from talking to the press.[17] In a situation parallel to that experienced by geologists and geophysicists in the early twentieth century while debating Alfred Wegener's theory of continental drift, the geophysicist's argument against the theory on physical causation alone *seemed convincing.* However, continents do indeed move. Likewise, the debate over the importance of solar radiation as a singular forecasting tool stems from a major question of great importance in the history of recent science: in interdisciplinary fields like meteorology, how do scientific communities come to evaluate methods, procedures and conclusions as they relate to accuracy? Who has the better tools to make the evaluations?[18] Those within the community, or those

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[16] Abbot wrote extensively on the connection between solar radiation variations and weather forecasting. For one example, see Charles G. Abbot, "The Weather and Radiation," *The Yale Review* 16 (1927): 485-500.

[17] John Billings, Jr., "Is the Sun Fickle?" *The Independent* 117 (4 September 1926): 269.

[18] Factional tensions within more broadly defined disciplines are addressed by Naomi Oreskes, *The Rejection of Continental Drift: Theory and Method in American Earth Science* (New York and Oxford: Oxford University Press, 1999); Ronald E. Doel, *Solar System Astronomy in America: Communities, Patronage, and Interdisciplinary Science, 1920-1960* (Cambridge, England and New York: Cambridge University Press, 1996); Naomi Oreskes and Ronald E. Doel, "Geophysics and the Earth Sciences," in *The Cambridge History of Science,* Vol. 5, *Modern Physical and*

outside who may have greater prestige and social standing within the wider scientific community? As discussed in Chapter 2, when newly appointed Agriculture Secretary Henry A. Wallace found himself under fire for purported problems with "efficiency" and accuracy within the Weather Bureau in 1933, he did not call in an outside board of meteorologists: he called in an outside board of physicists and geographers headed by Millikan to recommend necessary changes in the Weather Bureau's structure and operation. Meteorologists, apparently, were just not scientifically respectable.

The surprising fact is that a young, influential, entrepreneurial meteorologist had already been spreading the news of a meteorological renaissance that had taken place in Europe in the early part of the century.[19] Swedish-born, Norwegian-trained Carl-Gustav Rossby had arrived in the United States in the mid-1920s on an American-Scandinavian Fellowship to work at the Weather Bureau. Having irritated Bureau leaders with his promotion of Vilhelm Bjerknes's air-mass analysis methods as a way of improving forecasts, Rossby was "fired."[20] However, he was soon hired by the Guggenheim Fund to establish a "model airway" in California. That mission complete, the Guggenheim Fund asked him to create and lead a Guggenheim-funded meteorology program at the Massachusetts Institute of

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Mathematical Sciences, Mary Jo Nye, ed. (Cambridge, England: Cambridge University Press, 2003).

[19] For a thorough discussion of the Bergen School's founding and methodologies, see Robert Marc Friedman, *Appropriating the Weather: Vilhelm Bjerknes and the Construction of a Modern Meteorology* (Ithaca and London: Cornell University Press, 1989).

[20] Horace R. Byers, "Carl-Gustaf Arvid Rossby," *BAMS* 39 (1958): 98-99.

Technology (MIT) in 1928.[21] This program, encouraged by the leader of Navy weather services, the future Chief of the Weather Bureau, Francis W. Reichelderfer, was established to provide graduate training to Navy aerologists.[22] Opened to civilians, Rossby's program would come to provide virtually all of the graduate-trained meteorologists that Millikan's Science Advisory Board recommended that the Weather Bureau hire in 1933. And yet, Rossby was not invited to serve on that Board. But what he did do was even more important. Through Rossby's efforts, Scandinavian meteorologists – the most theoretically-oriented and forward-looking of their day – were invited to come to the United States as guest lecturers. They, in turn, introduced new methodologies and techniques that relied on objective calculations in place of the subjective feel for the atmosphere that had dominated meteorology at the Weather Bureau.[23] When some of them were trapped in the United States as Nazi Germany invaded Norway, these Scandinavians formed the backbone of an advanced meteorological training program under Rossby's direction, a program that trained thousands of men and women to be meteorologists during the war years.[24] The Scandinavians thus brought an international flavor to American meteorology, and a profoundly

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[21] Richard P. Hallion, *Legacy of Flight: The Guggenheim Contribution to American Aviation* (Seattle and London: University of Washington Press, 1977), 219.
[22] T. J. O'Brien, "Comments on 'Sixteen Years of American Meteorology and Its Society," *BAMS* 17 (1936): 380-381.
[23] F. W. Reichelderfer, "The Atmospheric Sciences and the American Meteorological Society: The Early Years," *BAMS* 51 (1970): 210.
[24] "Meteorological Education in the United States: Facilities at Twenty Leading Universities," *Weatherwise* 6 (October 1953): 126-141.

different way of looking at the atmosphere – one that combined a subjective feel *and* objective calculation.

These new, war-time trained meteorologists, in contrast to their counterparts in the Weather Bureau, were all mathematics and physics-savvy. Very few, if any of them, would ever have picked meteorology as a disciplinary field before the war. But when faced with a choice between being a meteorologist and leading a ground platoon somewhere in Europe or the Pacific, they apparently decided that meteorology sounded like the safer decision. Not many of these men stayed in meteorology after the war, but those who did had a significant impact on the community which only numbered a few hundred professionals in the pre-war years.[25] These men had a different approach to the science: a *mathematical* one. Some of them – Jule Charney, Philip D. Thompson, Gilbert Hunt – would devote themselves to finding a mathematically-based theory that would describe atmospheric circulation. But they could not do it without the computer.

Fortunately for the advancement of meteorological theory, the development of the computer occurred contemporaneously with the appearance of mathematically-driven meteorologists. Institute for Advanced Study mathematician John von Neumann had made the development of a digital computer his priority in the period immediately following the end of the war.[26] His desire for a problem

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[25] Rossby to Major General B. M. Giles, 10 September 1943 (Edward L. Bowles papers, Library of Congress, Manuscript Division, B30, F4) [Hereafter **Bowles papers**].
[26] For an entry into the historical literature on twentieth century computing, see Michael R. Williams, *A History of Computing Technology* (Englewood Cliffs, NJ: Prentice-Hall, 1985); David Ritchie, *The Computer Pioneers: The Making of the Modern Computer* (New York: Simon and

intractable by analytical methods led him to the weather forecasting problem. As it turned out, this was an attractive problem to an important patron: the Office of Naval Research (ONR). It was also attractive to Weather Bureau leaders, who recognized that their meager research funds prevented independent research on this topic. Using Rossby as a mediator between itself, von Neumann, and the Navy, Weather Bureau Chief Francis Reichelderfer successfully encouraged the creation of the Meteorology Project in 1946.[27] The Weather Bureau, as usual facing staffing shortfalls, was interested in faster, more accurate weather map production, which would remove some of the inherent subjectivity of a hand-drawn product.[28] The computer could take advantage of the wealth of new data available due to the war-time expansion of both surface and upper air observational networks. The Navy, however, had a different interest. Von Neumann did not simply anticipate being able to *predict* the weather. As von Neumann's proposal to the Navy's Office of Research and Inventions put it, "[The] first step towards influencing the weather by rational, human intervention will have been made – since the effects of any hypothetical intervention will have become calculable."[29] His ultimate goal was to

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Schuster, 1986); Martin Campbell-Kelly and William Aspray, *Computer: A History of the Information Machine* (New York: Basic Books, 1996); and Paul E. Ceruzzi, *A History of Modern Computing* (Cambridge, MA: The MIT Press, 1998).

[27] Reichelderfer to Weather Bureau staff, 29 December 1945 (Wexler papers, B2, F1945); Rossby to Reichelderfer, 16 April 1946 (John von Neumann papers, Library of Congress, Manuscript Division, B15, F7) [Hereafter **von Neumann papers**].

[28] Reichelderfer to von Neumann, 29 December 1945 (Wexler papers, B2, F1946).

[29] Frank Aydelotte (Director, IAS) to Lieutenant Commander D. F. Rex, 8 May 1946 (von Neumann papers, B15, F6). Part of the proposal had been written by Rossby, but the possibility of weather control was definitely von Neumann's contribution.

control the weather.[30] There would be just a short step between understanding the weather to modifying physical weather variables and producing weather-on-demand. Of great interest to military planners, this non-radioactive offensive and defensive weapon could be an extremely important tool in the military's arsenal. It was certainly worth investing in – and was prominently mentioned by Navy personnel who spoke off-the-record when the *New York Times* broke the story of von Neumann's plans in January 1946.[31] And so while the civilian Weather Bureau would lead the effort to pursue numerical weather prediction for operational use, military patronage alone would ensure its eventual success. Just as military funding poured into the coffers of physics and engineering departments during the Cold War years to enhance the production of sophisticated weaponry, detection devices,

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[30] The historiography on weather control is primarily limited to the period prior to World War II. For example, see Clark C. Spence, *The Rainmakers: American "Pluviculture" to World War II* (Lincoln: University of Nebraska Press, 1980). Spence provides a highly readable account of early rainmaking efforts in the United States describing the many truly remarkable characters who populated the field. For a popular account see D. S. Halacy, Jr., *The Weather Changers* (New York: Harper and Row, Publishers, 1968). Fitzhugh Green's *A Change in the Weather* (New York: W. W. Norton and Company, Inc., 1977) takes a non-technical, journalistic approach to weather modification efforts since World War II. Meteorologist Louis J. Battan's *Harvesting the Clouds: Advances in Weather Modification* (Garden City, NY: Doubleday and Company, Inc., 1969) was written as a non-technical discussion of the history and current status of weather modification for secondary school and general audiences in cooperation with the American Meteorological Society as an outgrowth of the Physical Science Study Committee. *Weather Modification: Science and Public Policy* edited by Robert G. Fleagle (Seattle: University of Washington Press, 1969) addresses historical as well as technical, ecological, economical and legal issues of weather modification. *Weather and Climate Modification* edited by Wilmot N. Hess (New York: John Wiley and Sons, 1974) contains two historical articles; the rest of the volume is technical. *Human Impacts on Weather and Climate* by William R. Cotton and Roger A. Pielke (Cambridge: Cambridge University Press, 1995) has a brief historical discussion of weather modification before attacking technical and policy issues. A recent edited book on inadvertent weather modification, global warming, and associated policy issues, *Changing the Atmosphere: Expert Knowledge and Environmental Governance*, edited by Clark A. Miller and Paul N. Edwards (Cambridge, MA: MIT Press, 2001) provides very brief coverage of early weather control efforts.

[31] Sidney Shallet, "Weather Forecasting by Calculator Run by Electronics is Predicted," *The New York Times*, 11 January 1946: 12.

and rocketry for the nascent space race, it provided a much-needed boost to meteorological research in a direction other than agriculture.

While it may seem naïve to think that a virtually-designed atmosphere could be translated into a designer atmosphere in reality, the immediate post-war period was full of optimism for a scientific and technological fix for all manner of natural and man-made problems. The scientific successes of the war led scientists and the general public alike to think that dissipating and/or preventing hurricanes, tornadoes, floods, droughts, fogged-in airports, and polluted air was just as possible as irrigating deserts, preventing flooding by dam projects, or creating usable land from swamps. Weather control was not far-fetched at all – it was just a few years away.[32] Or perhaps it was very close. RCA scientist Vladimir Zworykin, a strong proponent of weather control who had encouraged von Neumann to pursue the weather forecasting problem, seriously suggested to Weather Bureau meteorologists that they could prevent hurricanes from forming by putting oil on the ocean surface under cumulonimbus build-ups and "setting it on fire."[33]

Of course, very few people would now consider designer weather to be just around the corner. But no one is surprised that computers and myriad large-scale,

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[32] Notable examples of work addressing the control of nature theme include Paul W. Hirt, *A Conspiracy of Optimism: Management of the National Forests since World War Two* (Lincoln: University of Nebraska Press, 1994) which argues that the implementation of "sustained yield" and "multiple use" concepts to control forest lands led to severe ecological problems; Donald Worster, *Rivers of Empire: Water, Aridity, and The Growth of the American West* (New York : Pantheon Books, 1985); James C. Scott, *Seeing Like a State* (New Haven: Yale University Press, 1998) on the failure of governmental efforts in social engineering and central planning; Richard White, *The Organic Machine: The Remaking of the Columbia River* (New York: Hill and Wang, 1995) on the control of the Columbia river for a variety of human purposes over time.

[33] H. Wexler to Reichelderfer, 18 October 1946 (Wexler papers, B2, F1946).

meso-scale, and micro-scale models in operation around the globe produce an immense variety of weather forecasting products everyday – products available to anyone with a computer and a modem. What is now taken for granted – numerical weather prediction – was just a dream in 1946 and barely an operational reality in 1955. That its creation took place in the United States – with a combination of Weather Bureau leadership, military funding, and significant Scandinavian influence and assistance – is a case study in the better-late-than-never professionalization of the classical field of meteorology.

AN OVERVIEW OF THIS WORK

The history of numerical weather prediction development opens with an examination (Chapter 2) of the stagnant state of the meteorological services in the period between the end of World War I and the years just preceding the entry of the United States into World War II. Although the Signal Corps and the Navy had both maintained meteorological services during the Great War, the rapid drawdown at war's end left them both with skeleton crews. With the exception of tasks reserved specifically for the military services – and they were few – the Weather Bureau was responsible for the nation's weather. Always operating on a shoe-string budget, the Weather Bureau was decimated by the funding reductions of the Great Depression while the military weather services were barely able to stay alive. The interwar period saw little progress meteorologically, or in new services offered, by the weather services.

In contrast to the penury of the weather services, a disciplinary identity for meteorology was starting to appear in America during this same period. Chapter 3 discusses the emergence of academic programs, and later departments, in meteorology starting in the late 1920s. With Rossby's theoretical MIT program followed within a few years by more practical meteorology programs at New York University and the California Institute of Technology, young people interested in meteorology – not to mention those already employed by the Weather Bureau, Navy, and Signal Corps – finally had viable options for meteorological educations. At the same time, meteorologists finally formed their own national professional organization in 1919: the American Meteorological Society (AMS). A late entry into the realm of professional societies, the AMS, in contrast to other organizations, welcomed anyone interested in meteorology. Indeed, amateurs composed fully half of the membership, in sharp contrast to such older professional societies as the American Physical Society and the American Astronomical Society.[34] The AMS actively encouraged the expansion of educational opportunities at all levels and worked to influence an emerging research agenda in meteorology. These new academic departments and the American Meteorological Society nurtured a slow but steady advancement of scientific theory, scientific practice, and scientific education in meteorology. These advances turned out to be critical for both military needs in World War II, and the theory-based efforts to create numerical weather prediction in the post-war period.

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[34] See Kevles, *The Physicists*; David H. DeVorkin, *The American Astronomical Society's First Century* (College Park, Maryland: American Institute of Physics, 1999).

The meteorological community in the United States was clearly not prepared to provide the massive amounts of atmospheric support to address atmospheric and meteorological phenomena that the Second World War demanded. Chapter 4 addresses the academic community's response to this need: the training of thousands of meteorologists under the direction of Rossby's University Meteorological Committee. Composed of one representative from each of the "Big Five" (MIT, NYU, Caltech, UCLA, and the University of Chicago) meteorology programs, the committee had fulfilled its mission by 1943 and started anticipating the ways in which academic meteorology could influence post-war meteorology. The primary issue: the professionalization of the field. Never before had so many university-trained meteorologists stood ready to influence the discipline. Rossby and his confreres were determined to take this opportunity to move meteorology from an art to a theoretically-based science respected by both the public and their fellow scientists. Their vehicle: a revitalized American Meteorological Society that would promote meteorology as a professional discipline on par with engineering.

Rossby and his colleagues shared the same scientific goal: to pursue basic meteorological research aimed at developing a mathematically-based theory of general circulation. That goal became wedded to John von Neumann's Computer Project in early 1946. How this critical project emerged is the subject of Chapter 5. The Weather Bureau's Francis Reichelderfer, introduced to the idea of forecasting the weather by computer, encouraged von Neumann to use his new machine to attack the weather forecasting problem. Reichelderfer, with no research funding of his own, called upon his long-time colleague, Rossby, to formulate a plan and arrange a patron for a meteorology project at the Institute for Advanced Study in Princeton. Rossby, with contacts in the highest reaches of government, quickly secured the support of the Office of Naval Research. After arranging funding and personnel, Rossby suggested that von Neumann pursue an approach that would first require theoretical development in meteorology to be followed by an operational application to weather prediction. Von Neumann readily agreed and by the middle of 1946 both the Computer and Meteorology Projects were underway. But the Meteorology Project was hampered by a series of personnel problems that prevented much forward progress. Fortunately, war-time educated meteorologist Jule Charney, under the influence of Rossby, was spending this period studying in Norway and trying to develop a series of equations which would describe the motions of the atmosphere. With his equations and a method to filter out "noise" in hand, he and the first "Scandinavian Tag-Team" member, Arnt Eliassen, were ready to join the Princeton team and move numerical weather prediction closer to reality. This chapter, drawing on extensive archival collections, reveals an important example of national styles in science, revealing distinctly different characters of American and Scandinavian meteorological practices.[35] It also provides important new insights into the development and operation of this project, demonstrating that meteorologists, and not von Neumann himself, were the intellectual leaders of this influential undertaking.

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[35] On national styles in science, see Mary Jo Nye, "National Styles? French and English Chemistry in the Nineteenth and Early Twentieth Centuries," *Osiris*, Second Series 8 (1993): 30-49.

When Charney and Eliassen arrived in Princeton, they brought the methods and influence of Rossby and his research school with them. Rossby was, in all actuality, the de facto head of the Meteorology Project. Chapter 6 tells the story of Rossby's research school, its influence on meteorology in general – both in the United States and in Europe – and its influence on development and acceptance of numerical weather prediction as a valid and necessary technique for the creation of meteorological theory and the improvement of weather forecasting. Founder of two meteorology programs in the United States (MIT and the University of Chicago), responsible for war-time training, founder of the first peer-reviewed meteorological journal in the United States and a journal in Sweden aimed at the broader international geophysics community, Rossby was in the perfect position to sway an extremely skeptical international meteorological community to see the wisdom of numerical weather prediction. Providing a series of Scandinavian meteorologists who could bridge the gap between synoptic and dynamic meteorology, Rossby played a critical, and to date, largely unheralded role in the successful development of numerical weather prediction techniques.

Four years into the Meteorology Project, Charney and his team were ready to try their simple barotropic model on a computer: their efforts are the subject of Chapter 7. Unfortunately, von Neumann's computer was not yet ready. The Meteorology Project pushed forward with tests of a variety of atmospheric models – on the Army Ordnance's ENIAC computer and eventually on von Neumann's new computer – modifying the models after each run until team members had fulfilled their goal of creating a realistic atmospheric prediction. But they were not the only ones pursuing numerical weather prediction. AAF meteorologist Philip D. Thompson, an original member of the IAS Meteorology Project, had been developing his own models and testing them at the Air Force's Geophysical Research Directorate in Cambridge, Massachusetts. Alarmed to find out that Charney's group was approaching the point where operational prediction would be reality, Thompson attempted to derail Charney's desires for a joint operational group using the same weather service participants as the Meteorology Project. Indeed, Thompson wanted to control numerical weather prediction himself under the Air Weather Service umbrella. The resulting dispute embroiled the Weather Bureau, the Navy, the Air Force, and von Neumann's group in Princeton. In the end, the combined efforts of the three weather services which had guided the Meteorology Project from the beginning would continue as operational numerical weather prediction inched slowly toward implementation.

With the decision made to pursue a joint operational approach, the Meteorology Project team members changed their focus (Chapter 8) to operational models while continuing their more theoretical work into general atmospheric circulation. The Weather Bureau leadership, for perhaps the first time out in front with a new methodology in meteorology, concentrated on being prepared to house, staff, and carry out a numerical operation as an adjunct to their subjective techniques. All three weather services – Weather Bureau, Navy, and Air Force –

being members of the Joint Meteorological Committee of the Joint Chiefs of Staff, were able to coordinate – more or less harmoniously – in the details of the new operational unit: the Joint Numerical Weather Prediction Unit (JNWPU), to be located in Suitland, Maryland. However, forming an operational organization paid for and staffed by weather services stemming from very different cultures was not an easy task. Inter-service rivalry issues aside, there were problems with setting up a computer-based center in the early 1950s. The Eisenhower administration, seeking to reduce appropriations, was particularly concerned about what seemed to be a flurry of computer purchases. As the administrative coordinator for the JNWPU, the Weather Bureau was left to justify the requirement for a computer powerful enough to handle weather forecasting. In addition, service representatives were forced to execute a competitive bid for a computer in a period of limited competition. Despite all the problems, external created and internally induced, the Air Force, Navy, and Weather Bureau were finally able to produce their first "operational" weather map in May of 1955 – almost three years after the decision was made to move numerical weather prediction from the realm of research to the realm of operations. In doing so, they more quickly advanced numerical techniques than would have been possible under the less pressure-packed research environment.

The opening of the Joint Unit marked the end of the preliminary research period into numerical weather prediction, but it was just the beginning of a period which would see numerical weather prediction spread worldwide in just a very few years. The Epilogue (Chapter 9) briefly extends the story to the current time. As their very different meteorological missions exacerbated cultural differences between the three weather services, the Navy and the Air Force pulled out their personnel and formed their own operational prediction units, leaving the Weather Bureau to fund and man their own center. As computer availability increased along with processing speed and memory capacity, universities began their own modeling and research projects. The modeling and prediction efforts of individual European nations joined forces to create the European Center for Mid-Range Weather Forecasting (ECMWF) which would provide formidable competition to the U.S.-based efforts. In time, modelers would attempt to forecast for longer and longer periods of time until long-range forecasts took the first steps to becoming (sometimes controversial) climate models.[36]

It has been almost fifty years since the first operational forecasts made their appearance. They were just barely satisfactory by the standards of the day – they would be even less satisfactory today. But they were a start. And the modeling of the atmosphere – for both operational purposes and theory development – continues

[36] Norman Phillips's work on the general circulation models was a precursor to climate modeling. See Norman Phillips, "The General Circulation of the Atmosphere: A Numerical Experiment," *Quarterly Journal of the Royal Meteorological Society* 82 (1956): 123-164. The development of climate modeling in the twentieth century is beyond the scope of the current study. On the emergence of climate models, see Paul N. Edwards, "Representing the Global Atmosphere: Computer Models, Data, and Knowledge about Climate Change," in *Changing the Atmosphere: Expert Knowledge and Environmental Governance*, Clark A. Miller and Paul N. Edwards, eds. (Cambridge, Massachusetts: The MIT Press, 2001), 31-65. Human recognition of possible anthropogenic contributions to climate change is another distinct issue; see, for instance, Spencer R. Weart, "From the Nuclear Frying Pan into the Global Fire," *Bulletin of the Atomic Scientists* 48 (5) (1992): 18-27, and James Rodger Fleming, *Historical Perspectives on Climate Change* (New York: Oxford University Press, 1998).

today, and will continue into the future, to expand the knowledge of all those who seek to understand its secrets.

CHAPTER 2
A STAGNANT ATMOSPHERE: THE WEATHER SERVICES IN THE INTERWAR PERIOD (1919-1938)

The meteorological "renaissance" which occurred in Norway and extended to other European countries at the close of World War I did not extend to the United States. In Europe, meteorology held the same "rank" as astronomy in academic institutions, and research on the theoretical underpinnings which would advance the science was carried out at a number of academic institutions in Norway, Germany and England. In the United States, the top academic institutions did not treat meteorology as a topic on par with any of the physical sciences. If it appeared at all, it was generally within a geography course dealing with climatological issues. At state universities, meteorology courses were often related to agricultural instruction and indeed were frequently developed by Weather Bureau personnel charged with state climatological and crop studies.[37]

Research was limited in the United States because meteorology fell under the control of the U. S. Weather Bureau, which in turn operated under the jurisdiction of the U.S. Department of Agriculture. Although the Weather Bureau had a mission to keep the general public informed of upcoming weather events, its primary obligation was to provide forecasts of value to agricultural interests. Because it was a government agency, any research it performed had to have an

[37] Herbert H. Kimball, "Recent Advances in the Science of Meteorology and in its Practical Applications," *BAMS* 14 (1933): 4.

immediate practical result.[38] Similarly, the other two very small "weather services" in the country – maintained by the War and Navy Departments – existed to provide specialized forecasts for army and navy units. Any research they were able to conduct in their spare time supported operational requirements.

The military use of aviation increased dramatically during the Great War, and with it the importance of meteorology in keeping pilots and their aircraft safe and out of trouble. The Weather Bureau received an infusion of $100,000 to establish aerological stations and coordinate services with the War and Navy Departments once the United States entered the war; "flying-weather forecasts" started to appear in December 1918 in support of the military and the Post Office. Although the funding continued after the war ended, the Weather Bureau made little progress in expanding its services during the immediate postwar period. In contrast, European countries were heavily subsidizing the establishment of civil airways and the meteorological services needed to support them.[39] As Charles F. Brooks, American Meteorological Society (AMS) Secretary, noted in 1922, the Belgians were "astonished" that the Weather Bureau's annual budget was only $2 million – or two cents per person – and he concluded that "[m]eteorological expenditures and general interest in meteorology are greater in Europe than in the United States."[40] As a result, meteorological advances in Europe were aided not

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[38] Ibid., 4, 6.

[39] Willis Ray Gregg, "History of the Application of Meteorology to Aeronautics with Special Reference to the United States," *Monthly Weather Review(MWR)* 61 (1933): 165.

[40] Charles F. Brooks, "Reclassification," *BAMS* 3 (1922): 164.

only by the academics working in special institutes, but by those working on the applied side of the science.

While European meteorology flourished after the war, it stagnated in the United States. The initial promise of increased spending, the rise of aeronautics, and the need for meteorologists that appeared during the war years very quickly gave way to retrenchment. Progress was limited – academically, theoretically, and within the applied sector. The under-funded, undermanned, under-trained, and chronically discouraged Weather Bureau personnel advanced the practical, forecasting side of meteorology despite externally imposed limitations. The Army (Signal Service) and Navy weather services, decimated by the immediate drawdown of forces at the end of World War I, limped along with a handful of wartime leftovers who saw a future supporting military aviation. While the Signal Service concentrated on designing and building new meteorological instrumentation, the Navy actively sought a more theoretical path towards weather forecasting. The Navy's drive to professionalize its ranks would lead to the first graduate meteorology program in the United States. And by the end of the 1930s, major meteorology programs would be established at MIT, New York University, and Caltech. These programs, and others that followed soon after, would lay the groundwork for U.S. meteorology during World War II and the Cold War. This educational foundation was a necessary condition for the numerical weather prediction efforts which would begin in the immediate post-World War II period.

WEATHER FOR ALL REASONS: THE WEATHER BUREAU

The U.S. Weather Bureau – the nation's official weather service – was established by an act of Congress in 1890. However, an observational network had been in place since the early nineteenth century when the U.S. Army Medical Department, professors employed by some of New England's colleges, academies in New York, and the General Land Office began systematically collecting weather information. By the 1830s and 1840s, basic observations had been expanded to include data on storms and winds. Meteorological research then shifted to the Smithsonian Meteorological Project between 1849 and 1861. This program, directed by Joseph Henry, focused on storm movement and climate statistics, and was conducted in conjunction with a number of government agencies and the Canadian government. The U.S. Army Signal Office started transmitting daily reports of current conditions and forecasts (called "probabilities") via the nation's telegraphy circuits in 1870.[41] The Signal Service continued as the national meteorological service until 1 July 1891 when its weather duties were transferred to the Weather Bureau under the Department of Agriculture. The transfer was due to a Congressional Act of 1 October 1890 (26 Statutes at Large, 653). The Weather Bureau's functions, as set

[41] See Fleming, *Meteorology in America.* Fleming's book is the definitive work on these early years of meteorology in the United States. Kutzbach, *A Thermal Theory of Cyclones* addresses the development of meteorological thought in the United States and Europe from the mid-nineteenth century to the polar-front theory of the Bergen School.

forth in Section 3 of the act, remained in force through this period.[42] They were as follows:

> The Chief of the Weather Bureau, under the direction of the Secretary of Agriculture, shall have charge of forecasting the weather; the issue of storm warnings; the display of weather and flood signals for the benefit of agriculture, commerce and navigation; the gauging and reporting of rivers; the maintenance and operation of seacoast telegraph lines and the collection and transmission of marine intelligence for the benefit of commerce and navigation; the reporting of temperature and rainfall conditions for the cotton interests; the display of frost, cold-wave, and other signals; the distribution of meteorological information in the interest of agriculture and commerce and the taking of such meteorological observations as may be necessary to establish and record the climatic conditions of the United States, or are essential for the proper execution of the forgoing duties.[43]

To carry out this mission, the Bureau was organized into sixteen divisions. Some were administrative (stations and accounts, supplies, printing, telegraph, library), while the rest covered the range of scientific interests – meteorology, hydrology, seismology and volcanology – plus the instrument division to support them.

Five regional forecasting districts covered the United States and issued forecasts and warnings for the states in their region. The regional district for the eastern U.S. was located within Weather Bureau headquarters in Washington, D.C., and its chief forecaster had veto power over all forecasts issued by the other

[42] See Whitnah, *A History of the United States Weather Bureau*, 131-200 for a discussion of this period in Weather Bureau history, based primarily on internal Weather Bureau publications and the transcripts of legislative hearings.

[43] 26 *Statutes at Large* 653, Section 3 (1890) as quoted in Weber, *The Weather Bureau*, 16.

regional sites.[44] The district offices were in turn supported by over 200 regular stations, which employed between one and fifteen full-time paid employees who took and transmitted observations and issued local area forecasts. When, and if, these workers had time, they performed supervisory functions and conducted limited research. Repair and vessel reporting stations also employed full-time paid workers. In addition, there were part-time employees who were paid nominal amounts, e.g., $10-25/month, to make specific observations. An example would be those who read river gauges. Since these stations could in no way provide sufficient coverage of the entire United States, there were several thousand unpaid volunteers who maintained so-called "cooperative stations" to collect observations for climatological studies, weather-crop and weather road services. These volunteers often distributed forecasts and warnings in their local area.[45]

Whether paid employees or volunteers, Weather Bureau staffers were dedicated to providing the best possible weather forecasts to a wide variety of agricultural, commercial, and industrial interests. Although many people thought the recently inaugurated (1919), and highly publicized, aviation service occupied the bulk of the Weather Bureau's time, in fact it was a minor, albeit growing, portion of the Bureau's work load.[46] Furthermore, since many citizens were involved, directly or indirectly, with agriculture, it is easy to understand why the

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[44] Weber, *The Weather Bureau*, 17.
[45] Weber, *The Weather Bureau*, 44.
[46] *Report of the Chief of the Weather Bureau, 1932-1933* (Washington, D.C.: Government Printing Office, 1933), 1.

general population grew to associate the Weather Bureau with providing services just to them.

By the early 1920s, the Weather Bureau's five regional offices produced weather maps and written forecasts for the general public, and transmitted them to major media outlets. Newspapers in larger communities printed the forecasts which were also posted in a variety of public places: railroad stations, post offices, hotels and department stores. The Weather Bureau was *the* source for weather information for the general public. Local stations issued forecasts for the geographic region within a 20 mile radius and warnings in case of severe weather.[47]

In addition to weather forecasts for the public at large, the Bureau performed extensive work in agricultural meteorology. Although most forecasts and advisories were tailored to an individual crop, the weekly *National Weather and Crop Bulletin* presented the previous week's meteorological data and that weather's impact on vegetation, stock, and farm work. The Bureau collected specialized data for corn, wheat, cotton, sugar, and rice states, while cattle-grazing states pushed hard for information that would aid them. It published data for fruit frost for tobacco, fruit, truck and alfalfa seed districts.[48] Fruit-frost warnings were important to citrus-growers in California and to orchardists in Oregon and Washington – allowing them to "smudge" their groves and save the crops when the

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[47] Weber, *The Weather Bureau*, 17.
[48] *Report of the Chief of the Weather Bureau, 1920-1921* (Washington, D.C.: Government Printing Office, 1922), 19.

local station forecasted a hard freeze.[49] Similarly, there were forecasts and advisories aimed at tobacco growers, grain growers, New York apple-growers who needed to spray for scab,[50] millers needing to rid their mills of Mediterranean flour moths by flooding them with very cold air from outside, and beekeepers whose bees needed a "cleansing flight" before the winter.[51] Another agricultural interest group – forestry – lobbied hard for expansion of fire-weather forecasting in the west, which warned of periods of extreme fire danger based on meteorological conditions, and also advised when forecast precipitation would help quench fires. Additional appropriated funds combined with private funding helped to develop and extend more detailed warnings in fire-sensitive areas.[52]

Environmental historian Stephen J. Pyne has argued that "[meteorology] is a statistical science" because it deals with large-scale events. He further argued that "[fire] helped to bring meteorology out of the clouds and back to the earth."[53] Both assessments are incorrect. Although statistical methods were used to draw information for long-term climatological trends and probabilities which were attached to forecasts (e.g., 50% chance of rain), meteorology is fundamentally a

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[49] *Report of the Chief of the Weather Bureau, 1921-1922* (Washington, D.C.: Government Printing Office, 1923), 17.
[50] *Report of the Chief, 1921-1922*, 14-15.
[51] *Report of the Chief of the Weather Bureau, 1923-1924* (Washington, D.C.: Government Printing Office, 1925), 8.
[52] *Report of the Chief of the Weather Bureau, 1925-1926* (Washington, D.C.: Government Printing Office, 1927), 4. Stephen J. Pyne has written several books on the history of fire. See his *Fire in America: A Cultural History of Wildland and Rural Fire* (Princeton, N.J.: Princeton University Press, 1982), 314-317, for background on fire issues and a discussion of fire weather forecasting as provided by the Weather Bureau (later the National Weather Service) and the coordination that took place with the U.S. Forest Service. However, the large topic of fire weather forecasting and its ties to weather control has not received the attention it needs.
[53] Pyne, *Fire in America*, 317.

geophysical science. Furthermore, the field forecasters – the ones providing fire-weather forecasts – have always been down to earth. There was no room for theoretical flights of fancy in the forecast center.

In support of commercial interests, Weather Bureau forecasters advised shippers when extreme temperatures might harm produce and animals in shipment. For example, freezing temperatures ruined bananas in transit, and extreme heat could kill livestock being moved from farms and ranches to feedlots and slaughter houses. As Chicago Weather Bureau "official forecaster" Henry J. Cox wrote for *The American Magazine*, "The weather has a finger, so to speak, in almost every business pie."[54] Cox pointed out that businesses dealing in perishable crops and livestock would do well to consult the weather map or call their local forecasting station for advice. Doing so saved businesses millions of dollars every year. If it were not for the Weather Bureau, Cox continued, "[Consumers] would have to pay more for [their] fruit and vegetables."[55] So while *free* weather forecasts saved businessmen and their customers many millions of dollars annually, the Weather Bureau's budget was only two million dollars in 1922.[56]

The Bureau also created "highway forecasts" for drivers – much in demand by automobile associations and road commissioners who needed to know when to activate snowplows during the winter. Marine forecasts were also within the purview of the Weather Bureau, having taken this responsibility back from the

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[54] Henry J. Cox, "Curious Ways in Which the Weather Affects Business," *The American Magazine* 94 (August 1922), 54.
[55] Ibid.
[56] Weber, *Weather Bureau,* 70.

Navy in 1904. Cooperative agreements with ocean shipping interests, including the fleets of Standard Oil and the Texas Co., enabled the Weather Bureau to get timely reports from ocean areas, which helped make more accurate forecasts for these same units.[57]

Businesses and those involved with the development of water supplies for hydro-electric power and irrigation encouraged the expansion of the Bureau's river and flood services. Although the flood warning system met the demand, measurements of stream flow as related to precipitation amounts remained unfunded – an issue that became more critical during the drought years of the early 1930s.[58] Additionally, the Bureau started collecting and publishing earthquake data in 1914 and was also watching over volcanic activity – in particular the Kilauea volcano in Hawaii.[59] Although the latter task was eventually passed to the Geological Survey, the Weather Bureau was apparently viewed as the all-purpose collector of earth sciences data whether they were water-related or not.

Nevertheless, the fastest-growing forecasting and data collection area during this period was in support of aviation. Aeronautics – in the form of airships, balloons, and fixed-wing aircraft – had taken on greater importance as a result of World War I.[60] At war's end, the military meteorological organizations that had

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[57] *Report of the Chief, 1920-1921*, 11.
[58] *Report of the Chief, 1920-1921*, 13. *Report of the Chief of the Weather Bureau, 1933-1934* (Washington, D.C.: Government Printing Office, 1934), 3. For more on river management in the United States, see Worster, *Rivers of Empire.*
[59] *Report of the Chief, 1921-1922*, 27. *Report of the Chief of the Weather Bureau, 1922-1923* (Washington, D.C.: Government Printing Office, 1924), 10.
[60] For more on aviation during World War I, see Bill Robie, *For the Greatest Achievement: A History of the Aero Club of America and the National Aeronautic Association* (Washington, D.C.:

expanded to fill the need contracted rapidly. However, the aviation assets remained and accurate forecasts of take-off, in-flight, and landing conditions were a requirement for safe flight operations. Since the military services no longer had the manpower to provide those services and with the increasing demand for air mail services, the Bureau started its flying weather forecasting service in July 1919 for the Army's Air Service, the Navy and the Post Office Departments. Within a short period of time, commercial aviation companies started requesting forecasts. More and more aviators were stopping by weather stations before taking off. That aviators actually wanted forecasts was good news. The problem: forecasters did not have sufficient upper air and local reports to make the kinds of forecasts the aviators needed.[61] The demand for services increased with each passing year. Individual pilots in larger numbers were requesting forecasts and other weather information. The Bureau worked out cooperative agreements with both the Army Air Service and the Navy. Air Service pilots visited stations to find out the details of weather conditions in different parts of the country and made contacts so that they knew whom to call upon for weather information in the future. The Weather Bureau started giving lectures to aviators about what the Bureau could and could not do for them. These talks touched on climatology, air currents, physics of the air

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Smithsonian Institution Press, 1993), 79-92. For background on aviation during the early part of the twentieth century, see Hallion, *Legacy of Flight*. For a brief discussion of aeronautical meteorology, see Gordon D. Cartwright and Charles H. Sprinkle, "A History of Aeronautical Meteorology: Personal Perspectives, 1903-1995," in *Historical Essays on Meteorology, 1919-1995: The Diamond Anniversary Value of the American Meteorological Society*, James Rodger Fleming, ed. (Boston: American Meteorological Society, 1997), 443-480.

[61] *Report of the Chief, 1920-1921*, 10.

and other meteorological subjects that impacted aviation interests. In addition, the Bureau's headquarters office made a desk available for one of the Navy's meteorologists so he could prepare a weather map at the same time as his Bureau counterparts and then transmit weather information to naval stations around the country.[62] The Army and Navy shared the cost of obtaining upper air information by forwarding their pilot balloon reports to Weather Bureau headquarters.[63]

The rapid growth in aviation after World War I had a huge impact on the Weather Bureau. By the early 1930s, the U.S. had 25,000 miles of civil airways for which the Bureau provided support with the assistance of over 500 cooperative (non-paid) and second-order (minimal pay) stations along the routes. The 13,000 miles of airways which supported all-day flying were served by 24-hour stations. These were significantly more expensive to operate than Bureau stations, which provided routine support to public and agricultural interests.[64]

The provision of climatological data was a major, non-forecasting service provided by the Weather Bureau. Over 4500 volunteers (termed "cooperative observers") made observations and mailed them to headquarters monthly. The

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[62] *Report of the Chief, 1921-1922*, 10-11.
[63] *Report of the Chief, 1920-1921*, 16. A pilot balloon is a (usually) red balloon, in this period filled with hydrogen, sent aloft to rise at a predetermined rate and tracked with a theodolite (an instrument used to visually track an object and determine its azimuth and elevation angles while in flight). The observer would then be able to determine upper level winds near the station. At night, observers would attach a small paper lantern holding a lighted candle to the balloon. The combination of hydrogen filled balloons and lighted candles made this operation a risky one.
[64] *Report of the Chief, 1932-1933*, 5.

climatology section compiled the data to determine average temperatures and precipitation all over the country and published the results.[65]

The insurance industry in the United States expanded its product line from life and fire coverage into weather insurance during this period. Companies underwriting weather insurance became major consumers of climatological data. As this sector of the insurance business grew, so too grew the demands on the Weather Bureau for another "free" product. Providing climatological data was just another service of the Bureau, and anyone could ask for and receive data destined for publication at no charge. For information on rainfall that would not otherwise have been computed, the Weather Bureau charged 70 cents per hour in overtime.[66]

The demand for rain insurance, e.g., insurance which guaranteed receipts for a ball game in case the weather turned bad, increased dramatically during this period. So did the demand for hail insurance, which had already been offered for about 25 or 30 years. Henry W. Ives and Company, a New York firm, issued "Pluvius Weather Policies" which guaranteed losses due to unfavorable weather. Such policies were purchased by farmers wishing to guarantee their crops receipts against losses, by contractors desiring to protect themselves from losses due to penalties for weather-induced delays, and to sports promoters to guarantee gate receipts. Customers had to purchase the policy at least a week in advance of the event. Why a week? Because weather forecasts were only good for about 24 to 36 hours. No one could predict the weather a week in advance. However, as the

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[65] Weber, *The Weather Bureau,* 40-41.
[66] Harold Yost, "Adjusting Rain Insurance Policies," *BAMS* 5 (1924): 17-19.

accuracy of longer forecasts increased, a longer lead time would come to be required for purchasing the policy. Premiums were set based on climatological data. If climatology indicated that heavy rains were more likely than not on the day of the scheduled sports event, the premiums would be higher than they would be if climatology indicated that dry weather were likely. Without climatological data the insurance companies had no way to determine their risk and therefore the premium rate that they should charge for coverage. While data were not available for the amount of money spent on premiums for weather insurance, data were available for hail insurance premiums paid during 1919. During that year, customers paid $30 million for premiums insuring $559 million in risks.[67] And for the climatology data to determine this risk the insurance companies paid next to nothing. Indeed, the author of the article on weather insurance which appeared in the AMS *Bulletin* had been a Weather Bureau employee at one time. He undoubtedly increased his meager salary dramatically when he left the Bureau for his new position with an insurance company.

The Weather Bureau had a clear-cut and obvious civil role providing weather forecasts to the general public and to a wide range of business interests. But during times of war, the Bureau was, by necessity, drawn in to support military operations. The Weather Bureau provided weather forecasts and observations (surface and upper air) in support of aviators, balloonists, and artillery units during

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[67] *Report of the Chief, 1922-1923*, 17. "Insurance Against Adverse Weather," *BAMS* 2 (1921): 13, reprinted from the *Birmingham News,* August 12, 1921. A. H. Palmer, "Weather Insurance," *BAMS* 3 (1922): 67–70.

World War I. Two members of the Weather Bureau staff took on reserve status in the Army Signal Service and worked with others from the Bureau who, along with their British and French counterparts, had joined the active duty forces to form a special forecasting unit in Europe. Their forecasting problems included gas dispersal and ballistic winds calculations for artillery units in addition to aviation forecasts. Wind forecasts were particularly critical for gas and flame regiments. When the ground forces in the trenches lobbed gas canisters or aimed flame throwers at the enemy, they had to be sure that the gas or flames would not be blown back over their own positions. Even if the wind were not blowing the gas back over them, a too-strong wind could disperse the gas quickly and render it ineffective. Shells fired over long distances were also affected by the wind. The effect of the wind had to be entered into the firing solution before the larger guns were fired to ensure the ordnance would land on the desired target.[68]

On the home front, the Weather Bureau provided forecasts and warnings to army camps and navy bases, and forecasts to railroads which were handling food and other supplies for the war effort. In addition, the Bureau provided meteorological equipment to the military services, climatological data to the Surgeon General's office in connection with health issues for military personnel, made studies of upper air conditions in support of aviation, reported vessels entering and leaving ports where they had stations, and assisted in the organization

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[68] Weber, *The Weather Bureau*, 36-37.

of gas and flame regiments.[69] Weather forecasts in support of the military services during World War I stand in marked contrast to the situation during the Civil War. Granted, forecasting methods did not exist in the mid-nineteenth century. However, weapons did not have sufficient range to be affected by the weather. Furthermore, given the relatively short distances over which troops moved, military leaders may not have been able to alter their strategies even if they had had weather forecasts. The Civil War did disrupt the volunteer observational network that had been carefully knit together across the eastern United States. The destruction of this network dealt a damaging blow to Joseph Henry's meteorology project at the Smithsonian.[70]

Once the war ended, there was once again discussion about which agency would provide meteorological support for military units when President Wilson convened a board to look in to this issue. Some argued that each entity should be supported by its own meteorological group. However, the Weather Bureau argued that while separate groups made sense during wartime, during peacetime there was no reason why the Bureau could not and should not provide all meteorological services.[71] The Weather Bureau thought that it was fiscally prudent to expand its mission to cover aviation services rather than to outfit a meteorological service for each military department. Although it acknowledged that each service did need to

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[69] Ibid.

[70] For more information on the Smithsonian meteorology system and its problems during and after the Civil War, see Fleming, *Meteorology in America,* 146-147.

[71] *Report of the Chief of the Weather Bureau, 1924-1925* (Washington, D.C.: Government Printing Office, 1926), 3.

maintain small meteorological units at flying fields, naval bases, ordnance proving grounds and similar military activities, and to have a small number of meteorologically trained personnel in the event of a national emergency, the Bureau argued that there were too few qualified meteorologists in the country to spread them among several agencies. With 90% of the "trained and dependable" meteorologists in or associated with the Weather Bureau, its leaders argued that it made more sense to adopt the U.S. Coast Guard model, which made its personnel military members during war time.[72]

This would prove to be an ongoing issue in the interwar period. Indeed it would extend through World War II and then reappear as an issue during the Cold War. The government's enemy: duplication of effort.

The impoverished Weather Bureau, already stretched thin just trying to meet the myriad demands of its non-paying customers, was not in a position to pursue a research agenda. In this way, the Bureau was unique among many other Department of Agriculture agencies which devoted considerable time, talent, and funding in the pursuit of research. While eighteen percent of the Bureau of Chemistry and Soils' and almost half of the Bureau of Experiment Stations' budgets were earmarked for research, the Weather Bureau's budget include *no* funds for research.[73] The bulk of Weather Bureau appropriations was for the

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[72] *Report of the Chief, 1924-1925*, 4.
[73] See appendices 1, 2, and 6 of Gustavus A. Weber, *The Bureau of Chemistry and Soils: Its History, Activities and Organization* (Baltimore: The Johns Hopkins Press, 1928); Milton Conover, *The Office of Experiment Stations: Its History, Activities and Organization* (Baltimore: The Johns Hopkins Press, 1924).

fulfillment of practical weather services for the public, agriculture and industries. Consequently, research efforts were limited to whatever could be squeezed out of the time remaining at the end of the forecast period.

Weather Bureau meteorologists were interested in advancing their discipline, but their "investigations" did not usually extend to asking or answering theoretical questions. On the contrary, investigations focused on agricultural concerns, e.g., the relationship between weather and crops, storm development, upper air conditions, climatology, solar radiation and its impact on weather and climate, and the improvement of meteorological instruments.[74] With the government's emphasis on practical value, Congress was not going to appropriate funds for research that would not yield improved forecasts of economic importance.[75]

Work in agricultural meteorology did include the effects of weather elements, e.g., temperature and precipitation, on crops and how they influenced production and yields. For example, winter wheat is affected by the ambient air temperature and whether the precipitation falls as snow or rain. Determining the optimum conditions would help farmers to determine when they could anticipate bumper crops or poor harvests. Additionally, the Bureau conducted research on the impact of certain weather conditions on crop harvests and the geographical distribution of farm products and farming types throughout the United States. [76]

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[74] *Report of the Chief, 1932-1933*, 7; C. F. Marvin, "Committee on Research," *BAMS* 3 (1922): 11.
[75] Herbert H. Kimball, "Recent Advances," 4.
[76] Weber, *The Weather Bureau*, 28.

Another Bureau study, this one of practical importance to civil engineers, was on sky brightness, i.e., the amount of natural lighting that could be expected during different seasons, hours of the day, and atmospheric conditions. [77] Engineers needed to determine the extent of natural illumination which would be available in buildings they were designing and constructing, including schools, office buildings and industrial buildings. Although not included in the studies, the Bureau recognized a need to determine the amount of light that was available due to its reflection from surrounding buildings.[78]

Solar radiation investigations, which were common starting in the early 1920s and extended throughout this period, soon came to embroil the Weather Bureau in a very public controversy with non-meteorologists. This would not be the first, or the only, time that scientists without a meteorological background would attempt to tell the meteorology community in general, and the Weather Bureau in particular, what physical variables were really important in understanding the atmosphere. Scientists both within and outside the Bureau were attempting to establish a possible link between solar intensity and weather phenomena which would aid in forecasting.

Solar radiation investigations involved making continuous records of the amount of radiation received on a horizontal surface from the sun and the sky to determine the rate of heat received during the day, the amount of heat lost during

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[77] Weber, *The Weather Bureau,* 32.
[78] *Report of the Chief, 1921-1922*, 29.

the night and the relationship between these values and atmospheric conditions.[79] Early in these researches the Weather Bureau was not as optimistic as those who argued that observed solar intensity was an accurate indicator of incoming weather, although it did allow that there might be a connection between solar intensity and longer period variations.[80] While acknowledging that solar radiation was important to weather, the Weather Bureau did not believe that the insolation, e.g., the amount of *in*coming *sol*ar radi*ation*, varied greatly from day to day. Because the variations were so small, it made it highly doubtful that meteorological effects could be seen as a result.[81]

Vigorously, and publicly, opposing this view was Dr. Charles Greely Abbot (1872-1973), Assistant Secretary of the Smithsonian Institution.[82] Abbot, who had been trained in chemistry and physics at MIT, was the second Director of the Smithsonian's Astrophysical Observatory. He was convinced that even small changes in the sun's output of heat could significantly effect the earth's weather. By correlating solar output with weather conditions over a period of several years, Abbot thought it would then become possible to use the solar radiation measurements alone to predict the weather – not just for the next day or so, but for many weeks, months or years in advance. The primary difficulty was in obtaining accurate measurements. This was made particularly problematic because, according

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[79] Weber, *The Weather Bureau*, 32.
[80] *Report of the Chief 1920-1921*, 22.
[81] *Report of the Chief, 1924-1925*, 8.
[82] For a short biographical sketch of Abbot's life, see David H. DeVorkin, "Charles Greely Abbot," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1998), 73:1-23.

to Abbot, a one percent change in solar radiation received at the earth's surface could produce noticeable weather effects. Therefore, measurement stations were moved out of the United States and down to an observatory in the Nitrate Desert of Chile because of its clear skies and limited rainfall. (Another station was established later on Mount Harqua Hala, Arizona.) Abbot argued that while the recorded change in solar radiation and weather might be small at the Chilean station because the affected ground area was so large, that same radiation could produce huge changes towards the poles. Therefore, it was not necessary to measure the insolation at the site of the forecasted change – one only needed to get an accurate measurement at a few optimally placed stations.[83]

The Weather Bureau, and its Chief, meteorological instrument specialist Charles F. Marvin (1858-1943), vehemently disagreed.[84] Marvin disputed Abbot's claims. Instead, he argued that the "variations" in solar radiation measurements observed by Abbot were not necessarily due to changes in the sun's output. Indeed, considering that the measurements were only taken after the sun's rays had passed through 20 miles of the earth's atmosphere, it was more likely that radiation variability depended on the state of the atmosphere and not on solar variability. Abbot countered that it made no difference whether the changes were on "the sun, the earth, or some distant star," if they enabled weather prediction.[85] Although Marvin agreed to collaborate with Abbot within the limits of the Weather Bureau's

[83] "The Sun and the Weather," *The Literary Digest* 85 (11 April 1925): 25-26.
[84] For a short obituary on Marvin, see E. W. Woolard, "Charles F. Marvin," *BAMS* 26 (1945): 237.
[85] "Long-Range Weather Forecasting," *The Outlook* 140 (20 May 1925): 88.

resources, Marvin clearly thought the entire idea of forecasting the weather based on solar measurements in a South American desert to be absurd.

By the fall of 1926, the controversy had gotten even hotter. Abbot was claiming that it was possible to predict the weather a year in advance by his solar radiation method. Articles in the popular press implied that the "fundamentalists" running the Weather Bureau were just too conservative to embrace this revolutionary forecasting method. As John Billings, Jr., writing for *The Independent*, put it, "[Abbot's] pioneering with solar radiation forecasts has set the tom-toms of the conservative meteorologists beating wildly. The official Weather Bureau, plodding along carefully with day-to-day forecasts...would quickly crush [this theory] out of existence." Marvin had become so agitated while dealing with journalists over this issue that his boss, Secretary of Agriculture William Marion Jardine (1879-1955), ordered him to stop talking to the press and "[observe] the dignified silence compatible with his official position."[86] Despite Abbot's arguments, Marvin refused to introduce solar radiation measurements as a forecasting technique until scientific evidence pointed to a direct link between solar radiation changes and identifiable weather patterns.[87] In the meantime, the idea that

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[86] Billings, "Is the Sun Fickle?" For more information on Abbot and his research into the solar constant, see David H. DeVorkin, "Defending a Dream," *Journal for the History of Astronomy* 21 (Part 1) (1990): 121-136. There were numerous articles on this controversy in the popular press. In addition to those already cited, see Robert DeC. Ward, "Are Long-Range Weather Forecasts Possible?" *The Outlook* 140 (8 July 1925): 366-371;
"'Bickering' About the Weather," *The Outlook* 142 (27 January 1926): 131; "Science and the Weather's Secrets" and "The Weather Bureau is Skeptical," *The Outlook* 143 (28 July 1926): 428-429; Abbot, "The Weather And Radiation." Charges and countercharges also flew across the pages of *Science* in the mid-1920s for the benefit of the scientific audience.
[87] *Report of the Chief, 1924-1925*, 8.

the Weather Bureau was a reactionary, bunker-mentality ridden agency became more deeply entrenched in the American public's psyche. Even though the Weather Bureau eventually showed that Abbot's correlations had been due to seasonal variations in stratospheric ozone concentration, Abbot remained immune to their criticism.[88]

Abbot was not the only person to promote the influence of heavenly bodies on the weather. To the consternation of Weather Bureau leaders, sometimes these people were hired by the Department of Agriculture. In October 1934, Secretary of Agriculture Henry Wallace hired Larry Page, a statistician from Des Moines, Iowa, to conduct studies of the moon and the stars. Page, who considered stars the "key" to the weather, was appointed to find how these extraterrestrial bodies could aid long-range weather forecasting.[89] Spending money that the Weather Bureau did not have on what meteorologists considered a cockamamie idea must have demoralized the entire forecasting section, not to mention the new Weather Bureau Chief, Willis Gregg, who took over from Marvin in 1934.

Despite the arguments over using solar radiation measurements for forecasting, there was no argument over their use for agricultural purposes. These efforts included investigating the effect of shade cloth used by farmers, i.e., how much radiation needed by the plants was allowed to penetrate different types of cloth, and investigations into the amount of heat generated by orchard heaters to

[88] DeVorkin, "Defending a Dream," 127.
[89] "Calls Stars Weather 'Key'," *The Evening Star* (Washington, D.C.), 11 October 1934 (Found in the National Academy of Science Archives, Agencies and Departments, Agriculture, Weather Bureau, 1933-1935) [Hereafter NAS].

prevent citrus and other orchard crops from freezing.[90] The Bureau conducted experiments on orchard heating ("smudging") with the Army's Chemical Warfare Service to determine if the smoke barrages, which the Army used to cover troop movements in the field, were effective against frost damage. As a result of these studies, the Bureau concluded that the best way to protect vegetation from frost was to heat the surface layer by burning the cheapest fuel available.[91] These two investigations were directly related to preserving the economic value of agricultural commodities.

As noted above, areas of research outside of meteorology and climatology included studies of earthquakes and volcanoes. Seismological studies were included under the Bureau's umbrella because it bore a "sufficiently close relation" to what it did with weather studies. Assigned to take on earthquake duties in 1914, the Bureau's mission vis-à-vis earthquake studies and observations was to find and map fault lines and reduce damage to dams, bridges, and other similar public works structures by recommending locations away from potential areas of slippage.[92] Similarly, as noted above, the Bureau assumed responsibility for monitoring and studying the Kilauea volcano on the island of Hawaii in 1919. These studies included measurements of lava and examinations of the compositions and reactions of volcanic gases. The Bureau sought to determine any relation that might exist between volcanic activity and earthquakes, and volcanic emissions with air and

[90] Weber, *The Weather Bureau*, 32.
[91] *Report of the Chief, 1922-1923*, 22.
[92] Weber, *The Weather Bureau*, 32-33.

water, and further if volcanic energy could be available "for the use of man."[93] The Geological survey took over the volcano studies in 1924.[94]

Although almost all of the Bureau's climatological research was related to agriculture, not all of it was. One specific climatological study undertaken during this period at the request of "other departments of the National government and for the use of the Peace Conference in Paris [1919]" dealt with the climate of Africa. In particular, the Weather Bureau was assigned to prepare a summary of African climate with special attention paid to former German colonies. The summaries included graphs of monthly precipitation and temperature values for the entire continent as well as a discussion of the general characteristics of the climate. What department made this intriguing request, or why, was alas not recorded.[95]

Of all its "research" tasks, however, the primary one was always forecasting – the improvement of short-term forecasts and the extending of the forecast period. The Weather Bureau routinely received requests "from all sides" for forecasts extending months, seasons and years ahead.[96] A Weather Bureau forecaster assigned to the Kansas City, Missouri station, reported that he was once asked – *in the winter* – to name a date six weeks in advance when the "sun would shine and [the weather would] be otherwise pleasant" so a bridge could be dedicated. He did so based on climatological information and, by the kind of miracle occasionally

[93] Weber, *The Weather Bureau,* 34.
[94] Weber, *The Weather Bureau,* 19.
[95] Weber, *The Weather Bureau,* 35.
[96] *Report of the Chief, 1922-1923*, 5.

bestowed upon weather forecasters, got it right.[97] However, that was a "no-skill" forecast: it was no better than climatology. Skillful long-term forecasts, if they were accurate, would of course be of huge benefit to many sectors. Farmers wanted to know ahead of time if there would be drought, too much rain, extremely high or low temperatures. Road crews and transportation industries wanted to anticipate especially bad winters which could impact their ability to keep goods moving. Manufacturers wanted lead time to produce items needed by consumers. Retail outlets wanted to know what they should order.

However, the Bureau's leaders were steadfast in noting that there were, to their knowledge, no "sound physical laws" which would allow such forecasts to be made with any degree of success. This was made more complicated by those outside the science of meteorology – including astrophysicists like Abbot, sociologists, and geologists – who claimed to have discovered methods of making accurate long-term forecasts. Even an economist fancied himself a long-range weather forecaster. The father of econometrics, American Henry Ludwell Moore (1869-1958), published a 29-page article in *The Quarterly Journal of Economics* which argued that the eight-year generating cycle in England, the eight-year crop cycles in England, France, and the United States, and the eight-year meteorological cycles could all be tied back to the motion of the planet Venus with respect to the

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[97] Patrick Conner as told to Courtney Ryley Cooper, "Sunshine for Saturday, *Please!*" *The American Magazine* 106 (July 1928): 105.

earth and the sun.[98] The Bureau was left in the position of sorting through public demands for long-range forecasts based on questionable methods which, upon closer inspection, did not yield valid forecasts. While not denying that it was possible to make such forecasts, it did argue that until there was solid scientific research done on the problem, forecasting would not advance much beyond where it was.[99]

Into the early 1930s, the Bureau was still defending its stance against long-range forecasts made without scientific underpinnings acceptable to the meteorology community, i.e., forecasting methodologies that did not include the physical processes of the atmosphere. Chief Marvin's 1930-1931 report stated that there was no "real way" to make long-range forecasts. The Bureau was familiar with the literature on the subject, and the methods which had been explored could be summed in three categories: (1) an examination of physical processes which would lead to a specific weather condition, (2) periodicities or cyclical recurrences which correlated astronomical or other sequences of events with a specific weather event, or (3) mathematical correlations between current weather in one location and weather which had occurred in the past, either in the same location or in a different location. However, none of these methods had resulted in any techniques which would extend the forecast period; i.e., they were "no-skill" forecasts. In order for a

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[98] Henry Ludwell Moore, "The Origin of the Eight-Year Generating Cycle," *The Quarterly Journal of Economics* 36 (November 1921): 1-29.
[99] *Report of the Chief, 1922-1923,* 5.

forecast to indicate skill, it had to be better than a forecast derived from climatology data and persistence, i.e., the next day would have the same weather.[100]

Bureau officials admitted that they had made very little progress in forecasting the weather in many years. However, by using radio (to improve observational data transmission) and airplane observations, which extended knowledge of the vertical structure of the atmosphere, Bureau personnel hoped to expand their understanding of atmospheric dynamics which would aid in attacking the forecasting problem.[101] But unbeknownst to Bureau officials, a storm was brewing on the horizon which would profoundly impact their operation.

The Weather Bureau leaders knew the Bureau had functional areas which needed improvement, but basically viewed their work as being the best a consistently meager budget would allow. The American Society of Civil Engineers (ASCE), however, was not content with the services received by the engineering community. In April 1931, the ASCE Board of Directors appointed a special committee to "give thought as to how the United States Weather Bureau could be made of greater service to engineers." The five-member committee presented its report at the ASCE Annual Meeting held 18 January 1933, and published the report in the January 1933 issue of the *Proceedings of the American Society of Civil Engineers*. The extremely detailed 29-page report laid out deficiencies in the Weather Bureau's operation vis-à-vis meteorological observation station data as

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[100] *Report of the Chief of the Weather Bureau, 1930-1931* (Washington, D.C.: Government Printing Office, 1932), 4.
[101] *Report of the Chief, 1933-1934*, 2.

seen by the engineering profession. It was followed by a series of letters, both pro and con, which appeared in five subsequent volumes of the *Proceedings* during 1933. This report hit a raw nerve. Consequently, the ensuing uproar did not experience a quick death. Briefly, the engineers attacked the placement of observation stations, the handling of the resultant data, and the format in which the data were made available to the engineering profession. They also impugned the scientific standing of the Weather Bureau, which, they charged, "[had] not kept pace...with research in other lines of science, either pure or applied." After producing a laundry list of recommendations, the committee members closed by recommending that when the current chief (Marvin) retired, the President of the United States should appoint his successor from the ranks of those who were experienced administrators, possessed "broad fundamental science training," and possessed the "rare qualities of mature judgment and progressiveness. Further, the new chief had to be a "courageous [and] diplomatic leader, who will release the latent abilities now bound by archaic tradition." There was one additional caveat: the new chief did not need to be a meteorologist.[102]

The ASCE was not the only group complaining. The Navy had been stung by the crash of the rigid airship USS *Akron* (ZRS-4) on 4 April 1933. *Akron* had been operating off the coast of New England when high winds forced her into the

[102] "Meteorological Data: Progress Report of Special Committee," *Proceedings of the American Society of Civil Engineers (PASCE)* 59 (1933): 153-156. Letters, pro and con, appeared throughout 1933 under the title "Meteorological Data: Progress Report of Special Committee, Discussion." See *PASCE* 59 (1933): 708-720 (April), 896-900 (May), 1024-1035 (August),1195-1200 (September), and 1340-1343 (October). For Chief Marvin's rebuttal, see pages 709-718.

water and she sank. The accident killed 73 men including the then Chief of the Bureau of Aeronautics, Rear Admiral William A. Moffett.[103] A joint congressional committee investigated the crash and the Navy held a court of inquiry to determine the causes of the disaster. Since high winds had forced the airship to ditch, all eyes turned to the data provided by the Weather Bureau. While Navy aerologists were routinely required to provide detailed forecasts for periods longer than a day to aviators, the Weather Bureau aviation forecasts were for only twelve hours and were quite vague. Of even greater importance, the Navy's Bureau of Aeronautics had been stressing the importance of taking four weather observations per day (instead of the two then being taken by the Weather Bureau) to the Secretary of Agriculture for several years. The Agriculture Secretary had taken no action. With the *Akron* disaster, the Navy wanted action, and was being backed up by the congressional investigating committee.[104] Both the Weather Bureau and the Secretary of the Agriculture were under extreme pressure to change their operations and to do so quickly.

The loss of *Akron* and the ASCE report caused a firestorm that came to envelop not only the Weather Bureau, but the Secretary of Agriculture: Henry A. Wallace (1888-1965). Wallace, whose father Henry Cantwell Wallace (1866-1924)

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[103] For more on *Akron*, see Richard K. Smith, *The Airships Akron and Macon; Flying Aircraft Carriers of the United States Navy* (Annapolis: U.S. Naval Institute, 1965). For a brief discussion of airships in general as well as *Akron*, see Richard K. Smith, "The Airship, 1904-1976," in Eugene M. Emme, ed., *Two Hundred Years of Flight in America: A Bicentennial Survey* (San Diego, CA: American Astronautical Society, 1977), 69-108.
[104] RADM E. J. King to the Secretary of the Navy (H. L. Roosevelt, Acting), 8 July 1933 and 14 July 1933 (NAS, Executive Board Science Advisory Board, Committee on Weather Bureau, General 1933).

had been the Secretary of Agriculture in the early 1920s, was a graduate of Iowa State College. He had worked on the family's paper, *Wallace's Farmer*, becoming editor when his father took the Agriculture Department post. Wallace was also a plant geneticist working on corn hybridization.[105] He was very interested in the connection between weather and crops, and had close ties to the Weather Bureau. However, the ASCE report, despite having little merit, had become a political hot potato. The *Akron* disaster had the President's attention. Wallace had to address the issues raised, or he would find himself under fire for supporting a purportedly non-scientific scientific bureau which could not provide the minimal weather support required for the safety of aviation interests. Looking for a way out of this potential quagmire, Wallace found the solution: the Science Advisory Board.[106]

Established to study the functions, relationships, and programs of the government's scientific agencies, President Franklin D. Roosevelt created the Science Advisory Board (SAB) by Executive Order 6238 on 31 July 1933. The

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105 "USDA Past Secretaries," found on the USDA website: http://www.usda.gov/history/pastsec.htm, 15 March 2003. There have been numerous books written about the many facets of Henry A. Wallace's life. For an introduction to the literature, see for example, John C. Culver and John Hyde, *American Dreamer: A Life of Henry A. Wallace* (New York:: Norton, 2000); Edward L. Schapsmeier and Frederick H. Schapsmeier, *Henry A. Wallace of Iowa: The Agrarian Years, 1910-1940* (Ames, Iowa: Iowa State University Press, 1968); Graham White and John Maze, *Henry A. Wallace: His Search for a New World Order* (Chapel Hill, NC: University of North Carolina Press, 1995).

106 There has been little written on the inner workings of the Committee on the Weather Bureau. For more on the Science Advisory Board and its work during its short (1933-1935) existence, see Dupree, *Science in the Federal Government*, 350-358; Joel Genuth, "Groping Towards Science Policy in the United States in the 1930s," *Minerva* 35 (1987): 238-268; Robert Kargon and Elizabeth Hodes, "Karl Compton, Isaiah Bowman, and the Politics of Science in the Great Depression," *Isis* 76 (1985): 301-318; Lewis E. Auerbach, "Scientists in the New Deal: A Pre-War Episode in the Relations Between Science and Government in the United States," *Minerva* 3 (1965): 457-482; Carroll W. Pursell, Jr., "The Anatomy of a Failure: The Science Advisory Board, 1933-1935," *Proceedings of the American Philosophical Society* 109 (1965): 342-351.

SAB would operate under the auspices of the National Research Council (NRC). Roosevelt named MIT President Karl T. Compton (1887-1954) as chair of the nine-member board.[107] Board members would offer recommendations on how to increase the efficiency of the agencies they studied, and aid the nation in exploiting its scientific expertise. In particular, the board was concerned with this question: "[How] far should Government itself go in conducting or supporting research or guiding the applications of scientific discoveries?"[108]

Wallace contacted National Research Council Chairman, geographer Isaiah Bowman (1878-1950), for help. Bowman recommended that World War I weather training coordinator and Nobel Prize winning physicist Robert A. Millikan (1868-1953), and fellow physicist, MIT President Karl T. Compton (1887-1954), serve on an advisory committee dedicated to addressing Weather Bureau problems. Because of his geographical training, Bowman also suggested that Wallace consider the statistical records kept by the Bureau and how they might come to bear on atmospheric problems. Wallace, who had been on the job for less than six months, was frustrated with the lack of research funding available for the Weather Bureau. He felt "helpless" to answer the criticisms being heaped upon the Bureau, and consequently, on the Department of Agriculture. In the darkest days of the Great

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[107] Executive Order No. 6238, Appointment of the Science Advisory Board by President Franklin D. Roosevelt, 31 July 1933. The other members were: W. W. Campbell, President, National Academy of Sciences; Isaiah Bowman, Chairman, National Research Council; Gano Dunn, President, J. G. White Engineering Corporation; Frank B. Jewett, Vice-President, American Telephone and Telegraph Company; Charles F. Kettering, Vice-President, General Motors Corporation; C. K. Leith, Professor of Geology, University of Wisconsin; John C. Merriam, President, Carnegie Institution; R. A. Millikan, Director, Norman Bridge Laboratory of Physics (Caltech).
[108] Karl T. Compton, Chairman, "Report of the Science Advisory Board, July 31, 1933 to September 1, 1934" (Washington, D.C., 20 September 1934), 11.

Depression and with huge agricultural problems waiting to be fixed, Wallace did not have time to be encumbered by Weather Bureau problems. He was, therefore, enthusiastic about Bowman's idea to bring in "outside meteorological interests" to effect the improvement of weather services, which would not only advance science, but would contribute to the nation's defense posture.[109]

At Wallace's request, the SAB created the Committee on the Weather Bureau in late August 1933. The members: Millikan (chairman), Compton, and Bowman.[110] This committee, as structured, had no meteorologists as members. Therefore, Compton asked Weather Bureau meteorologist Charles D. Reed of the Des Moines, Iowa weather office to serve.[111] Thus, the committee assigned to "assist" the Weather Bureau was unlike any of the others formed to study the government's scientific agencies. It was the only one composed of scientists who were *not* subject matter experts in the dominant discipline of the agency under consideration. Just as the astrophysicists felt entitled to claim that the sun alone determined the weather, two physicists and geographer apparently had a superior grasp of meteorological problems than did the meteorologists.

The committee, less Reed, met with Chief Marvin on 26 August 1933. Marvin, apparently oblivious to the fact that his days as Chief were numbered, was "immensely pleased," according to Bowman, with both the committee's

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[109] Isaiah Bowman to Robert A. Millikan, 3 August 1933 (Robert A. Millikan papers, California Institute of Technology Archives, B9, Folder 9.9) [Hereafter **Millikan papers**].
[110] Progress on the Committee on Weather Bureau, 4 December 1933 (NAS, Executive Board SAB, Committee on WB, General 1933).
[111] Karl T. Compton to Charles Dana Reed, 24 August 1933 (NAS, Executive Board SAB, Committee on WB, General 1933).

composition and their mission. Marvin promised his full cooperation to Millikan's committee.[112]

The committee members soon focused on meteorological research. The German-born seismologist Beno Gutenberg, who had introduced meteorology courses under the geophysics umbrella at Caltech, and Lieutenant Commander Francis W. Reichelderfer, the senior Navy Aerologist, had both provided written statements about the importance of introducing air-mass analysis methods. This method, introduced by Vilhelm Bjerknes and his son Jacob at the Geophysical Institute in Bergen, Norway – known in scientific circles as the "Bergen School" – had been available since the early 1920s.[113] Indeed, as will be discussed below, Reichelderfer had already introduced these techniques to the Navy.[114] While it did not appear that air-mass analysis methodologies would significantly lengthen the forecast period, committee members believed they would lead to increased accuracy over the 24 to 36 hour forecast period.[115]

Wallace was expecting the committee's first report on 1 November. Millikan volunteered to write the first draft. Committee members agreed that if they were to make their points about Weather Bureau structure and expansion of research opportunities, they would need to make the case for the economic benefits

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[112] Isaiah Bowman, Minutes of SAB Committee on the Weather Bureau, 22, 25, and 26 August 1933 (NAS, Executive Board SAB, Committee on WB, General 1933).
[113] For a detailed discussion of the Bergen School see Friedman, *Appropriating the Weather.*
[114] Science Advisory Board, Minutes of the Meeting of the Committee on Meteorological Research, 24 September 1924 (NAS, Executive Board SAB, Committee on WB, General 1933).
[115] Science Advisory Board, Committee on Meteorological Research, Record of Meeting September 25, 1933 (NAS, Executive Board SAB, Committee on WB, General 1933).

which would be received by agriculture, commerce, and aviation. The report would include their recommendations on the adoption of air-mass analysis techniques and the full range of Weather Bureau functions. However, committee members would not concur in the report of the American Society of Civil Engineers, which they found lacking. Committee members thought the ASCE report had failed to appreciate the Weather Bureau's many responsibilities and the way it carried out its functions. Thus, one precipitating event for the committee's formation had been dispatched. Wallace would not need to worry about the engineers' narrowly defined complaints.[116]

The committee still needed to address the matter of replacing the Weather Bureau Chief. In early October, retired Weather Bureau meteorologist Oliver L. Fassig paid a call on Bowman. While the appointment was ostensibly to talk about trade winds (Fassig was working on a study of trade wind flow in Puerto Rico), Fassig's real mission was to discuss Marvin's replacement. Fassig argued that *no one* in the Weather Bureau had ever encouraged research. To his way of thinking, the Bureau still suffered from "the old army spirit" from which it had sprung. The Bureau needed someone from outside to come in. Fassig, however, could only think of one person he would recommend to be the new chief: Willis R. Gregg, a longtime Weather Bureau meteorologist. Perhaps more importantly, Fassig was

[116] Ibid.

worried that political influences could lead to a choice that would ultimately be detrimental to the Weather Bureau's best interests.[117]

By mid-November, the committee had a preliminary report – which did not include a recommended replacement for Marvin – ready for Secretary Wallace. The report's primary recommendation: that the Weather Bureau adopt the Bergen School methods of air-mass analysis and do so immediately with the aid of the Army and Navy. The Weather Bureau needed the cooperation of the military services in order to expand the upper air observation system within the Bureau's appropriated funds. Secondly, the report recommended that all data reporting and recording be assigned to the Weather Bureau.[118] To fulfill this recommendation, the Bureau needed to find and hire meteorologists who had training and experience in air mass analysis. It also needed daily reports of temperature and humidity up to three to four miles above the earth's surface throughout the country and more frequent and detailed surface reports from both terrestrial and oceanic stations. Even if the people and the reports were in place immediately (an impossibility due to the substantial across-the-board funding cuts due to the Great Depression, which affected all government agencies), the Bureau estimated that it would take 3-5 years to introduce the techniques to experienced forecasters. Upper-air reports would be available with the assistance of Army and Navy reporting stations. The

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[117] Bowman to Millikan, 2 October 1933 (NAS, Executive Board SAB, Committee on Weather Bureau, General 1933).
[118] Millikan to Wallace, 15 November 1933 (NAS, Agencies and Departments: Agriculture, Weather Bureau 1933-1935). Report of the Science Advisory Board, July 31, 1933 to September 1, 1934 (Washington, D.C., 20 September 1934), 53.

additional surface reports were problematic: stations were only equipped to report twice a day and they would need at least four reports per day taken simultaneously around the country to be able to produce the four maps/day required by the Norwegian method.[119] Since no additional money had been appropriated for this expansion of data collection, the Bureau could only hope to make limited progress on the introduction of new forecasting techniques.[120] If it did not study and vigorously apply the results of new scientific work, the Bureau realized that it would fall "hopelessly" behind other similar institutions.[121] Indeed it already had. The Europeans were expending more money on research and applications than was the United States, and the Bergen School techniques were already in use on the Continent.[122]

While not heavily engaged in what would normally be called "research," the Weather Bureau was responsible for publishing the only journal – albeit non-peer-reviewed – devoted to meteorological research in the United States: *The Monthly Weather Review (MWR)*. In addition to publishing articles on scientific advances both at home and abroad, the *MWR* published current and average weather conditions. The journal also aimed to eradicate "false ideas, which everywhere abound respecting the weather" and to assist those providing meteorological

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[119] *Report of the Chief, 1933-1934*, 1.
[120] *Report of the Chief of the Weather Bureau, 1934-1935* (Washington, D.C.: Government Printing Office, 1936): 1.
[121] Ibid., 7.
[122] Kimball, "Recent Advances," 4.

instruction in secondary schools and higher education institutions.[123] Additionally, *MWR* fulfilled the obligation of the United States to the wider international meteorological community of providing observational and statistical data related to meteorology and climatology.[124] In return, the Weather Bureau received like information from other nations.[125] *MWR* articles included investigations of upper air phenomena (including the strength and direction of air currents), protection of agricultural products from weather extremes, and the role of weather in health related issues (physiological meteorology). As the only journal providing an outlet for publishing longer meteorological research articles, *MWR* was greatly affected by depression era funding reductions. In 1932, *MWR* editor W. J. Humphreys eliminated all articles as a cost-cutting measure – only the data portions remained. This action temporarily eliminated the one medium for exchanging new meteorological information and thus further dampened the ability of the discipline to advance.[126] Funds were restored almost a year later, at which point Humphreys requested immediate submissions of completed articles.[127]

Monthly Weather Review may have been the most visible research-related line item to be cut, but it was not the only one. The entire government research budget was reduced by 12.5% in 1932. That included the Weather Bureau's

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[123] *Report of the Chief, 1920-1921*, 20.
[124] *Report of the Chief, 1922-1923,* 10.
[125] *Report of the Chief, 1923-1924,* 18.
[126] Charles F. Brooks, "*Monthly Weather Review* Halved," *BAMS* 13 (1932): 154.
[127] S. D. F., "News Item," *BAMS* 14 (1933): 212.

scientific work that did not fall directly under the heading of research.[128] This loss of funding directly affected the Bureau's ability to pursue its climatological work.

An even greater problem than its paltry, virtually non-existent research budget, was the Weather Bureau's inability to hire and keep scientifically trained staff members due to chronic under-funding. Although the Bureau's personnel situation most certainly deteriorated during the severe cutbacks of the Great Depression, it had been dogged by personnel problems for many years.

The War and Navy Departments had few personnel trained in meteorology prior to the entry of the United States into World War I. The greatest numbers of people with meteorological training – professional and technical – resided within the Weather Bureau. Therefore the Weather Bureau was responsible for providing both personnel and training to the war effort in addition to expanding services to military aviation. Despite the resulting increase in demand for services, the Bureau experienced a *decline* in personnel starting in 1914, while foreign meteorological bureaus were simultaneously expanding.[129] Funding did not keep up with expenses or the expansion of services. Congress turned down a request for additional fiscal year 1921 appropriations to cover aerological work in support of both military and civil aeronautics, data-gathering and forecasting in support of marine meteorology (the Weather Bureau was responsible for open ocean forecasting), and data-

[128] From Science Service: "Nine and a Half Million Cut from Government Research," *BAMS* 13 (1932): 154. See also Dupree, *Science in the Federal Government*, 343-350.
[129] "Proceedings of the First Meeting," *BAMS* 2 (1921): 13. This decline in the Weather Bureau's posture in the face of foreign expansion was used in support of an American Meteorological Society resolution encouraging Congress to increase the Bureau's appropriations.

gathering and forecasting related to fire-weather, fruit-frost, and other specialized agricultural-related missions.[130] As reported by the Weather Bureau Chief in his annual report for 1919-1920, stagnant appropriations coupled with rapidly rising costs for goods and services were crippling his ability to meet new obligations. There was an inadequate number of stations to support aircraft forecasts, even with army and navy stations added into the mix; limited personnel had forced cut-backs in services; and demands by the insurance industry for timely, accurate data (provided a no- or low-cost) were taking their toll. As Marvin, the Chief, put it, "In general terms, the Weather Bureau is suffering from the ravages of the war and the consequences of an enormous change in economic conditions. Its work is conducted under strained conditions by a faithful personnel, largely discouraged by the slow and inadequate adjustment of Federal compensations to existing conditions of life."[131] During the 1920s, more and more employees were leaving the WB – some after as many as 30 years on the job – because their salaries were not high enough to support their families. One hundred percent of the lower grades turned over each year. The Weather Bureau was serving as a training ground for meteorological observers who would then leave for better paying jobs elsewhere.[132] Meteorologists with a bachelor's degree working for the Army Signal Service earned more than $2500/year to start while Weather Bureau meteorologists with

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[130] "Some Weather Bureau Projects," *BAMS* 1 (1920): 11-12. The request included $200K for aerological work, $50K for marine meteorology, and $15K for fire-weather forecasting.
[131] *Report of the Chief of the Weather Bureau, 1919-1920* (Washington, D.C.: Government Printing Office, 1921).
[132] "Efficiency of the Weather Bureau Endangered," *BAMS* 1 (1920): 140.

master's degrees and 10 years of experience only earned $1800/year.[133] The salary discrepancy between the Weather Bureau and other science-based agencies was not limited to the lowest levels. In 1921, the Weather Bureau Chief was paid $5000 to lead an organization with over 200 stations and a $2 million budget. The Chief of the Office of Experiment Stations received the same salary – only he supervised an organization with just five stations and a budget of $250,000. The Chief of the Bureau of Chemistry and Soils was paid $8000. His organization's budget: just $1.3 million.[134] Other agencies show similar discrepancies between wages paid and corresponding levels of responsibility. No wonder the Weather Bureau, as an organization, carried itself in the manner of one who has been constantly put-upon. Bureau employees were put-upon. They were not given the respect, and the corresponding remuneration, accorded to the employees of other scientific agencies.

The pay situation did not improve, even marginally, until the 1920s. Positions were reclassified in 1924 in an effort to align the pay of similar positions across agencies, but Weather Bureau employees were still insufficiently paid considering their level of education and training, responsibilities, and length of service compared to others in government service.[135] By the early 1930s, the Bureau was looking for more people due to the expansion of aviation forecast

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[133] Charles F. Brooks, "Reclassification," 163-164. $1800/year was less than most shop employees earned at the Bureau of Standards or clerks earned with the Department of Agriculture's Office of Experiment Stations.

[134] Weber, *Weather Bureau,* 49, 70; Conover, *Experiment Station,* 128, 166; Weber, *Chemistry and Soils*, 165, 188.

[135] "Increased Pay for Weather Bureau Employees," *BAMS* 9 (1928): 120.

services. However, it was having a difficult time finding enough men (there were no women in the Weather Bureau) with adequate training. More senior grades required degrees in mathematics or physics, preferably with some meteorology courses. However, so few colleges offered separate meteorology courses, the Bureau could not make such courses a requirement for employment. All positions, regardless of educational background, were filled by competitive Civil Service examination.[136]

The personnel situation deteriorated further once the government instituted economy measures dictated by the Great Depression. In mid-1932, all men over age 70, with few exceptions, were immediately retired. The Weather Bureau lost 25 of its most senior people – including two-thirds of those with earned Ph.D.s. Only Chief Marvin, a meteorological physicist, and the head of the New Orleans field office were retained. Most of those retired had been heading field stations – a position for which years of experience were the most important indicator of probable success. Those remaining within the system lacked equivalent education and training. This situation adversely affected the Bureau's ability to provide effective weather services.[137]

The Weather Bureau faced continued reductions in funding and personnel losses in 1933. Congress appropriated $400,000 less for the Bureau for fiscal year 1934 than for the previous year. It then imposed a spending limit that was an

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[136] "Personnel of the Weather Bureau," *BAMS* 11 (1930): 71. Originally printed in the *Christian Science Monitor*.
[137] Charles F. Brooks, "Many Government Meteorologists Retired," *BAMS* 13 (1932): 153-154.

additional $800,000 below that – a total loss of $1.2 million, yielding a final budget total just shy of $3 million. The result: 500 Bureau employees were laid off and more than 20 first-order stations (including Fort Worth, Texas; St. Paul, Minnesota; and San Jose, California) closed, along with a large number of substations. A number of departments (particularly Agriculture and Commerce) which depended upon the Bureau for their weather support lost those services with the budget cuts.[138] Worse yet for the Bureau, the losses were again those of senior personnel. Many of those with 30 or more years of service were involuntarily retired. Some of the remaining employees moved down into lower positions in order to maintain their jobs.[139] And, along with everyone else in government, they took a 15% cut in pay which was not helpful for recruiting anyone who was even anticipating a career as a Weather Bureau meteorologist.[140]

Weather Bureau personnel had many concerns during this period: loss of jobs, loss of pay, loss of funding for goods and services, and the subsequent pressure to provide consistent, high-quality weather services as they had in the past. Therefore it must have been especially irksome to be on the receiving end of complaints by the American Society of Civil Engineers (discussed above) which declared Weather Bureau data to be not user-friendly. An ASCE committee that had looked into the Bureau's methods found personnel to have an "inferiority complex" compared to those working elsewhere in the Department of Agriculture,

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[138] S. D. F., "Retrenchment in Weather Bureau Activities," *BAMS* 15 (1934): 29-30. *Report of the Chief, 1932-1933,* 3.
[139] "Effect of Economy Program on U.S. Weather Bureau," *BAMS* 14 (1933): 182-183.
[140] "Weather Bureau Salaries," *BAMS* 14 (1933): 112-113.

enhanced by weaker educational backgrounds and inadequate equipment for scientific investigations. Oliver L. Fassig, the former chief of the climatology division (one of those involuntarily retired because he was over 70), fired back that the Weather Bureau did not exist to support "special interest" groups. Furthermore, the Bureau was still hampered by the attitude towards meteorology as a science that existed during its establishment in the late 1800s, i.e., that it was not a "real science" like physics or chemistry. As a result, it still continued to suffer from long-standing "poor intellectual visibility."[141]

The Bureau started its climb out of this situation with the hiring of three young meteorologists with newly-earned MIT Ph.D.s in 1935: Horace Byers, Harry Wexler and Stephen Lichtblau. Their mission was to bring Science Advisory Board-recommended Norwegian polar-front and air-mass theory to the Bureau. Their mandate was to first study how Bergen School techniques could be applied to the North American weather problem. Then these young professional meteorologists would introduce the Bergen techniques to those in the field. But the addition of these three young men did not markedly improve the educational profile of the Bureau. Byers surveyed personnel in the late 1930s and found that only 27% of "professional personnel" had any college degree (half of which were in science or engineering). By 1939, there were *five* Weather Bureau employees with actual

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[141] Oliver L. Fassig, "The Weather Bureau," *BAMS* 14 (1933): 111-112. For the arguments surrounding the appropriate bureaucratic home for the nation's weather service in the 1880s, see Dupree, *Science in the Federal Government,* 187-192.

degrees in meteorology.[142] This dearth of professionally educated meteorologists was largely due to the low opinion held by academia about meteorology as a scientific discipline before World War II – a subject addressed in the next chapter.

Stagnant, and then dwindling, appropriations kept the Weather Bureau locked into a routine from which it could not escape. Furthermore, reduced funding exacerbated already low salary levels and the Bureau's inability to expand observation stations: terrestrial, upper air, and oceanic. With no research budget, the Bureau was unable to analyze the data that it collected from over 5000 voluntary observers around the country. With little real support from Congress, it had not sought out new ways of analysis and forecasting until the Science Advisory Board report provided the impetus to take the necessary steps towards adopting the Bergen School techniques. As the 1930s closed, long-range forecasting and objective forecasting techniques appeared to be on a very distant horizon.

LOOKING ABROAD FOR INSPIRATION: NAVY AEROLOGY

Like the Weather Bureau, the Navy Aerological Service operated on a shoestring – limited funds, limited manpower, and virtually no support from the "battleship" admirals that ran the Navy. Unlike the Weather Bureau, the Navy looked to the Bergen School and adopted its methods. Indeed, the Navy's early adoption of the Bergen School methods would form, shape, and strengthen the professional

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[142] Horace R. Byers, "The Founding of the Institute of Meteorology at the University of Chicago," *BAMS* 57 (1976): 1343.

relationships between several major figures who would eventually influence the development of numerical weather prediction in the United States.

Having transferred its marine meteorological service (minus the weather information plotted on pilot charts) to the Weather Bureau in 1904, the Navy paid scant attention to meteorological services until the advent of the First World War. Then, demands from aviation units forced the Navy to expand its meteorological mission. Once the war was over, the issue of which agencies would provide what meteorological services reached a critical turning point. In September 1919, President Wilson convened an inter-departmental board to determine the future provision of meteorological services for the military and for civilians. The board made three recommendations:

(1) that the Weather Bureau be responsible for collecting and disseminating information;

(2) that the military (War and Navy Departments) maintain skeleton meteorological organizations which could be ramped up and deployed quickly to field sites in the event of a national emergency; and

(3) that all three groups should coordinate station placement so as to avoid duplication of effort.[143]

However, it soon became obvious that the Weather Bureau was not always going to have a station near every naval activity. Naval stations were scattered up and down

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[143] Frederick J. Nelson, "The History of Aërology in the Navy," *U.S. Naval Institute Proceedings* 60 (1934): 524. The concern with duplication of effort between the civilian Weather Bureau and the military meteorological organizations is a recurring theme that was periodically resurrected at least through the 1980s (author's personal experience).

the Atlantic and Pacific coasts as well as all along the Gulf Coast. As the Secretary of Agriculture, David Franklin Houston, wrote to Josephus Daniels, the Secretary of the Navy, on 14 January 1920:

> It is fully recognized that certain meteorological work and observations must of necessity be conducted by the Navy in connection with its operations at base stations and on vessels at sea, but such work does not involve duplication of effort. In fact, stations so maintained by the Navy will supplement those of the Weather Bureau and be valuable to it.[144]

The Navy's aerological mission would be to provide "detailed weather information to naval aviators and aeronauts" and to provide local weather forecasts when a Weather Bureau office was not close by.[145] While not appearing to be too onerous a task, with only five officers and two enlisted personnel left over from war service, the Navy was far from able to meet all the incoming requests for meteorological support.

Because virtually all weather observing and forecasting tasks had been absorbed by the Weather Bureau in the earliest days of the twentieth century, the Navy was unprepared to fill a rapidly expanding need for meteorological support. It had no meteorological specialists and only a few pieces of basic equipment. Naval Air Stations, in particular, were very interested in obtaining allowances for meteorological equipment and personnel.[146] However, navy personnel were

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[144] Quoted in Nelson, "The History of Aërology in the Navy," 524.

[145] C. N. Keyser, "Aerological Work in the U.S. Navy (Abstract)," *BAMS* 1 (1920): 6-7.

[146] The Navy maintains allowance lists which provide guidelines on what types and quantities of equipment may be purchased by different kinds of organizational units. Similarly, there are allowances for people with different vocational and professional specialties. In order to obtain the personnel and equipment to support aeronautics, the Naval Air Stations had to first justify the

unfamiliar with meteorology, and civilian meteorologists were unfamiliar with the navy, so it took some time to put a new navy meteorological organization together. Starting in 1917, Dr. Alexander McAdie, Director of the Blue Hill Meteorological Observatory, associated with Harvard University, began to provide meteorological training to officers in conjunction with the MIT ground school for aviators. At the request of then Assistant Secretary of the Navy, Franklin D. Roosevelt, McAdie accepted a reserve commission as a lieutenant commander in January 1918 so that he might determine the Navy's aerological needs and organize an aerological service.[147] Shortly thereafter, enlisted personnel started receiving meteorological training at Pelham Bay, Long Island, New York. The Navy shipped a total of nine officers and fifteen enlisted men to England for further training with the British Meteorological Office and then on to European assignments for the duration of the war. At war's end, 50 officers and 200 enlisted men provided meteorological services to a variety of navy activities.[148]

By the time it was determined that the Navy would need to provide for its own aviation mission and local forecasts when Weather Bureau stations were not

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requirement so that higher authority would modify their allowance. Then they could seek to have the allowance filled. However, having an allowance did not guarantee equipment or people.

[147] For more information on the "aerological" training of naval officers during World War I, see John H. Conover, *The Blue Hill Meteorological Observatory: The First 100 Years – 1885-1985* (Boston: The American Meteorological Society, 1990), 144-149.

[148] Nelson, "History of Aerology," 523. See also Bates and Fuller, *Weather Warriors*, 23-29. The definitive work on Harvard's scientific achievements, Clark A. Elliot and Margaret W. Rossiter, editors, *Science at Harvard University: Historical Perspectives* (Bethlehem: Lehigh University Press, 1992) mentions meteorology only in passing and notes that meteorology at Harvard has received little attention outside of the Conover book on Blue Hill. For a description of the early days of the Blue Hill Observatory by a participant, see Alexander G. McAdie, "The Blue Hill Observatory, 1884-1929," in Samuel Eliot Morison, ed., *The Development of Harvard University Since the Inauguration of President Eliot, 1869-1929* (Cambridge, MA: Harvard University Press, 1930), 549-554.

close by, it was obvious that the seven remaining meteorological personnel could not meet all of the Navy's mission-specific meteorological requirements. There were not only naval activities along the thousands of miles of United States' coastline, but there were naval activities overseas, and ships at sea that also needed weather support. Because weather conditions were so critical to safe flying operations, the Navy established a School of Meteorology at Naval Air Station (NAS) Pensacola, Florida – home of the flight school. Training both officers and enlisted personnel, it included instruction in the science of meteorology and its applications to naval operations. Enlisted personnel took a four-month long course which prepared them for assignments, at naval air stations, on aircraft tenders, and with other ships and stations. [149] In addition to this "in-house" instruction, the Weather Bureau provided some officers with two additional months of "post-graduate study" at their central office. Of the six officers who graduated from the basic course, three went directly to field assignments, while the other three moved to Washington, D.C. for further training. This "advanced" course included non-instrumental observations of weather, in particular clouds and their significance, discussions of flying weather, weather map construction, discussion and forecasts, and physics of the air. The Bureau gave free access to their library to visiting navy officers and to any of the meteorologists working in the central office.[150]

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[149] Aircraft tenders were ships whose mission was to provide routine maintenance and repair for seaplanes.
[150] C. N. Keyser, "From the Committees," *BAMS* 1 (1920): 15-25. "Naval Officers' Advanced Course in Meteorology at the Weather Bureau, Washington, D.C.," *BAMS* 1 (1920): 90-91.

Despite the Navy's laudable effort to establish a training program which would boost its numbers of meteorologically trained personnel, any naval officer who planned to maintain a successful career needed to spend a considerable amount of time either at sea or serving with the nascent aviation units. Consequently, receiving meteorological training was not high on the list of desirable career decisions for many officers. With insufficient volunteers, people who had little or no interest were drafted into the training courses. They stayed within the aerological program for the minimum time required and then transferred to more career-enhancing positions. The limited meteorological interest of these officers led to inefficient weather stations, and some rather serious aircraft incidents resulted from inattention to the weather. The most high profile of these incidents was the loss of the rigid airship USS *Shenandoah* in a line squall on September 3, 1925. Fourteen people died. Coming just one day after the disappearance of two of the Navy's PN-9 seaplanes in the Pacific, the case for the necessity of good weather support had been made.[151]

The Navy needed to take a different approach in order to maintain a cadre of highly trained meteorologists who could apply their knowledge to naval operations. The problem: line officers, i.e., primarily those who served on afloat units, already considered themselves to be good weather forecasters. They spent their lives at sea and had to be able to read the skies (as in, "Red sky at night, sailor's delight; red sky in morning, sailor take warning!") for indications of

[151] Nelson, "The History of Aërology in the Navy," 525. See also Bates and Fuller, *Weather Warriors*, 37-38.

weather conditions. Therefore, they felt no need for advanced training. Further, remaining in a specialty area like aerology would have effectively ended their careers. Promotions depended upon filling "combat" positions aboard ship. Making weather forecasts to aid the fleet was not sufficient to guarantee advancement. Despite the training program in Pensacola, by 1925 there were only two officers practicing meteorology in the navy. One of them was destined to become the Chief of the Weather Bureau: Francis W. Reichelderfer.[152]

Francis Wilton Reichelderfer (1895-1983) had graduated from Northwestern University with a degree in chemistry in 1917 just as the United States was entering the Great War. Joining the naval reserve with the intention of becoming a pilot, he signed up for meteorology training and was assigned to McAdie's training unit at Blue Hill. Reichelderfer did earn his wings after the war was over, but remained in the meteorological field. By 1922, he was the head of Navy Aerology (a position he held until 1928) and occupied the Navy's desk at Weather Bureau headquarters, where he filled a liaison function while pursuing his own studies of the Bergen School techniques.[153] With demand for aviation forecasting increasing while the numbers of meteorological practitioners dwindled, Reichelderfer decided that the only solution was to establish a post-graduate course for Navy meteorologists. By then a lieutenant commander, Reichelderfer and the Assistant Navy Secretary for Aeronautics, MIT aeronautical engineering professor

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[152] Bates and Fuller, *Weather Warriors,* 37.

[153] Jerome Namias, "Francis W. Reichelderfer," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1991), 60: 274-277. For an interview with Reichelderfer, see Hessam Taba, *The "Bulletin" Interviews* (Geneva: World Meteorological Organization, 1988): 85-98.

Edward P. Warner, established a two year post-graduate course in meteorology in 1926.[154] Reichelderfer argued that the importance of weather information for aviation missions was new and distinct from the previous use of forecasting to insure safety at sea. The Weather Bureau took care of marine forecasts. The aviators needed special weather information, e.g., cloud layers, fog, strong winds, in order to make decisions on launching aviation missions which could include scouting and bombing activities. Because that kind of detailed information could not be transmitted via teletype, an officer needed to be on-site to provide "over-the-counter" briefings and to answer questions.[155]

The first year of this new course, emphasizing advanced physics and mathematics, was held at the Naval Postgraduate School on the campus of the U.S. Naval Academy in Annapolis, Maryland. The second year, concentrating on meteorology itself, needed to be taught elsewhere. Reichelderfer approached climatologist and eugenics proponent Robert DeCourcy Ward (d. 1931) of Harvard about the possibility of hosting the course at his school; however Ward did not have enough instructional assistance. He did agree to host it the first year of its offering if the MIT physics and mathematics faculty would teach the course material in dynamic meteorology.[156] At the end of the first year, neither MIT nor Harvard had the faculty to carry out the Navy's proposed instructional program.

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[154] T. J. O'Brien, "Comments," 380-381.

[155] F. W. Reichelderfer, "Postgraduate Course in Aerology and Meteorology for Naval Officers," *BAMS* 9 (1928): 149.

[156] Dynamic meteorology is the branch of meteorology which deals with the solution of hydrodynamical and thermodynamical equations as related to the full range of atmospheric motion.

However, Warner had convinced the Daniel Guggenheim Fund for the Promotion of Aeronautics that support for aeronautics meant more than research on aircraft design and construction. Meteorological instruction and research leading to more accurate forecasts were essential for safe flight. The Guggenheim Fund then gave MIT $34,000 to fund the first three years of a meteorology course, and provided Carl-Gustav Rossby to lead it.[157]

The Swedish-born Rossby (1898-1957) would in time emerge as the twentieth century's most influential theoretical meteorologist. He had studied mathematics, mechanics and astronomy at the University of Stockholm before moving on to work with Vilhelm Bjerknes at the Geophysical Institute in Bergen, Norway. After a couple of years there, he studied hydrodynamics at the Geophysical Institute of the University of Leipzig. Returning to Sweden in 1921, Rossby took a position with the Swedish Meteorological and Hydrological Institute (SMHI) while he completed his *filosofie licenciat* in mathematical physics at the University of Stockholm. Awarded a fellowship from the American-Scandinavian Foundation to study in the United States, Rossby joined the headquarters staff of the Weather Bureau where he attempted, with no success, to introduce the Bergen School techniques to Bureau forecasters while working on problems of atmospheric turbulence. While he was there, however, Rossby's ideas fell upon the fertile ground that was Reichelderfer's mind. Their friendship blossomed, and this pair of

[157] Richard P. Hallion, *Legacy of Flight*, 219. Carl-Gustav Rossby's first name appears as "Carl-Gustav" in articles he wrote and as "Carl-Gustaf" in articles written about him. I have used "Carl-Gustav" throughout the text.

meteorologists would continue to work together to advance the discipline until Rossby's death in 1957. Having stirred up too many problems for the Weather Bureau hierarchy and needing another position, Rossby was invited by the Guggenheim Fund to organize the weather services for its model airway being constructed between Los Angeles and San Francisco in 1928. Once the weather services had been turned over to the Weather Bureau, Rossby was available to lead the new MIT meteorology program.[158]

Rossby established the course at MIT (within the department of aeronautical engineering) with the help of synoptic meteorologist Hurd C. Willett (1903-1992).[159] Willett had joined the Weather Bureau after graduating from Princeton in 1924, and subsequently spent time studying with the Bergen School. He was completing his Ph.D. at George Washington University when he joined the new MIT program. This new curriculum included course work in physics of the air, mathematical and dynamical meteorology, and practical work in forecasting. Reichelderfer hoped that the course would "arouse more general interest throughout

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[158] Byers, "Carl-Gustaf Arvid Rossby," 98-99. For additional memorials to Rossby see Tor Bergeron, "The Young Carl-Gustaf Rossby," (51-55), and Horace R. Byers, "Carl-Gustaf Rossby, the Organizer," (56-59), in Bert Bolin, ed., *The Atmosphere and the Sea in Motion: Scientific Contributions to the Rossby Memorial Volume* (New York: Rockefeller Institute Press, 1959); Gisela Kutzbach, "Carl-Gustaf Arvid Rossby," in Charles Coulston Gillespie, editor in chief, *Dictionary of Scientific Biography* (New York: Scribner, 1981), 11: 557-559; Bert Bolin, "Carl-Gustaf Rossby in memoriam," *Tellus* 10 (1957): 257-258; Horace Byers, "Carl-Gustaf Arvid Rossby," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1960), 34: 249-270. For an analysis of the relationship of Reichelderfer and Rossby to the development of meteorology in the United States, see Charles C. Bates, "The Formative Rossby-Reichelderfer Period in American Meteorology, 1926-1940," *Weather and Forecasting* 4 (1989): 593-603.

[159] Reichelderfer, "The Atmospheric Sciences," 209. Synoptic meteorology is that branch which coordinates observations into a picture of the day's weather and makes predictions of future weather.

the country in instruction in weather science and [lead] to fruitful research and development.[160]

The Bergen School techniques became the basis for instructing the navy officers completing their postgraduate training at MIT.[161] This new graduate program provided the Navy with a cadre of formally trained meteorologists. By 1934, 24 officers had attended the course at MIT and were still working as aerologists. However, by 1940 these numbers had dropped to eighteen. The continued lack of upward career mobility had taken its toll.[162] Once again, the Navy was entering a war without sufficient personnel to provide the required meteorological support to the operating forces.

With so few meteorologists and no research budget, the Navy aerologists, like their Weather Bureau counterparts had little opportunity to implement new ideas and techniques. Despite these difficulties, Reichelderfer circulated the first Bjerknes paper on frontal analysis techniques to his fellow navy officers in the early 1920s and started applying those techniques to the analysis of surface weather maps shortly thereafter. He actively sought everything he could find on the Bergen school and ensured its distribution within the navy. Therefore, navy officers were familiar with the Norwegian methods before they attended their graduate school courses at MIT.[163] The Norwegian methods were also taught to the navy's

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[160] Reichelderfer, "Postgraduate Course,"149.
[161] F. W. Reichelderfer, "Correspondence: Letter on the Polar Front Theory and the U.S. Navy," *BAMS* 18 (1937): 168-169.
[162] Bates and Fuller, *Weather Warriors,* 38.
[163] Reichelderfer, "Correspondence,"168-169.

aerographer's mates (enlisted men) at the Aerology Observatory at Lakehurst, New Jersey – site of the airship base. While Reichelderfer was in Lakehurst to forecast for airship operations, he had Rossby's group at MIT mail him the weather maps they drew on a daily basis. Even though the aerographer's mates were being trained in the Bergen School methods, Reichelderfer noticed that the maps drawn in Lakehurst did not match the maps drawn at MIT. It was obvious to Reichelderfer that someone would need to go to Bergen in order to learn the technique well enough to pass them on to the enlisted men. Reichelderfer convinced the Navy to send himself to Bergen for a week or two. En route he spent almost a month with the British Meteorological Office to see how they were organized and forecasted the weather. Reichelderfer ultimately stayed in Bergen for six months.[164] And Reichelderfer visited weather offices all over Europe (including in France and Germany), making detailed accounts of how their operations worked and what kind of equipment they used. These reports marked "Restricted" were sent via diplomatic pouch from the U.S. Embassy in Paris under Naval Intelligence cover sheets.[165] One of the Norwegians then came to the United States to give lectures to the navy aerologists (officers). This event led to what Reichelderfer later termed "successive invitations by universities which led to permanent residences by some of the well-known and distinguished Viking scientists."[166] Thus, the efforts of both

[164] Hassam Taba, "Dr. F. W. Reichelderfer," *The "Bulletin" Interviews* (Geneva: World Meteorological Organization: 1988): 91.
[165] 1931 Trip Reports (Francis W. Reichelderfer papers, Library of Congress, Manuscript Division, B5, F1) [Hereafter: **Reichelderfer papers**]
[166] Reichelderfer, "The Atmospheric Sciences," 210.

Rossby and Reichelderfer to promote Bergen School methods significantly influenced the eventual immigration of Scandinavian meteorologists to the United States. This influx of Scandinavian expertise would have a tremendous impact on the advancement of meteorology in America.

Newspaper articles of the mid-1930s generally described this "new" air-mass method of analysis to be of recent U.S. origin. In fact, in addition to being a Norwegian import, the Navy had been using it for a number of years and had been actively advocating its widespread adoption. The primary problem inhibiting its full implementation was a combination of infrequent weather observations coupled with inadequate spatial distribution.[167] In order to be effective, weather observations needed to be taken country-wide every six hours and data density needed to increase. This was not a small issue for the Navy because it obtained all weather data from the Weather Bureau, which was responsible for data acquisition, and its budget could barely handle its current requirements.

The Navy was also actively encouraging and carrying out the collection of upper air observations. Navy aerologists – led by Reichelderfer – were the first to use special recording instruments (meteorographs) to obtain temperature, pressure and humidity data during aircraft flights. These measurements could then be used for both local area forecasting and to supplement the Norwegian methods.[168] In addition, navy aerologists worked on developing new instrumentation and on

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[167] T. J. O'Brien, "The Navy's Part in Modern Aërological Developments," *U.S. Naval Institute Proceedings*, 61 (1935): 390.
[168] P. G. Hale, "The Navy's Part in Modern Aerological Developments in the United States," *BAMS* 16 (1935): 114-116.

methods for clearing fog from runways.[169] The navy aerological organization thought it was of the utmost importance to keep up with the latest scientific developments – which were primarily coming from overseas – so as to make a significant contribution to the advancement of meteorology in the United States and to be ready to fulfill its mission of support to operating forces both in war and peacetime. Weather forecasts for flight operations, visibility forecasts necessary for the accurate firing of shipboard guns, and wind forecasts for ballistic targeting would all be critical as the Navy prepared to enter another war.[170]

FIGHTING TO "GROUND" METEOROLOGY: THE ARMY SIGNAL CORPS

The Army Signal Corps (Meteorological Division) had a longer history than either the Weather Bureau or the Navy's aerological service. After all, weather services in the United States had been a function of the Army Signal Corps from 1870 until their transfer to the Department of Agriculture in 1891.[171] Once the Weather Bureau moved out from under Army control, it maintained a lock on the provision of all meteorological services in the United States until the advent of World War I.

Unlike earlier armed conflicts, as the Army prepared to enter the war in 1917, it recognized that weather support would be critical to its success on the

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[169] O'Brien, "Modern Aerological Developments," 385.
[170] Ibid., 385, 392.
[171] Whitnah, *History of the U.S. Weather Bureau,* 61. Whitnah provides details on the Army's control of weather services and their shift to civilian control (22-60). Also see Karl Larew, *Meteorology in the U.S. Army Signal Corps, 1870-1860* (Washington, D.C.: Signal Corps Historical Division, 1960), 1-22; Fleming, *Meteorology in America,* 157-166; Bates and Fuller, *Weather Warriors,* 9-14; Fuller, *Thor's Legions,* 3-7; Dupree, *Science in the Federal Government,* 187-192.

battlefield. Weather services had not become important because predictions were significantly better than they had been in the past, or because Army leaders had finally figured out that weather conditions impacted the outcome of battles. Weather prediction was important because advances in armaments had come to dictate the requirement for meteorological support. Artillery ranges had increased to ten miles or more. Therefore, atmospheric conditions, in particular winds, impacted targeting. Army units were also using listening posts to determine the location of enemy artillery batteries. Known as "sound ranging," this method was woefully inadequate to begin with. Its accuracy decreased dramatically without knowledge of air density and wind speed and direction. Poison gas, the most unfortunate choice of weapons during the war, depended upon favorable winds to be lethal to the enemy. If winds were too high, the gas either blew right over the trenches or dispersed too rapidly to be effective. If the winds were too light, the gas took so long to reach the target that the enemy could take countermeasures. If the wind shifted, it would drift back over the friendly forces. That particular event would "seriously interfere with the career of the gas officer."[172] Therefore, accurate knowledge of the wind regime was critical to the success of a gas attack. And, of course, the introduction of aviation assets meant forecasts for pilots. Therefore, the Air Service of the American Expeditionary Forces (AEF) was one of the first Army organizations to require weather support. These early aviation forecasts were not for tactical reasons. Their purpose was to keep these planes built from "wood, glue,

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172 Philip Maynard Flammer, "Meteorology in the United States Army, 1917-1935" (Master's thesis, George Washington University, 1958), 14.

wire, and fabric" out of adverse weather – high winds, turbulence, and hailstorms – which could bring them down.[173]

General John Joseph "Black Jack" Pershing, AEF Commander, requested meteorological personnel. Just like the Navy, the Army did not have enough meteorological officers to meet the demand when the United States entered the war. Not only did Pershing need meteorologists in Europe, the Army had requirements stateside too, which could not be met by the Weather Bureau. Stateside activities supporting aviation, the Gas Warfare Service, the Ordnance Proving Grounds, and Field and Coast Artillery units led to increased demands for in-house weather services.[174]

Manpower was a huge problem. The Chief Signal Officer, General G. O. Squier, called upon the National Research Council for recommendations on possible officer personnel. He also asked the Weather Bureau for help because "virtually all the trained meteorologists in the country were employed by the [Bureau]." A planning committee, headed by then Lieutenant Colonel Robert A. Millikan, who was serving as the Officer in Charge of the Signal Corps Science Research Division, was composed of Weather Bureau personnel including Chief Marvin. The committee determined that the available assets had to be divided among three basic support areas: the AEF, the stateside activities needing weather services, and research into meteorological problems. To solve the manpower

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[173] Ibid., 11-15.
[174] Bates and Fuller, *Weather Warriors*, 18.

problem, the Weather Bureau donated 145 of its 600 employees to the Army for the duration of the war. Hundreds more were trained just for wartime service.[175]

Given the activities needing support, at its essence, the Signal Corps meteorology problem was really one part science and one part military tactics. Meteorology personnel faced challenges that fell into one of three groups. First, they needed to develop statistical meteorology, i.e., climatology, in order to determine the appropriate placement of military units and aerodromes. Second, meteorology personnel needed to provide current meteorological information for use by aviation units, artillery and sound ranging units (ballistic winds, pilot balloon and theodolite observations). Third, they needed to provide forecasts in advance of military operations.[176] Observers needed to be able to measure temperature, air density, and wind direction and speed so that artillery units could determine how to aim their guns. These same data allowed for sound ranging – the use of sound to determine target location.[177]

Even though it was cobbled together at the last minute, the meteorological division had performed well during the period of conflict. Alas, just like their Navy brethren, meteorological personnel left the Army in droves and returned to their peacetime occupations. However, the mission remained. Planes were still flying. The Chemical Warfare Service was conducting experiments and practice

[175] Flammer, "Meteorology in the United States Army,"18.
[176] William Gardner Reed, "Papers Presented at the Toronto Meeting: Military Meteorology," *BAMS* 3 (1922): 57-58.
[177] D. Wobbe, "Meteorology in its Application to Artillery Fire," *BAMS* 13 (1932): 137-138.

maneuvers. The field artillery units still needed standard ballistic range tables for their varied artillery pieces.[178]

Despite the hundreds of men trained in meteorology in World War I, between 1921 and 1935 no more than eleven weather officers served in the Signal Corps. Along with a handful of enlisted personnel, they were able to fulfill less than one-fifth of the demand for their services. The Signal Corps continued to build more weather stations – they grew from 11 to 41 – but with so few people to man them, the quality of meteorological services was, in consequence, poor.

The Signal Corps trained both officers and enlisted weather personnel at Camp Vail (later Fort Monmouth), New Jersey, which also was home to meteorological instrument development efforts. Additional enlisted men received meteorology training at Carlstrom Field (Florida) and March Field (California) as part of flight training.[179] With close ties to the Weather Bureau – indeed, the Weather Bureau was providing most of the forecasts for it – the Signal Corps took no interest in the Bergen School techniques (which would eventually replace the extrapolation method of moving pressure centers from west to east over time). When Reichelderfer offered Signal Corps leaders the chance to participate in the Navy's new postgraduate program at MIT, they declined.[180]

However, the Air Service was not content waiting for the Signal Corps to upgrade their weather support. From 1922 to 1924, the Meteorology Section's

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178 Flammer, "Meteorology in the United States Army," 33-34.
179 "From the Committees," *BAMS* 1 (1920): 15-25.
180 Fuller, *Thor's Legions,* 18-19.

budget more than doubled from $27,000 to $67,000 – and it was all due to the requirements of the Air Service. As far as the Air Service was concerned, meteorological services should have been under its jurisdiction. The Signal Corps argued that weather services were not exclusive to the Air Corps. Therefore, the Meteorology Section stayed within the Signal Corps, but fell under the Intelligence Division, having escaped from the Special Services Division which supervised the Pigeon, Photo and Commercial Sections.[181]

From the mid-1920s on, a power struggle ensued between the older Army "ground pounders" and the younger aviators. The latter wanted more support. The former had control and intended to retain control. The Signal Corps was really not that enamored of the Meteorology Section and probably could have been forced to give it up. However, during this period, the Air Service was fighting for survival within the military structure. It did not have the time or the energy to become embroiled over what seemed like a side issue.[182]

Although the Air Service was not effectively fighting for control over meteorology, it did start sending its own people for meteorology training. So while the Signal Corps declined the opportunity to train its officers in MIT's new program, the Air Service accepted Reichelderfer's offer. It sent its first student to

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[181] Larew, *Meteorology in the U.S. Army Signal Corp,* 39-41.

[182] Larew, *Meteorology in the U.S. Army Signal Corps,* 42; Flammer, "Meteorology in the United States Army," 48.

MIT in the fall of 1929. However, the shift to the Bergen School methods did not take place until 1935.[183]

By 1934, the Chief Signal Officer declared that he wanted to be out of the weather business if he did not get more funding. That same year, the government discovered that air mail contracts had been awarded fraudulently. Therefore, they were all cancelled and the Army Air Service took over the flights. Unfortunately, the weather was bad and the forecasts were inadequate. The result: ten pilots and their aircraft were lost in three weeks in March 1934. The Signal Corps received the brunt of the criticism, and tried to use it to the Corps' advantage in its quest for additional funds. This ploy did not work. In the end, the Air Corps, which by this time had more weather-trained officers than the Signal Corps, took over weather forecasting responsibilities since it was the primary user of weather services.[184] Commenting on the separation of meteorology from the Signal Corps, one officer later testified: "[Meteorology] has no more to do with signals than Donald Duck."[185]

The Air Corps took over sponsorship of weather services for the aviation sector and for any ground forces at the division level or higher in 1937. Signal Corps officers desiring to transfer to this new part of the Air Corps had to qualify as pilots – a requirement that did not facilitate the transfer of trained personnel. However, sufficient personnel were attracted to this new meteorological service

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[183] Fuller, *Thor's Legions*, 18-19.
[184] Flammer, "Meteorology in the United States Army," 48.
[185] Testimony of Colonel Clark, quoted in memorandum to Edward L. Bowles, 17 August 1943 (Bowles papers, B30, F3).

that by the end of 1939 there were 30 officers and 388 enlisted personnel in the Air Corps weather branch.[186] Even this increase, however, would not be nearly enough to provide for the requirements of the approaching war.

While the actual forecasting mission moved to the Air Service, the limited meteorological research function within the Signal Corps remained. Most R&D activities were centered around the development and refinement of meteorological instrumentation. Despite pressure to move instrumentation work from Camp Vail (later Ft. Monmouth) to Wright Field (Ohio) because of aviation requirements, it remained at Camp Vail since the Army required meteorological support for all of its forces – not just the aviators.

The work at Ft. Monmouth would be critical for the eventual development of numerical weather prediction models. The researchers worked on the development of an "audiomodulated radiosonde" (then called a radiometeorograph). This instrument would allow upper air observers to gather data during the night or during cloudy weather – whenever the pilot balloon would normally be obscured. The signal from the equipment carried by the balloon could be picked up by a radio receiver and then transmitted to interested users.[187] The Army also conducted meteorological research related to chemical warfare. The Chemical Warfare Service, an outgrowth of the use of gas warfare during World War I, sponsored almost 700 projects for the Army, Navy and civilian

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[186] Bates and Fuller, *Weather Warriors*, 48.
[187] Dulany Terrett, *United States Army in World War II: The Technical Services: The Signal Corps: The Emergency (To December 1941)* (Washington, D.C.: Department of the Army, 1956), 28, 32.

organizations. However, appropriations were so small (less than one million dollars annually for *all* projects combined) from 1923 to 1926 and less than two million dollars annually from 1927 to 1938, that on average each project only received several thousand dollars. Efforts were directed towards the effects of micrometeorological phenomena on the movement of gas.[188]

WEATHER SERVICES IN THE INTERWAR PERIOD

While European weather services, were, relatively speaking, awash with money, encouraging of research, and eagerly trying the new ideas of the Bergen School during the interwar period, in the United States this was a period of retrenchment for all of the weather services. While the Norwegians and the Germans funded geophysical institutes to conduct research into meteorological problems, the Americans did not. The military services experienced dramatic drops in personnel and funding immediately after the close of World War I, from which they did not start to recover until war loomed once again. The Weather Bureau, which had been forced to operate without almost a quarter of its personnel during World War I, got them back, only to face stagnant appropriations in the 1920s, followed by drastically reduced appropriations as the Great Depression deepened.

Losing its most experienced personnel, and having no opportunity to replace them, the Bureau could barely provide the routine services demanded of it,

[188] Leo P. Brophy, Wyndham D. Miles, and Rexmond C. Cochrane, *United States Army in World War II: The Technical Services: The Chemical Warfare Service: From Laboratory to Field* (Washington, D.C.: Department of the Army, 1959), 32.

much less expand its aviation services. With low levels of funding, low levels of compensation, and a training program that assumed that the only way to create a forecaster was through a five to six year long apprenticeship, it should not be surprising that the Bureau was not in the forefront of implementing new forecasting techniques. Furthermore, the Bureau was being told by physicists, astrophysicists, statisticians, economists, and anyone else with an idea for a better forecasting technique, how it should be doing its job. What could have been more demoralizing for government meteorologists than being told, through word and deed, that their scientific discipline really did not deserve to be included with the scientific "big boys"?

Navy Aerology, on the other hand, suffered more from benign neglect. Seagoing captains thought they knew everything there was to know about the weather and were perfectly satisfied with their ability to operate under any conditions. That they, and commanders of entire fleets, failed to consider the impact of weather on naval operations was a long-standing problem. However, operating in all types of weather was just part of fulfilling the mission at sea. This same line of thought did not last as long in the aviation community. The atmosphere is much less forgiving to aircraft under less than perfect conditions. Therefore, aviators demanded increased meteorological support even while the Navy did not provide a career path for those who would provide it.

The interwar period saw the Navy adopt and spread the Bergen School methods through its own professional networks in a way that did not occur in the much larger Weather Bureau which was top-heavy with older men. Thus when the Science Advisory Board directed the adoption of the air-mass analysis techniques, the Navy was able to meet the requirements with less resistance among its personnel. Instruments were being developed and put on aircraft to gather needed data, and subsequently shared with the Weather Bureau. In this way, the Navy was forward-looking. What it could not see was how once again it was approaching a time of war with insufficient personnel to fulfill its mission.

The Meteorology Section of the Army Signal Corps was probably in the worst shape of all the weather services. It was definitely a low-priority organization – lumped as it was with the messenger pigeons. Aviation units received their forecasts from the Weather Bureau. With no war in sight through the 1920s and into the 1930s, there was little concern about providing meteorological services to ground troops overseas. Research was almost exclusively focused on developing and improving meteorological instrumentation. And although those newly developed instruments, in particular the prototype radiosonde, would greatly enhance meteorologists' ability to collect upper-air data, the success of that endeavor was not enough to keep the Meteorological Section going. Scant attention was paid to new developments in the atmospheric sciences and the old methods – good enough for the Weather Bureau – were good enough for the Signal Service. It was not until the end of this period that the Air Corps prevailed and the meteorological mission was moved out of the Signal Corps. With that move, the focus would shift to keeping aviation assets – pilots and aircraft – safe and effective.

Meteorological services did not advance much in the United States between the wars. Instrumentation improved primarily through the efforts of the Signal Corps' research arm and because of the interest of instrument specialist Charles Marvin, then Chief of the Weather Bureau. High profile criticism of the Weather Bureau prompted Agriculture Secretary Wallace to make the politically expedient move and call in outside "experts" in the guise of the Science Advisory Board to recommend ways to "fix" the Bureau. However, many of the Bureau's problems could be directly attributable not to a failure of leadership, but to a failure of adequate funding for an organization providing a free service that earned business and agriculture interests millions of dollars a year. Not even the distinguished members of the Science Advisory Board could secure the funding the Bureau needed – it could just recommend changes that the Bureau could not afford to make. And so, the nation's weather services limped along – doing their best to provide safety of flight, warn farmers and the general public of weather hazards, and get out a forecast that made sense. The Weather Bureau, Navy Aerology, and the Air Corps' new weather section would soon be put to a huge test – a test for which none of them were ready.

CHAPTER 3
TOWARDS A MORE DYNAMIC ATMOSPHERE: DISCIPLINE DEVELOPMENT IN THE INTERWAR PERIOD (1919-1938)

The research university structure had emerged in the United States in the late nineteenth century. The physical and life sciences, and later the earth sciences, continued to develop as major disciplinary communities into the early twentieth century. A National Research Council compilation of doctoral degree data from 1923 illustrates the significant differences in disciplinary strength. From 1919 to 1923, there were a total of 621 doctoral degrees awarded in chemistry, 201 in botany, 185 in physics, 93 in the geological sciences, and 20 in astronomy. In stark contrast, there were only *two* Ph.D.s awarded in meteorology.[189] This paltry number of doctoral students in a discipline that had been a serious scientific undertaking since the time of Aristotle begs for an explanation.

Writing in 1918, Harvard climatologist Robert DeCourcy Ward opined that meteorology was not more widely-studied because having spent a lifetime becoming familiar with meteorological phenomena makes each man think of himself as being a "born meteorologist." And that very familiarity "[here] as elsewhere, breeds a certain degree of contempt." Because weather is a topic of everyday conversation, serious study did not seem to be worthwhile. He quoted a "highly educated" woman of his acquaintance who told him one day, "You have a

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[189] Unpublished table of the National Research Council, Table III, Doctorates Conferred According to Subjects (1923) (National Academy of Sciences Archives, Research Information Service, 1920-1923, Information Files Doctorates Conferred).

very difficult subject to teach. People, generally, do not care to hear about things which they think that they already know."[190] Everyone, in essence, *was* a meteorologist.

In addition to the general lack of interest due to "knowing it all" already, there were so few available meteorologists that finding meteorological instructors was difficult. Ward hoped that faculty attached to physics, geography, geology, or other more marginally related science departments would take it upon themselves to learn enough meteorology to prepare and teach an elementary course. With their interest sufficiently piqued, students might demand more advanced courses, thus opening the door for the establishment of meteorology curricula in colleges across the country.[191] Ward's dream would eventually come true, but it would prove to be a very slow process.

The massive meteorological training effort, which produced both weather forecasters and observers for the military during the First World War, spurred renewed interest in meteorological education for the civilian community. However, meteorology was not yet a university discipline. Indeed, it was hardly a scientific discipline at all. In contrast, by this time physics, biology, chemistry, astronomy, and geology already had robust university departments with well-organized

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[190] Robert DeC. Ward, "How Meteorological Instruction May Be Furthered," *MWR* 46 (1018): 554.
[191] Ibid. For contemporary assessments of the state of academic meteorology in the early twentieth century, see Frank Waldo, "The Study of Meteorology," *Education* 26 (1906): 149-153; Cleveland Abbe, "The Progress of Science as Illustrated by the Development of Meteorology," *Annual Report* (Washington, D.C.: Smithsonian Institution, 1907), 287-309; "Curiosities of Science and Invention: Meteorology in American Universities," 343; and William G. Reed, "Meteorological Observations at the University of California," *Science* XXXVII (23 May 1913): 800-802.

graduate programs.[192] Meteorology was seldom taught at all, by anyone. In order for meteorology to become a respectable, bona fide scientific discipline recognized by other scientific communities, meteorologists needed to create the two essential elements of any professional discipline: dedicated academic programs in major research universities and a professional society. As World War I ended, neither existed. But the impetus provided by the war would lead to the establishment of both academic meteorology and a professional society. By the end of the 1930s, the situation would radically change, as MIT, Caltech and NYU established meteorology programs and the American Meteorological Society aggressively sought to put meteorology into the scientific mainstream. The professionalization of meteorology had begun.

ORGANIZING ACADEMICS: FROM MILITARY NEEDS TO CIVILIAN WANTS

As World War I came to a close, there were three basic approaches to meteorological instruction in the United States: (1) the climatological approach espoused by Ward at Harvard; (2) the physical approach of the Weather Bureau's atmospheric physicist W. J. Humphreys; and (3) the combined climo-physical approach of Charles F. Brooks of the Blue Hill Meteorological Observatory. The

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[192] While a disciplinary history of meteorology is lacking, historians of science have explored the disciplinary histories of the physical sciences. For a discussion of academic programs in astronomy and their comparison to chemistry and biology fields, see John Lankford, *American Astronomy: Community, Careers, and Power, 1859-1940* (Chicago: University of Chicago Press, 1997), 371-382. For a discussion on academic programs in physics, see Kevles, *The Physicists,* 61-63, 70-72, 77.

latter had been the approach of choice at the Army Signal Service meteorology school, which trained more people in meteorology in a shorter period of time than any other organization in the United States.[193]

Planning for the Signal Corps school started in Fall 1917. The Army had needed to train 1000 men in meteorology and, for obvious reasons, needed them to be trained quickly. As the United States entered the war, the only trained weather observers were those working for the Weather Bureau. They had already been inducted into the military, and more were needed.[194]

The Weather Bureau trained these new recruits within its own offices until finally being overwhelmed by the sheer number of soldiers. By Spring 1918, the Signal Corps established a special school at Texas A&M (College Station, Texas), which included instruction on the physical properties of the atmosphere, weather forecasting, the different uses of meteorology (aeronautics, agriculture, commerce), and the physiological effects of weather and weather changes.[195] This stands in stark contrast to the *mobilization* of chemists and physicists during the same conflict. Those disciplines possessed ample numbers of scientists who just needed to be brought in under the military umbrella – usually as reserve officers.[196] Meteorologists, both professionals and sub-professionals, had to be trained.

Future Nobel Prize laureate and Caltech President physicist Robert Andrews Millikan (1868-1953) – organizer of the entire Army meteorology

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[193] Charles F. Brooks, "Collegiate Instruction in Meteorology," *MWR* 46 (1918): 555.
[194] Oliver L. Fassig, "A Signal Corps School of Meteorology," *MWR* 46 (1918): 560-561.
[195] Brooks, "Collegiate Instruction," 555.
[196] Kevles, *The Physicists,* 118-119,132-133, 138.

training program – required that all weather observer school recruits have college degrees, preferably in mathematics, science, or engineering. (This was a startling requirement, considering that by the start of the 21st century – despite all the highly technical equipment involved – observers generally possessed only high school diplomas.) The engineers, who comprised over half (175 of 300) of the original trainees, were the most interested in aerological work. Their education and training enabled them to suggest new designs for meteorological instruments and to develop faster methods for reducing observations for the computation of ballistic wind values needed by artillery units.[197]

Academic and Weather Bureau meteorologists knew that military training was not equivalent to the kinds of college courses which would be necessary to create a cadre of professional meteorologists in the United States. As prominent British meteorologist Sir Napier Shaw argued, professional training was an absolute requirement for the advancement of the science. "Observations, map making, and forecasting don't a science make," Shaw argued: empirical knowledge was important, but would not by itself lead to knowledge of the physical processes which take place in the atmosphere.[198] To figure out how those physical processes

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[197] Fassig, "A Signal Corps School of Meteorology," 561. For more information on Robert A. Millikan, see Daniel F. Kevles, "Robert Andrews Millikan," in Charles Coulston Gillespie, editor in chief, *Dictionary of Scientific Biography* (New York: Scribner, 1981), 9: 395-400; Robert H. Kargon, "The Conservative Mode: Robert A. Millikan and the Twentieth Century Revolution in Physics," *Isis* 68 (1977): 509-526. See also Millikan's autobiography, Robert A. Millikan, *The Autobiography of Robert A. Millikan* (New York: Prentice-Hall, 1950).

[198] Sir Napier Shaw, "The Outlook of Meteorological Science," *MWR* 48 (1920): 34-37.

worked would take considerably more meteorologists trained from a position of strength in physics and mathematics.

What the academic and applied meteorologists did not know was the extent of existing meteorological instruction being offered in American colleges and universities: course lengths, material covered, the types of students they reached, and who was offering them. During this period, at least three different researchers and organizations initiated studies of meteorological instruction. In 1919, the nation's largest employer of meteorologists – the Weather Bureau – asked the U.S. Bureau of Education to survey higher education institutions throughout the country about their meteorological offerings. The Education Bureau sent out 633 questionnaires, of which 433 (68%) were returned. The Education Bureau assumed that the unreturned questionnaires had gone to colleges with no offerings. Of those reporting, 84% did not offer a separate meteorology course. Of these colleges, 22% offered meteorology as part of a more general course, while another 13% intended to offer it as a separate course. Ten colleges listed Weather Bureau employees as their instructors. Of the 70 colleges that did offer meteorology courses, 20 did so in geology, ten in physics, and one each in chemistry, biology, and astronomy. The remaining 53% did not specify which departments offered their meteorology course.[199] Clearly meteorology garnered limited attention in academia. As Caltech

[199] Charles F. Brooks, "General Extent of Collegiate Instruction in Meteorology and Climatology in the United States," *MWR* 47 (1919): 169-170.

physicist Theodore von Kármán put it years later: "[Very] few academicians accepted meteorology because it was regarded as a guessing science."[200]

In 1920, leaders of the newly-formed American Meteorological Society (discussed in the next section) were concerned about this dearth of meteorological instruction. They formed a Committee on Meteorological Instruction to address the problem. Committee members concentrated their efforts on three areas: (1) collecting and publishing teaching techniques in the Society's *Bulletin* (*BAMS*) which would improve meteorological instruction; (2) reviewing books which could be used by meteorology/climatology instructors and identifying other instructional sources; and (3) promoting the establishment of meteorology courses in colleges and universities where they were not then offered.[201] Due to its importance to agriculture, at least one AMS member argued that meteorology certainly belonged at land grant institutions.[202] Cornell and Utah Agricultural College – both land grant schools – were already offering meteorology courses during these early years.

An exacerbating issue were poor career prospects in meteorology. Weather Bureau salaries were notoriously low. Likewise, active duty meteorologists in the

[200] Quoted in William A. Koelsch, "From Geo- to Physical Science: Meteorology and the American University, 1919-1945," in James Rodger Fleming, ed., *Historical Essays on Meteorology 1919-1995: The Diamond Anniversary History Volume of the American Meteorological Society* (Boston: American Meteorological Society, 1996), 522. Koelsch's article addresses the ways in which meteorology as a scientific discipline was handled by institutions of higher education in the United States during the 1920s and 1930s until it became recognized as a university discipline in its own right during the early 1940s.
[201] C. F. Brooks, "From the Committees: Meteorological Instruction," *BAMS* 1 (1920): 15-16. The committee membership sheds light on the issue of meteorological offerings by identifying affiliations: W. M. Wilson, Department of Meteorology, Cornell University *and* Section Director, Weather Bureau Office, Ithaca, NY; W. I. Milham, Department of Astronomy, Williams College; H. E. Simpson, Department of Geology, U. of North Dakota; F. L. West, Utah Agricultural College.
[202] J. Warren Smith, "From the Committees: Agricultural Meteorology," *BAMS* 1 (1920): 19-20.

Signal Corps also received low wages. Before WW I there had been very little demand for meteorologists. During the war, aviation and gas warfare operations generated a demand that far outstripped the supply, thus necessitating the hastily assembled training programs. With the end of the war, expanding aeronautics interests in the civilian sector kept the demand higher than the supply. Unfortunately, the years of poor career opportunities compounded the problem by leaving the profession without sufficient qualified people to teach the next generation. There were more calls for meteorology instructors than people to fill the jobs.[203]

Even those courses that *were* offered sometimes provided minimal instruction. One example is a course in "Applied Meteorology" (meeting for less than one hour per week) taught at the "Southern Branch of the University of California" (the forerunner of UCLA). In Spring 1920, it gave students an overview of weather studies, including climatology, and how such information could be applied to commerce, agriculture and horticulture. Students needed to have one year of physics, good algebra skills, and knowledge of general physiography. Their grades were based on two 300 word papers on such topics as "Advantages of Meteorology Study," and "High Pressure and Low Pressure Areas Compared." Given the requirements for the course, it is unclear why physics and algebra were high on the list of prerequisites. While their Scandinavian counterparts were doing air-mass analysis, the Southern Californians were concerned with "Factors in a

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[203] C. F. Brooks, "Meteorological Instruction," *BAMS* 1 (1920): 56-57.

Healthful Climate."[204] Of the 84 students (mostly seniors), a few were engineers, some were in political science, and many were teachers taking the course as a supplement to their geography studies. A "detail" from the nearby Army Air Service balloon school also attended the course.[205] None of these students was likely to go further in meteorology, and it is questionable how many of the pre-service teachers received enough information to create an exciting approach to studying the weather in their classrooms.

Before recommendations could be made on the improvement of meteorological instruction, the AMS Committee thought it would be important to determine the status of such instruction at all levels of education – primary, secondary and post-secondary. Reporting their findings at the first AMS annual meeting in 1920, the committee members noted that while meteorology was of great economic importance to the country and was an equally important part of a liberal education, it was not accorded an important position in the educational system. That such instruction was needed was widely recognized: primary schools had nature studies, for example, and high schools taught physiography, so there were already niches which could absorb meteorology. Unfortunately, the lack of meteorology courses at teacher training institutions translated into ill-prepared primary and secondary teachers. The committee concluded that once higher education institutions offered meteorology, it often became a very popular course for liberal arts and education students. This report did not address the lack of a

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204 "Notes of Interest to Teachers," *BAMS* 1 (1920): 89-91.
205 "Meteorology at the Southern Branch of the University of California," *BAMS* 2 (1921): 37-38.

more advanced meteorology curriculum being offered in at least one college in the country. The committee encouraged AMS to get involved in establishing such a program of study for the purpose of advancing the pool of qualified professional meteorologists.[206]

By the next year, more colleges were announcing the addition of meteorology to their curricula. The most ambitious program, and the one that produced the most graduate students during the 1920s, was Clark University's newly established geography school, which also offered meteorology and climatology courses.[207] But movement was still too slow for the AMS Committee on Meteorological Instruction. In 1922, the members proposed creating and sending "propaganda…often and with emphasis" to the head of every American college and university, extolling the benefits of meteorology and climatology courses. The departmental targets of choice: physics, geology, geography and astronomy.[208]

Despite the lamentations, meteorology and climatology instruction at some of America's more distinguished schools was on the upswing in the 1920s. Harvard students were allowed to "concentrate" in meteorology and climatology (a curriculum which entailed six courses including mathematics and physics) for undergraduates and a research course in climatology and aerology for graduate

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[206] "Proceedings of the First Annual Meeting: Report of Committee on Meteorological Instruction," *BAMS* 2 (1921): 7-8.
[207] A. H. Palmer, "Miscellaneous Notes," *BAMS* 2 (1921): 71.
[208] "The Second Year of the Society," *BAMS* 3 (1922): 1-13.

students. Cornell reported an overflow of applicants for its meteorology course, which was limited to 100 students.[209]

A survey of normal school and teacher's college catalogs in 1928 (76 schools in 36 states) – showed 35 courses being offered in what might be termed basic meteorology or climatology and its effect on man, 150 courses focused on specific continents or countries which contained climatology information, and another 50 geography courses which included meteorology and climatology sections. However, despite the overall increase in meteorology courses in teacher training institutions, meteorology had not yet appeared in the all-important methods courses, wherein students prepared how to teach different subjects depending on the age level of the student.[210]

The most significant new courses – created at MIT at the behest of the Navy and financially supported by the Guggenheim Fund – started in the fall of 1928. Almost ten years after the AMS started lobbying for advanced meteorological instruction in the United States, it appeared that meteorological instruction was about to take hold. Aviation interests – military and civilian – were primarily responsible. As the decade closed, meteorologists still saw the need for "truly serious training" in meteorology, grounded in physics and mathematics, as an "urgent" requirement. They hoped that the need would be met by the "most

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[209] "University News," *BAMS* 3 (1922): 157.
[210] J. M .Shipman, "Meteorology and Climatology in Normal Schools and Colleges," *BAMS* 9 (1928): 86.

progressive institutions."[211] However, the dearth of academic meteorologists meant that colleges had to rely on U.S. Weather Bureau and Canadian Meteorological Office meteorologists to fill instructor positions.[212]

Despite the catastrophic economic upheaval of the Great Depression, the AMS continued to pursue its goal of expanded educational opportunities in meteorology. Updated information about the state of meteorological education in the United States came from an unlikely quarter in 1934: the University of Southern California's School of Education. Graduate student Woodrow C. Jacobs, writing his master's thesis on meteorology instruction in U.S. higher education, analyzed the breadth and depth of available meteorology courses. Receiving 733 college catalogs in response to his query letters, Jacobs followed up by sending a questionnaire to each school offering at least one meteorology course.[213] He found that meteorology courses (those which were mostly meteorology, not just a subset of a course) had increased slowly and steadily until 1924. The burgeoning popular interest in aviation had precipitated a dramatic increase in courses starting in 1925. The greatest expansion had occurred at teacher's colleges and technical institutions. Indeed, over half of all such courses were added after 1924. A total of 162 (22%) of the 733 colleges offered meteorology courses, ranging from a high of twelve courses at Clark University, to just one course at 111 others.[214] Although often

[211] W. J. Humphreys, "The Claims of Meteorology for a Place in College," *BAMS* 10 (1929): 164-166.
[212] "Notes," *BAMS* 14 (1933): 269.
[213] Woodrow C. Jacobs, "A Survey of Instruction," 32.
[214] Ibid., 39. Rossby's program at MIT offered eleven courses.

offered at state colleges and universities, and teacher training institutions, meteorology courses were rarely offered at private, technical/engineering, or women's colleges. Most were just one semester long. Found in twenty different academic departments, meteorology courses were more likely to be found in geography (46%) than in physics (8%), even though meteorology was based on physical laws. Only four colleges had meteorology departments. Most geography offerings were climatologically-based or of the "weather and man" variety of meteorology. Jacobs concluded: "[The] study of meteorology as a pure or applied science seems to have been relegated to the background in most cases, a situation which is not generally true of science study in the colleges and universities of this country."[215] While most instructors were well qualified in their own discipline, they had little, if any, training in meteorology. So while meteorology offerings were quite extensive, the quality of undergraduate courses, and graduate and research work were poor.[216]

Courses offered at teacher training institutions were deliberately non-technical and required scant physics background. Most included discussions of the physical and chemical characteristics of air, climatological information, and instruction in the taking of measurements of temperature, pressure and humidity. Suitable textbooks were lacking. Faculty needed texts which provided a survey of meteorology that did not depend on physics knowledge.[217]

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[215] Ibid., 50.
[216] Ibid., 95.
[217] G. E. Harding, "Meteorology and Climatology at a Teachers College," *BAMS* 16 (1935): 40-42.

As this interwar period came to a close, only one other institution besides MIT established a meteorology department: New York University. Meteorology courses had been offered within its geology department for a number of years, but the separate department led by Rossby-trained South African Athelstan Spilhaus (1911-1998), opened in 1937.[218] Its curriculum included a general undergraduate program designed to meet the needs of airlines, the Weather Bureau and "other potential employers." The graduate program, however, was within the College of Engineering.[219] Students took upper air and surface observations – which were sent in to the Weather Bureau – from the university's meteorological laboratory.[220]

Meteorology instruction expanded dramatically in the interwar period. However, it was dominated by a non-technical approach in the geography departments of teacher training institutions. Two decades after the First World War had underscored the importance of meteorology to the nation's security, advanced instructional opportunities remained severely limited, particularly when compared with other scientific disciplines. The hoped for theoretical physics- and mathematics-based instruction that would lead to graduate study and research was

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[218] Spilhaus, well known for his invention of the bathythermograph for measuring the temperature of water with depth, left NYU in 1948 to become the Dean of the Institute of Technology at the University of Minnesota. Spilhaus granted oral history interviews with Frederick Peterson Jessup (1980) deposited with the Columbia University Oral History Research Office; with Robert A. Calvert and Monty Morèe (1976) deposited with the Niels Bohr Library of the American Institute of Physics (AIP); and with Ronald E. Doel (1989) also at AIP's Niels Bohr Library. For an obituary written by the Woods Hole Oceanographic Institution Press Office, see http://www.whoi.edu/media/news_spilhaus.a.obit.html.

[219] Robert G. Stone, "New Department of Meteorology at New York University," *BAMS* 19 (1938): 456.

[220] J. Edmund Woodman, "The New York University Institute of Aeronautical Meteorology – Its Structure and Problems," *BAMS* 17 (1936): 118-119.

available at only two institutions: MIT and NYU. And those meteorology programs did not have the resources to attempt major research projects. Most of the research that was conducted had limited value to forecasting applications.

The small numbers of academic meteorologists active during the interwar period was spending a great deal of time just getting their instructional programs off the ground. However, graduate thesis topics provide a glimpse of the emerging research agenda. Doctoral programs in meteorology were non-existent in the immediate post-war years, so graduate students obtained their degrees through physics and geography departments. The former were likely to lead to topics of a dynamical or physical nature, while the latter tended towards climatological topics.

While two doctoral degrees were awarded in 1922, there would be a seven year gap before another four were awarded in 1929. The first two degrees – one from Cornell and the other from George Washington University (GWU) – addressed climate and dynamics topics respectively. The latter, on free-air pressure maps, was written by aviation pioneer C. LeRoy Meisinger.[221] Unfortunately, his promising meteorology career was cut short when his balloon was struck by lightning while he was taking upper air measurements. Of the four degrees awarded at the end of the decade, two were climatology-based theses by geographers, but the other two (once again from GWU) were more meteorological. One, on the geometric theory of halos, went to Edgar W. Woolard (who opted to be a mathematics professor). The other went to Weather Bureau employee Hurd C.

[221] C. LeRoy Meisinger, "American Doctors of Meteorology and Climatology," *BAMS* 4 (1923): 78.

Willett (1903-1992) for his studies of fog and haze. Upon receiving his degree, Willett left the Bureau and joined Rossby on the MIT faculty where he taught synoptic meteorology and advanced the science of long-range weather forecasting.[222]

Since the home-grown academic meteorologists were few and far between, meteorological research was understandably limited. A gradual influx of European meteorologists – which increased dramatically as Hitler expanded his reach throughout Europe and visiting scholars were trapped in the United States – increased the numbers of researchers. Europeans were coming from universities and institutes where meteorology had the same standing as astronomy; i.e., it was considered to a "real" science. Cutting-edge meteorological developments were emerging from the Bergen School and some of its trainees and practitioners were spending time in the United States. Bernhard Haurwitz (1905-1986), of the University of Leipzig's Geophysical Institute, served as a Research Fellow at MIT and Harvard's Blue Hill Meteorological Observatory during the 1932-1933 academic year.[223] The Swedish-born and trained Rossby, at MIT, cycled back to

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[222] "Four New Doctors of Philosophy in Meteorology or Climatology," 166-167. For a short obituary on Willett from the MIT News Office, see http://web.mit.edu/newsoffice/tt/1992/apr01/25946.html.

[223] Haurwitz went on to become a prominent dynamic meteorologists in the United States. See Julius London, "Bernhard Haurwitz," *Biographical Memoirs* (Washington, D.C.: National Academy of Science, 1996), 69: 86-113; Bernhard Haurwitz, "Meteorology in the 20th Century – A Participant's View," *BAMS* 66 (1985): 281-291, 424-431, 498-504, 628-633; and George W. Platzman, *Conversations with Bernhard Haurwitz* (Boulder, CO: National Center for Atmospheric Research, 1985) (NCAR/TN-257).

Scandinavia periodically to keep up with the new approaches and to advance his own research in dynamical meteorology.[224]

As the 1930s unfolded and meteorological programs took root at NYU and Caltech, the research agendas at these institutions, in addition to those of MIT and Blue Hill, took on distinctive attributes. New York University concentrated its research program on investigating the upper air and visibility problems, i.e., those topics of the most immediate importance to aeronautics, as they pertained to local New York conditions.[225] MIT took a more geophysical approach emphasizing the development and application of the Bergen School's polar front theories along with empirical work on the movement of air masses.[226] All of its more theoretical work depended on mathematical or quantitative approaches, but this did not preclude synoptic work which emphasized weather map analysis as an adjunct to explaining atmospheric phenomena as well as forecasting future weather events.[227] MIT personnel also conducted research on the improvement of fog forecasting as the precursor to either preventing its development or dispersing it once formed at airports and landing strips.[228] Towards the end of this period, financial support from the Bankhead-Jones Fund (under the supervision of the Department of Agriculture) enabled the beginnings of research into long-range weather forecasting

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224 "Personal Notes," *BAMS* 13 (1932): 238-239.
225 Stone, "New Department of Meteorology," 456.
226 Koelsch, "From Geo- to Physical Science," 522.
227 Herbert H. Kimball, "A Review of Recent Advances in Meteorological Research," *BAMS* 14 (1933):187.
228 "Fog – A Method of Prevention?" *BAMS* 11 (1930): 157-158.

and its implications for agriculture.[229] Caltech's program, under the direction of the charming, piano-playing, and later very controversial, Irving Krick (1906-1996) in the mid-1930s, concentrated on applied meteorology at the expense of a more theoretical approach and was primarily a training program for meteorologists intending to work for the airline industry and other applied activities. Krick, who had entered meteorology at the behest of his brother-in-law, Horace Byers, received his Ph.D. at Caltech after struggling through the mathematical theory of the degree program. However, he ingratiated himself to the aviation and film industries in Southern California, and built up a substantial consulting business for long-range forecasting (based on "weather typing") and weather modification while still on the Caltech staff.[230] Finally, the Blue Hill Meteorological Observatory, connected with, but not funded by, Harvard University, conducted a variety of research projects during this period. Blue Hill researchers worked on instrumentation (particularly radiosonde development and deployment) for upper air data collection, atmospheric phenomena of importance to aviators (thunderstorms, lightning, icing, fog), dust

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229 Louis H. Bean, "Weather and Crop Research under Bankhead-Jones Fund: Progress Report," *BAMS* 17 (1936): 288-292. [Excerpts from the full report prepared by C. F. Sarle and L. H. Bean, U.S. Department of Agriculture and published by the Bureau of Agricultural Economics, U.S. Department of Agriculture, 14 May 1936 as mimeographed special No. 143.] For more information on the Bankhead-Jones Act of 1935, see Dupree, *Science in the Federal Government*, 364-365.
230 Koelsch, "From Geo- to Physical Science," 526. For more details on the meteorology program at Caltech, see J. M. Lewis, "Cal Tech's Program in Meteorology: 1933-1948," *BAMS* 75 (1994): 69-81. Judith R. Goodstein, *Millikan's School: A History of the California Institute of Technology* (New York: W. W. Norton, 1991) provides insight into Millikan's influence on Caltech. Krick, yet another fascinating character in the meteorology community, devoted his career as a private consulting meteorologist to weather modification and very long-range forecasting techniques that were greeted with extreme skepticism by academics and the Weather Bureau. He also resigned from the American Meteorology Society before being forced out for ethics violations. For a popular "little guy vs. the big guys" version of Krick's story, see Victor Boesen, *Storm: Irving Krick vs. the U.S. Weather Bureaucracy* (New York: G. P. Putnam's Sons, 1978).

measurements, and a variety of solar radiation measurement programs and associated data analysis.[231] However, there was no focused research program coordinated among the handful of groups doing theoretical work. Each took its own path, although MIT, NYU, and Caltech all were either funded by, or connected to, aviation interests – the driving force behind increasing interest in the science.

As the 1920s unfolded, the tightly-woven pre-war relationship between meteorology and climatology began to unravel. Geography departments settled on a climatology (both statistical and descriptive) model which would serve the needs of their economic and cultural sub-disciplines. Teacher's colleges remained non-technical, providing no opportunity for meteorology as a physical science to take hold. The U.S. Navy's need for advanced instruction in meteorology, coupled with the financial backing of the Guggenheim Fund, enabled MIT to establish a mathematics- and physics-based meteorology curriculum under the auspices of its aeronautical engineering department. With the hiring of Bergen School acolyte Carl-Gustav Rossby, the door opened to spread the air-mass and polar front theories among American meteorologists.

Research based on describing the atmosphere, which had tended to dominate the years before the Great War, went into decline. In the post-war years, research became increasingly theoretical and focused on the dynamical and physical properties of the atmosphere. Development of instrumentation, while perhaps not seen directly as atmospheric research, was necessary to the furthering

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[231] Conover, *The Blue Hill Meteorological Observatory*, 135, 156, 157, 163, 168, 172.

of the research agenda aimed at applying physical laws to atmospheric data. Scandinavian scientists visiting the United States brought new ideas and approaches, which could no longer be ignored. The graduate students of the 1930s, steeped in the Bergen School methods, would be critical for spreading them across the country. With war – particularly a war that would be fought in the air – looming in the late 1930s, knowledge of the atmosphere would prove to be of the utmost importance to a successful military outcome. That success would, in turn, gain disciplinary respect for a rapidly growing community of professional meteorologists.

ON THE PATH TO PROFESSIONALIZATION

As previously noted, the operational needs of the military services during World War I were responsible for a surge of interest in meteorological support services and the training necessary to ensure its provision. Once the war was over, the interest did not subside. In particular, increased aviation activity in both the civilian and military sectors as well as an awareness of the impact of weather conditions on commerce, agriculture, and health, led to increasing demand for additional educational opportunities, more trained meteorologists, and a variety of specialized forecasts from the Weather Bureau. The immediate post-war period was an opportune time for the far flung meteorology community to capitalize on this strong interest. Ultimately all these factors encouraged the organization of the

American Meteorological Society (AMS) at the 1920 American Association for the Advancement of Science (AAAS) meeting in St. Louis. Its stated purpose:

> The advancement and diffusion of knowledge of meteorology, including climatology, and the development of its application to public health, agriculture, engineering, transportation by land and inland waterways, navigation of the air and oceans, and other forms of industry and commerce.[232]

Considering that meteorology had been a topic of discussion since language had developed, and a part of natural philosophy since the Greeks, it is somewhat incongruous that no formal, broadly-based society had arisen to represent the professional interests of meteorologists in the United States before 1919. By the time the AMS was founded, the American Chemical Society (1876), the American Physical Society (1899), and the American Astronomical Society (1899) were firmly established.

One might assume that the AMS was the brainchild of either academic or Weather Bureau meteorologists. It was not. On the contrary, it sprang from the mind of a Signal Corps sergeant, Perez W. Etkes, a former student of Charles F. Brooks at the Signal Corps Meteorology School. In a letter to Brooks, Etkes argued that the aviators with whom he worked realized the importance of weather, but they did not have enough specific atmospheric information to find it valuable. Therefore, he proposed an "American Meteorological Institute" for the purpose of spreading meteorology "amongst the people" by establishing weather stations in schools and offering opportunities for graduate education via prizes and

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232 "The American Meteorological Society," *BAMS* 1 (1920): 1. The Meteorological Section of the AAAS was already in existence.

scholarships. Brooks was less than enthusiastic. However, he grudgingly acknowledged that it would not be that difficult to find over 100 members for a meteorological organization from the ranks of the Signal Corps, the Weather Bureau and meteorology educators. Even with limited dues, such a society could publish its own "periodical leaflet" containing items not normally published in *Monthly Weather Review*, which was then the only venue for publishing meteorological research results. The new leaflet would, nonetheless, foster meteorological research by providing an outlet for members' ideas and professional concerns. The more Brooks thought about, the better the idea seemed.

Pursuing Etkes's proposed organization, Brooks further consulted with meteorologists within the Weather Bureau and academia. Because most meteorological work was being done by the Weather Bureau and since the Association of American Geographers already had a niche for climatology (and by association meteorology), Brooks originally considered an organization of meteorology instructors. However, that seemed too restrictive. A general organization of both professionals and amateurs, which allowed meteorologists to get together and talk struck Brooks as the better approach. The primary stumbling block: the lack of meteorologists. As William Morris Davis, professor emeritus of geology at Harvard, wrote to Brooks: "You can get a lot of men who dabble; and a lot of men who add up temperatures and divide by thirty; but meteorologists are birds of a different feather."[233]

Despite Davis's warning, Brooks persisted. Brooks saw the future AMS as a way to promote badly needed meteorological instruction and research. The membership base he chose emphasized teachers, Weather Bureau employees, and current and former Signal Corps meteorologists and Navy aerologists. Over 900 – significantly more than the original 100 Brooks thought could be enticed to join – persons equally divided between professionals and amateurs signed up the first year.[234]

To fulfill its mission of promoting research and instruction, the AMS members immediately formed eleven committees to address these two areas: "the advancement and diffusion" of meteorology and "the development of numerous applications of meteorology to human affairs." The reports of topics discussed at the initial committee meetings give an indication of what were seen as the primary disciplinary goals in this early period.[235]

The Research, Meteorological Instruction, Public Information, and Membership committees composed the groups working on "advancement and diffusion." The Research Committee, with all but one member from the Weather

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[233] William Morris Davis to Charles F. Brooks quoted in Charles F. Brooks, "Our Society's First Decade," *BAMS* 11 (1930): 10. Morris had had experience in founding professional organizations. He had founded the Association of American Geographers in 1904. For a biographical sketch of Davis, see R. Daly, "William Morris Davis," *Biographical Memoirs* (Washington, D.C.: National Academy of Science, 1945), 23: 263-303.
[234] Charles F. Brooks, "Our Society's First Decade," *BAMS* 11 (1930): 8-12.
[235] "Committees," *BAMS* 1 (1920): 5.

Bureau staff, observed that a strong independent group of meteorologists in the United States was lacking. Therefore, the committee's initial work would need to be "educational" so as to both spark interest and direct subsequent research ideas down an appropriate path.[236] The Meteorological Instruction Committee, as discussed above, focused on expanding meteorological education throughout all levels of the school system.[237] The mission of the Public Information Committee was one that could be repeated by just about any scientific organization through the twentieth century: to eradicate popular errors concerning weather and meteorology and replace them with correct information by enlisting the aid of the newspapers and other media outlets. However, one of the "deeply rooted beliefs" that this group wanted to eradicate was that "the operations of mankind can have an important influence upon weather and climate."[238] In 1920, the idea that mankind's actions could impact weather and climate had not taken hold among professional meteorologists.

Committee members addressing meteorological applications covered Physiology, Agriculture, Hydrology, Business, Commerce (transportation issues), Marine, and Aeronautical Meteorology. With the exception of the Physiology Committee, the purposes of the others would remain self-evident into the twenty-first century. Their missions were to encourage meteorological research in areas that would be directly applicable to a given commercial segment and add either

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[236] "From the Committees: Research," *BAMS* 1 (1920): 17-18.
[237] "From the Committees: Meteorological Instruction," *BAMS* 1 (1920): 16.
[238] C. F. Talman, "From the Committees: Public Information," *BAMS* 1 (1920): 17.

economic value or increase safety. The Physiology Committee's mission is perhaps not so obvious. Led by Yale geographer Ellsworth Huntington (1876-1947), committee members included representatives from medicine, sanitary engineering, and hydrography.[239] Its purpose was to bring meteorological information to a variety of disciplines concerned with the connection of weather conditions to health. To do this, the committee intended to increase the sharing of research results across disciplinary boundaries, and to teach physicians how to take and use simplified meteorological measurements to improve the health of their patients. Members were concerned that not enough emphasis was put on tying weather events and conditions to the general health of the population.[240]

No matter their primary mission, all AMS committees were assigned to "spread the word" about meteorology, climatology and their economic and cultural importance. Considering the sorry state of the discipline in post-World War I America, this would prove to be no small task.

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[239] On Huntington's views about climate and environmental determinism, see David Livingstone, *The Geographical Tradition: Episodes in the History of a Contested Enterprise* (Oxford: Blackwell, 1992), 230-231. For a biography of Huntington, see Geoffrey J. Martin, *Ellsworth Huntington; His Life and Thought* (Hamden, CT: Archon Books, 1973).

[240] Ellsworth Huntington, "From the Committees: Physiological Meteorology," *BAMS* 1 (1920): 18-19. Huntington's ideas were widely published in the popular press. For example, see Bruce Barton, "What the Weather Does to You," *American Magazine* 97 (June 1924): 38-39+; Ellsworth Huntington, "The New Astrology," *Century* 110 (May 1925): 106-114; Ellsworth Huntington, "What the Weather Does to Us," *Scribners* 79 (June 1926): 571-577; E.E.F., "The Weather and Our Feelings," *Forum* 74 (September 1925): 390-392. There has been very little historical research done on medical meteorology. For an introduction, see Theodore S. Feldman, "Meteorology, Medical," in Gregory A. Good, ed., *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena* (New York and London: Garland Publishing, Inc., 1998), 2: 574-576; Genevieve Miller, "'Airs, Waters, and Places,' in History," *Journal of the History of Medicine* 17 (1962): 129-140; James C. Riley, *The Eighteenth-Century Campaign Against Disease* (New York: St. Martins, 1987); and Frederick Sargent II, *Hippocratic Heritage: A History of Ideas about Weather and Human Health* (New York: Pergamon Press, 1982).

Certainly American meteorology had had its moments of brilliance: the early theoretical work of nineteenth century meteorologists James Pollard Espy (1785-1860), Cleveland Abbe (1838-1916), and William Ferrel (1817-1891), and the research-based Smithsonian meteorology project (1849-1874). However, as the pressure for weather services increased, the flame of weather research started to dim. Soon, the primary mission of meteorology, as embodied in the Weather Bureau, was to produce forecasts and warnings. Disciplinary advances would not, however, arise from the daily forecasting routine. To become a scientifically respected professional community, meteorology and its practitioners would need to develop an active research component.

For as long as anyone could remember, meteorology had first and foremost been involved with *collecting* data. Based on experience, those data were used to make forecasts. But the act of *collecting* does not make a science. If meteorology were going to move from art to science, then meteorologists had to apply mathematical and physical principles to the data. As George Washington University-trained mathematical meteorologist Edgar W. Woolard put it, the processes of weather are "simply examples of the operation of ordinary physical laws." He acknowledged that those laws would need some special treatment and despite all the collecting of data, the needed data (primarily an issue of spatial coverage and lack of upper air reports) were just not there. He hoped that people with solid backgrounds in physics and mathematics could be enticed into addressing meteorological problems. He viewed as positive the work of Lewis F.

Richardson (on numerical weather prediction) and Vilhelm Bjerknes of the Bergen School as steps in the right direction, but much more work was needed. It is not clear, however, just how much Woolard understood of the Richardson work. In the discussion that followed the presenting of his paper, Woolard was asked how far in advance Richardson's method could be used to forecast the weather. His response of "six to twelve hours" when Richardson himself figured it would take 64,000 human "computers" working full-time just to keep up with the weather as it happened, shows a lack of comprehension of the magnitude of the problems that faced numerical weather prediction.[241]

While the Weather Bureau was concerned with practical, day-to-day problems, its meteorologists were also aware that those practical problems would not see a solution without research on theoretical issues. Chief Marvin presented a list of current problems in meteorology to the Meteorology Section of the American Geophysical Union (itself just founded in 1919) meeting in 1923. The first item on his list: the problem of solar radiation and its influences on terrestrial weather. As discussed in the previous chapter, there was great interest at this time in how variations in solar radiation controlled the weather – an indication of the extent to which weather forecasting was seen as an *astronomical* problem. Several research projects to measure incoming radiation under a variety of conditions and

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[241] Edgar W. Woolard, "Recent Contributions to Mathematical Meteorology," *BAMS* 3 (1922): 96-98. Lewis F. Richardson, *Weather Prediction by Numerical Processes* (New York: Dover Publications, 1965), 219. Richardson, a Quaker pacifist, had written his work during duty with the Friends Ambulance Unit during World War I. After the war, he secured a position as a lecturer of mathematics and physics at Westminster Training College, London.

in a number of different locales were being actively pursued by Weather Bureau and academic meteorologists, and astronomers at the Smithsonian Institution. Knowledge of general atmospheric circulation was still sketchy. Meteorologists needed to determine how air was exchanged across the equator and within the hemisphere. As of 1923, the northern hemisphere map, which had been available before World War I as a result of data sharing, could still not be produced because some observation networks in Europe had not been completely restored. Other questions dealt with the causes and/or events which led to the development of cyclones and anticyclones, West Indian hurricanes, and the difficulties of providing forecasts for marine and aeronautical interests. Long-range forecasting based on so-called "sequences" of weather conditions and periodicities of weather and climate also made Marvin's list. In the discussion that followed, his fellow meteorologists expressed their opinion that physics needed to be applied to these problems and that the discipline needed to get away from describing distributions of temperatures and precipitation without looking for explanations for their occurrence. Determining how air circulated in the atmosphere was of critical concern.[242]

Two years later, in 1925, Woolard again made a case for theoretical work. This time he presented the "origin, nature, structure, and maintenance of ordinary cyclones and anticyclones" as being a major unsolved problem. Discussing the theoretical work of the Bergen School and others, he made the point that this

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[242] C. F. Marvin, "The Status, Scope and Problems of Meteorology," *BAMS* 4 (1923): 73-76. Talk originally presented at the Annual Meeting of the American Geophysical Union, Meteorology Section.

problem was not just of theoretical importance. If meteorologists were unable to determine how cyclones and anticyclones developed and dissipated, then forecasting was not going to become more accurate.[243] He realized that the problems of atmospheric circulation were fundamentally ones of mechanics and thermodynamics. Thus, given a complete three-dimensional set of observations, how would one use the laws of physics to determine what the atmospheric conditions would be some time in the future? Woolard did not think that meteorology could be a credible science until researchers were on the path to an "*exact solution*" (emphasis mine). However, neither pure mathematics nor mathematical physics nor observational meteorology had been sufficiently developed to provide such an exact solution. Thus, weather services had to settle for "inexact and fallible" empirical methods of forecasting, and theorists had to settle for qualitative or statistical explanations instead of a complete mathematical one. Bergen School meteorologists were using graphical methods to solve the differential equations involved in these mathematical descriptions of the atmosphere because the direct solution was not possible with available techniques. However, it would be this mathematical approach which would ultimately allow insight into the mechanisms that controlled atmospheric processes.[244]

While some meteorologists were concerning themselves with the problems of atmospheric circulation, towards the end of the 1920s others were increasingly intrigued by the thought of long-range weather forecasting. Possible lines of

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[243] Edgar W. Woolard, "Theories of the Extratropical Cyclone," *BAMS* 6 (1925): 49.
[244] Edgar W. Woolard, "The General Problem of Theoretical Meteorology," *BAMS* 6 (1925): 78-81.

research for forecasts that would come several days or more in advance of the event included looking for patterns in the collected empirical data; determining the primary causes of unseasonal changes; examining the influence of topography which might sustain or ramify the initial changes in a weather pattern; or, when all else failed, looking at a combination of all these things. The empirical route could lead to some results in the short run, but it would take years of research before such forecasts could be made as a result of understanding atmospheric processes.[245] The research on the variability of the solar constant, primarily being pursued by astronomers, was also related to long-range forecasting and the possibility of climatic influence over time.[246] A serious problem with most long-range forecasting efforts was that the meteorology community did not grant long-range prediction any scientific standing. Consequently, students who might have been interested in pursuing this forecasting problem as a research opportunity were discouraged from entering it. Moreover, the government – the primary source of research funds – was not eager to provide the financial support needed to carry out such a program.[247]

The credibility issue as well as the funding problems evaporated with the passage of the Bankhead-Jones Act of 1935. An outgrowth of the Dust Bowl conditions in the Great Plains, the Bankhead-Jones Act provided funds for basic

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[245] Charles F. Brooks, "An Outline of the Study of World Weather and Long Range Weather Forecasting," *BAMS* 8 (1927): 31-32.
[246] E. A. Beals, "Is it Possible to Predict California's Rainfall Several Months in Advance?" *BAMS* 8 (1927): 103-107.
[247] L. E. Blochman, "The Difficulties of Long-Range Forecasting," *BAMS* 10 (1929): 222-223.

research that would lead to the solution of agricultural problems.[248] As related to meteorology, it provided funds for studying long-range weather prediction and the effects of weather conditions on crops and livestock. The general aim of the research was to attack the general circulation problem (a purely theoretical issue) by examining the day-to-day change in major features and how the resulting patterns affected weather patterns in different parts of the country. Additionally, researchers looked for empirical "clues" that could be used to anticipate the future state of the general circulation pattern, i.e., the location of semi-permanent high and low pressure areas and the wind fields that accompanied them.[249] Of course, there were differences of opinion as to what influenced the global circulation. Astronomers pinned their hopes on solar influence, including changes in sun spot patterns and other solar radiation changes. Oceanographers made their claim that the oceans were important to any studies of atmospheric circulation and that research into the interaction between the ocean and atmosphere was a necessity.[250] All seemed to agree that while statistics could give tantalizing hints of connections between patterns and weather phenomena, there could be no substitute for physical understanding.[251] Rossby, heading up the program at MIT, set out a research plan for long-range forecasting during a conference in mid-1937. It included

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[248] Dupree, *Science in the Federal Government,* 364.
[249] H. C. Willett, "The Importance of Observations from the Upper Atmosphere in Long-Range Weather Forecasting," *BAMS* 18 (1927): 284-287.
[250] For an introduction to the history of air-sea interaction, see Henry Charnock, "Ocean-Atmosphere Interactions," in Gregory A. Good, ed., *Sciences of the Earth: An Encyclopedia of Events, People, and Phenomena* (New York and London: Garland Publishing, Inc., 1998), 2: 623-625.
[251] Horace Byers and L. F. Page, "Conference on Long-Range Forecasting Held at the Department of Agriculture, Washington, D.C., April 30, 1937," *BAMS* 18 (1937): 371-373.

investigations of anticyclogenesis (the development of high pressure areas), particularly as related to the influence of lateral mixing; a study of how systems become dynamically unstable; and the development of a theory of the flow patterns in the atmosphere as shown on isentropic charts.[252]

Of course, not all research paths were driven by theoretical interest. The impact of weather elements and conditions on aviation safety prompted a number of fruitful efforts. Among them was an examination of aircraft icing – in particular research into what conditions seemed to favor its development and how it could be avoided – by the meteorologists of the Blue Hill Observatory. Unfortunately, efforts to rid airplanes of ice had not been successful. Therefore, meteorologists focused on guiding airplanes around clouds that could contribute to the problem.[253] Similarly, fog severely impacted visibility at both take-off and landing sites – the former being less critical as long as there was no emergency forcing a take-off. However, fog at the landing site could prevent the plane from coming in. If an alternate landing field was not close by, that could doom an aircraft running low on fuel. Studies at Blue Hill addressed both fog development and forecasting in addition to research into dissipating fog that had formed. Blue Hill's McAdie thought that early morning ground fog would be the easiest to dissipate – by

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[252] E. W. Woolard, "Conference on Long-Range Forecasting, Cambridge, Mass., Aug. 23, 1937," *BAMS* 19 (1938): 123-124. An isentropic chart is a synoptic chart with plotted meteorological elements (pressure, wind, temperature, moisture) on a surface of constant potential temperature. Potential temperature is the temperature that an unsaturated parcel of dry air would have if brought adiabatically (i.e., without heat transfer from or to its environment) from its initial state to a standard pressure of 1000 millibars (or 100 kilo Pascals).

[253] C. Fitzhugh Talman, "Ice Coating on Aeroplanes," *BAMS* 9 (1928): 106-107.

spraying it will electrified water. In fact, he envisioned ridding entire harbors of fog in this manner. However, it had yet to be tried.[254]

In the early years of the Society, most research in meteorology was government funded (limited though it was). Consequently, this research needed to show practical results in a fairly short period of time. As time passed and universities established meteorology programs – often in conjunction with aeronautical engineering programs – research agendas took on a more theoretical look. Academics in the United States sought input from European meteorological research centers, either by bringing in visiting scholars or sending personnel to study in Europe.

The variety and extent of meteorological research projects grew dramatically in the interwar period spurred by the active influence of the American Meteorological Society's members. Before the Society's creation, the only research publication venue was the Weather Bureau's *Monthly Weather Review.* Although not strictly an in-house organ, i.e., meteorologists outside the Bureau published their work in it, there was no opportunity for the larger meteorological community to influence the research agenda.

This changed when the AMS organized and produced its own publication: *The Bulletin of the American Meteorological Society.* A mixture of short research reports, book reviews, reprints from the popular press dealing with weather related issues, and "gossipy" news about members, *BAMS* served to guide the research

[254] Alexander McAdie, "The Hazard of Sub-Cooled Fog and Ice-Storms in Aviation," *BAMS* 10 (1929): 37-38.

agenda by keeping the membership informed. This widely disseminated meteorology community news built and strengthened professional contacts between the Weather Bureau, military, and academic meteorologists – contacts that would be critical to the eventual success of numerical weather prediction.

METEOROLOGY ON THE EVE OF WAR

By 1938, just three years away from another major war, the meteorology community in the United States had grown larger and more cohesive as a result of the demands of the First World War and the growth of the aeronautics industry in the years that followed. The Weather Bureau still employed the largest block of meteorologists: an underpaid, under-funded, and under-appreciated group of people which remained entrenched in the old ways of doing meteorology. Widely maligned for being reactionary and unwilling to try new ways, this criticism missed the mark in one crucial respect: with funding stagnant or falling (or even when rising, not even keeping pace with rising costs), the Bureau had little choice but to stay with the old ways. The new Bergen School methods demanded increased upper air observations, more surface observations (both spatially and temporally), and advanced training for the staff. Lack of funds alone was enough to prevent the implementation of new techniques. Apparently unable to successfully lobby Congress for sufficient funds, Bureau meteorologists gamely continued to perform research when they could, with the limited funding and facilities at their disposal. They recognized that although practical results in forecasting were desirable, basic theoretical work needed to come first before physical knowledge of the atmosphere would explain observed weather phenomena.

Starting in the 1920s, military meteorology – as represented by Navy Aerology and the Army Signal Corps Meteorological Service – had been in a period of retrenchment following the draw-down after the war. Nevertheless, aviation missions had not stopped at the end of the war, and demands for services had increased even as trained personnel decreased. In 1928, the Navy's solution – to form its own graduate program at a civilian school – had helped to encourage graduate education in meteorology for everyone in the community. The pursuit of the Bergen School methods by the Navy's aerological community leader, F. W. Reichelderfer, meant that the Navy had been the first to put the air-mass theory to the test in making its forecasts. However, research in the military services was generally tied to instrumentation, and not to theory.

Despite its growth since the end of World War I, the U.S. academic meteorology community remained very, very small – less than few dozen people. Professional meteorologists numbered a few hundred – about the same as astronomers.[255] In contrast, in 1932 there were 2500 physicists.[256] Few faculty members teaching meteorology actually had studied meteorology as graduate students. Meteorology tended to be a subject tossed into geography courses or occasionally added to a physics course. It did not exist as its own academic discipline. However, the need was there and more men entered the field. Teacher

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[255] Lankford, *American Astronomy,* 362. There were 362 professional astronomers in 1940.
[256] Kevles, *The Physicists,* 202.

training institutions, although providing non-technical instruction, still added a considerable number of meteorology courses to their curricula, which helped to stir interest among all segments of the school-aged population. The establishment of the first graduate meteorology programs, first at MIT in 1928 and later at NYU and Caltech, gave a significant boost to the numbers of mathematically and physically savvy meteorologists who were ready to move in to research positions and thus advance the development of the discipline. As part of the rise of the earth sciences in the early twentieth century, the founding of the American Meteorological Society in 1919 provided the pathways for improved communication of ideas among the generally isolated individual practitioners of the atmospheric sciences.

The interwar period thus nurtured a slow, steady advancement of scientific theory, scientific practice, and scientific education in meteorology. But the dramatic events of World War II were soon to place great demands on meteorologists, as national defense needs stretched their capacity to respond to increasingly sophisticated operational requirements. The modest gains of the 1920s and 1930s would very quickly be put to the test.

CHAPTER 4
AN EXPANDING ATMOSPHERE: THE WAR YEARS (1939-1945)

Throughout the interwar period, most meteorological training in the United States had taken place on-the-job through one of the nation's three weather services – those overseen by the Army, Navy and Weather Bureau. Civilians had enrolled in graduate meteorology programs starting in the early 1930s, but enrollment (and career opportunities) remained minimal despite expanding aviation requirements for meteorological support. With the advent of World War II, the small number of available meteorologically qualified people was insufficient to meet either domestic or military demand.

The University Meteorological Committee (UMC), under the direction of Carl-Gustav Rossby at the University of Chicago, established and coordinated an accelerated meteorology program to meet the needs of both civilian and military agencies. Military demand led to a flood of new students, most of whom would never have considered meteorology as an academic or career field prior to the war, into a previously very small scientific discipline. The training of thousands of new meteorologists within a five year period was an extraordinary event in the history of science in the United States which would dramatically change the face of meteorology. The coordination undertaken to provide this training and to assimilate these new meteorologists into the scientific community in the postwar years would prove crucial to the professionalization and advancement of the atmospheric sciences.

CHANGING LEADERSHIP – EXPANDING INSTRUCTION

As 1938 drew to a close, the Weather Bureau was moving slowly into the research arena. It established a small unit to direct and supervise research projects. The unit's goal, according to Bureau Chief Willis R. Gregg, was to foster cooperation with organizations conducting meteorological research and to coordinate its research efforts with those of other institutions. Gregg's personal delivery of that message at the AMS annual meeting was prevented by his untimely death from a thrombosis at the age of 58 on 14 September 1938.[257] He had led the Weather Bureau for less than five years.

Millikan, advised of Gregg's death via telegram, sprang into action. Within 24 hours of Gregg's passing, Millikan took the initiative to recommend a new Chief. His suggestion: Navy Commander Francis W. Reichelderfer. Millikan was concerned that if the National Academy of Sciences did not move quickly to make a solid recommendation, it would be unable to "prevent political influences from getting into this appointment. It is a very vital one for the scientific interests of the country."[258] A day later, Secretary of Agriculture Henry Wallace called Karl T. Compton, a member of the Science Advisory Committee on the Weather Bureau, to

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[257] W. R. Gregg, "Introductory Remarks," *BAMS* 20 (1939): 129-132.

[258] Telegram from Albert L. Barrows to Millikan, 15 September 1938. Millikan to Frank R. Lillie and Ross G. Harrison, 15 September 1938 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).

Washington for consultations on Gregg's replacement.[259] By 5 October 1938, Millikan reported to the other advisory committee members that only two names – Reichelderfer and Rossby – had surfaced from more than one person. Other recommendations included current and former Weather Bureau employees who were in their sixties. Millikan clearly did not want an old chief – he wanted someone who was young enough to vigorously transform the Weather Bureau into an organization that could provide "effective and progressive" service.[260] Acting on a request from Millikan, Assistant Secretary of the Navy Charles Edison arranged for Reichelderfer to be flown to Washington from the west coast for an interview with the entire advisory committee on 24 October. Rossby – the other contender – would also be in Washington for an interview.[261]

Compton, writing in favor of Rossby, and Chief of Naval Operations, Admiral William D. Leahy, writing in favor of Reichelderfer, were glowing in their praise of each candidate. Rossby, Compton wrote to Wallace, was the "unquestioned leader" in meteorology in the United States, both as an "investigator and a teacher." Indeed, Compton continued, "[The] majority of the present trained meteorological personnel in this country are his pupils." Further, the leader of the British delegation of the International Congress of Applied Mechanics, had recently

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[259] Barrows to Lillie, 16 September 1938 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).
[260] Millikan to Weather Bureau Advisory Committee (Isaiah Bowman, Karl Compton, H. D. Hughes, and J. B. Lippincott), 5 October 1939 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).
[261] Record of telephone call: Barrows and Compton, 18 October 1938 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).

stated that "[Rossby] has now become the leading meteorologist in the world." He would, in short, be a very big asset to the Weather Bureau. Similarly, Admiral Leahy wrote highly of Reichelderfer. Although the Navy would be sorry to lose such a valuable officer, taking the long view, Leahy thought it was in everyone's best interests to have strong, positive leadership at the Weather Bureau. He recommended Reichelderfer without reservation.[262]

Millikan's committee chose Reichelderfer to aggressively carry forward the changes in Weather Bureau structure and culture initiated by Gregg.[263] Reichelderfer was serving as the Executive Officer aboard USS *Utah* when he was tapped by Agriculture Secretary Henry A. Wallace to take over the Weather Bureau for three years. Reichelderfer was perfectly happy in the Navy and had an excellent career ahead of him. Indeed, by taking a three year leave to head the Weather Bureau, he was putting his Navy career at risk without any compensating financial rewards. Despite the possible negative career consequences, he accepted Wallace's offer and became the Weather Bureau Chief at the end of 1938. For Reichelderfer, the opportunity to contribute to a field "ripe for progress" was too good to turn down.[264] He in turn convinced Rossby, his long-time colleague, to take a leave of absence from the MIT Meteorology Program and become the Assistant Chief of Research and Education. (Recall that Reichelderfer had been one of those behind

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[262] Compton to Wallace, 24 October 1938; Leahy to Millikan, 20 October 1938 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).
[263] Whitnah, *A History of the U.S. Weather Bureau,* 132.
[264] Reichelderfer to Wallace, 15 November 1938 (Reichelderfer papers, B4, F9).

Rossby's appointment to MIT ten years before.)[265] With the appointments of Reichelderfer and Rossby arranged, the report of the Subcommittee on the Weather Bureau stated, "[The] direction of the Weather Bureau now possesses a prestige such as it has never before enjoyed."[266]

Although Rossby would have brought a different sort of spark to the Weather Bureau's top post, he did not want the job. Being the Weather Bureau chief would have interfered with his own research program.[267] Rossby's mission during his three year appointment with the Weather Bureau would be to expand its research and instruction programs. Because of the lack of educational opportunities in meteorology, the Weather Bureau had become saddled with many poorly trained people. Rossby intended to substantially raise the professional standing of the Bureau's staff. [268] Reichelderfer and Rossby would ensure that Weather Bureau staff members would be offered significantly expanded instructional opportunities under their leadership. As they had since World War I, aviation requirements would be the primary spur for these efforts. Indeed, in 1939, the money allotted for so-called general weather services had *declined* by $200,000 over a ten year period, while funding for aviation weather services had continued its steady climb.[269] This completely absurd funding situation occurred at a time of increasing demand by all

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[265] H. C. Willett, "Carl-Gustaf Arvid Rossby," *BAMS* 26 (1945): 243-244.
[266] Report of the Subcommittee on the Weather Bureau, April 1939 (NAS, NAS Organization 1938-1939, Government Relations and Science Advisory Committee: Subcommittee on Weather Bureau).
[267] Reichelderfer to Millikan, 28 November 1938 (Reichelderfer papers, B4, F9).
[268] "Expanding Instruction in Meteorology and Climatology," *BAMS* 20 (1939): 206. Byers, "Carl-Gustaf Rossby, the Organizer," 57.
[269] F. W. Reichelderfer, "The Weather Bureau Program for 1939," a talk presented before the Atlanta Meeting of the American Meteorological Society, 21 April 1939 (Reichelderfer papers, B1, F7).

sectors: agriculture, forestry, transportation, industry. The Weather Bureau was often criticized as being too "conservative." Given their funding structure, it would have been surprising to find them moving out to do cutting edge work. The Weather Bureau's situation was analogous to that of an overworked, underpaid employee. It would be completely unrealistic to expect such an employee to take innovative steps to do more with less. So it was with the Weather Bureau.

The expansion of instructional programs had already been directed by Section 803 of the Civil Aeronautics Act of 1938. This act directed the Weather Bureau to send not more than ten of its members for graduate training in meteorology at government expense each year. The Bureau could send staff members to either civilian or government institutions. It planned to select and send four staffers during the 1938-1939 academic year.[270] However, the number of institutions offering such training was still limited. MIT and NYU offered theoretical meteorology programs at the graduate level – and Rossby sent some of the Weather Bureau personnel there – while Caltech offered a master's program that was designed to meet the needs of industry.

The Caltech program – started in 1934 by German-born and educated geophysicist and seismologist Beno Gutenberg (1889-1960) – had begun its life as a course in atmospheric structure.[271] Meteorology, considered to be a branch of earth physics, was placed within the geology department. After the 1933 crash of

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[270] "University Training in Meteorology under Civil Aeronautics Act," *BAMS* 19 (1938): 259-260.
[271] For a short biography of Gutenberg, see Leon Knopoff, "Beno Gutenberg," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1998), 76: 114-147.

the Navy's airship USS *Akron* and the subsequent realization of the importance of meteorology to flight, meteorology moved under the aegis of the aeronautics department. Caltech offered its first regular courses in meteorology to seven students during the 1933-1934 academic year. Enrollment increased each year as graduates of this industrially-focused curriculum were hired by the airlines. The department chairman, Irving P. Krick, who was much enamored of "weather typing," i.e., the matching of past weather patterns to predict future weather, did not run a theoretical department.[272] Therefore, its offerings would prove to be of little use to the Weather Bureau. In fact, Reichelderfer detested Krick, a self-promoting braggart who routinely argued that he could forecast for the entire country better than the Weather Bureau could – which did not endear him to the Bureau. However, in a few years, Krick's claims of long-range forecasting ability would catch the attention of the father of the U.S. Air Force, Army Air Force General Henry H. "Hap" Arnold (1886-1950).[273] Krick created a special curriculum for the nascent Air Weather Service and Caltech became its graduate program of choice.[274]

The entrepreneurial Rossby was always on the lookout for opportunities to promote meteorological instruction and establish meteorology programs. Such an

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[272] Lewis, "Cal Tech's Program in Meteorology," 69-81.
[273] For additional information on General Arnold see Thomas M. Coffey, *Hap: The Story of the U.S. Air Force and the Man Who Built It, General Henry "Hap" Arnold* (New York: Viking Press, 1982); Dik A. Daso, *Hap Arnold and the Evolution of American Airpower* (Washington, D.C.: Smithsonian Institution Press, 2000); Flint O. DuPre, *Hap Arnold: Architect of American Air Power* (New York: Macmillan, 1972). For his relationship to the Air Weather Service see Fuller, *Thor's Legions*.
[274] Fuller, *Thor's Legions*, 32-33.

occasion presented itself when Jacob Bjerknes (1897-1975), son of Bergen School founder Vilhelm Bjerknes, was trapped in the United States after Germany occupied Norway. Rossby sprang into action. He was eager to establish a theoretical meteorology program on the west coast which would serve as an alternative to the non-theoretical Caltech offerings. He first persuaded Bjerknes to go to UCLA. Then Rossby persuaded UCLA to start a meteorology program within the physics department in 1940 with Bjerknes as the chair. Norwegian Jörgen Holmboe (1902-1979), another Bergen School-trained Scandinavian transplant, taught dynamic meteorology. Hungarian-born physicist Joseph Kaplan (1902-1991) taught his specialty: upper atmospheric physics.[275] They were assisted by several operational meteorologists from the Weather Bureau's district forecasting center in Los Angeles. The "Announcements" section of the AMS *Bulletin* proclaimed Los Angeles "a leading center of meteorological professional activity," due to the additions of the UCLA program and a new district forecasting center to the Caltech program.[276]

While Rossby was arranging employment for J. Bjerknes, he was also expanding the Weather Bureau's in-house training program from the Washington, D.C. central office to five district offices: Chicago, Washington, New Orleans,

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[275] "Meteorological Education in the United States," 126-141.

[276] "Announcements," *BAMS* 21 (1940): 308-309. For biographical sketches of J. Bjerknes, Holmboe, and Kaplan, see Arnt Eliassen, "Jacob Aall Bonnevie Bjerknes," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1996), 68: 3-22; Morris Neiburger, James G. Edinger, and Norton G. Wurtele, "Jörgen Holmboe, Meteorology: Los Angeles," *1980, University of California, In Memoriam* (http://sunsite.berkeley.edu:2020/dynaweb/teiproj/uchist/inmemoriam/inmemoriam1980/@Generic_BookTextView/2343); and William W. Kellogg and Charles A Barth, "Joseph Kaplan," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1998). 74: 178-191.

Denver, and San Francisco. MIT graduate and Rossby protégé Horace Byers (1906-1998), who desperately wanted out of the Washington-area bureaucracy, offered to make the move to Chicago. Victor Starr – another graduate of MIT's Ph.D. program – decided to go with him. In Byers's account of the story, both he and Starr were getting tired of training Weather Bureau personnel. Byers decided to check out the interest of the University of Chicago's physics department in meteorology. He discovered that the department head had been part of the Signal Corps balloon project during the First World War. As a result, both Starr and Byers were invited to give talks. Soon Byers was invited back by the vice president of the University for a lunchtime discussion about the possibility of establishing a meteorology program within the physics department. Although the Chicago officials suggested that Jacob Bjerknes or another member of the Bergen School would be a good choice to start and lead the program, Byers successfully argued for Rossby (who, of course, had trained at the Bergen School). The program started with thirteen courses in eight subject areas in the fall of 1940. Starr, and Weather Bureau meteorologists Harry Wexler (1911-1962) and ozone expert Oliver Wulf (1897-1987) filled out the new team.[277] They were assisted by physicist and future vice-president of research for Ford Motor Company Michael Ference (1911-1996), who specialized in the upper atmosphere, and geographer H. M. Leppard (Ph.D. (1928), Chicago). Rossby came on-board as a visiting professor in the second

[277] Byers, "The Founding of the Institute of Meteorology," 1343.

quarter and formally left the Weather Bureau in 1941.[278] With that, a "seat of meteorological education" came into being at the University of Chicago.[279]

UCLA, the University of Chicago, Caltech, MIT and NYU thus became the centers for professional meteorological education in the United States: The "Big Five." With the exception of Caltech, the Big Five were dominated by Bergen School polar-front theory reinforced by the presence of Scandinavians who had been part of that meteorological community. All five schools would prove crucial to the provision of meteorological instruction in support of national defense as the United States moved closer to war.

WAR AND WEATHER

As part of the rapid and radical demobilization of military personnel after World War I, the Army and Navy reduced their weather service staffs down to skeleton crews. Military planners assumed that the military weather services could come up to speed quickly in the event of a national emergency beyond the geographic range of the Weather Bureau. In the absence of such a threat, all three weather services exchanged data and reports to increase efficiency and avoid duplication of effort.[280]

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[278] Byers, "Carl-Gustaf Rossby, the Organizer," 58. For biographical information on Byers, see Roscoe R. Braham, Jr., and Thomas F. Malone, "Horace Robert Byers," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 2001), 79: 32-49. On Wexler, see M. Neiburger, "Harry Wexler," *BAMS* 43 (1962): 579-580. On Wulf, see Harold S. Johnston, "Oliver R. Wulf," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 2001), 79: 396-412. On Ference, see Julius J. Harwood, "Michael Ference, Jr.," *Memorial Tributes National Academy of Engineering* (Washington, D.C.: National Academy of Sciences: 2000), 9: 77-80.

[279] "Announcements," *BAMS* 21 (1940): 306.

[280] The effect of rapid demobilization on scientific research efforts connected with the WW I are discussed in Kevles, *The Physicists*, 147; Dupree, *Science in the Federal Government*, 324-325.

While the Army and Weather Bureau provided their meteorological training in-house, the Navy arranged for graduate meteorological educations for its aerological officers at MIT starting in the late 1920s under the direction of Rossby. This program helped the Navy increase the number of its aerological officers from a total of two in 1925 to 24 in 1934, but by 1940 the total number of Navy aerological officers had dropped back down to eighteen.

A similar dearth of meteorological officers in the Signal Corps contributed to losses of aircraft and crews during bad weather, a situation that led to the radical restructuring of meteorological support. The Signal Corps never had more than eleven meteorological officers serving at any given time between 1921 and 1936. The Weather Bureau provided most of the forecasts because Army weather stations were primarily places to gather and disseminate information. The physically distant Weather Bureau forecasters were unable to meet with their clientele who were, increasingly, army aviators. The loss of some of those aviators was a problem which had to be addressed. The Army Air Corps took over sponsorship of weather services from the Signal Corps in 1937. The nascent Air Weather Service then had forecasting responsibility for all aviation units and for ground forces at the division level and above. Signal Corps personnel continued to forecast for ground organizations smaller than divisions. With the move to the Army Air Corps, officers within the Signal Corps who desired to join the new service had to qualify as pilots. By 1939, the Air Corps weather service had a total of 30 officers and 388 enlisted men.[281] Even with these additions, the military weather services were still understaffed for the conflict to come. At the time, no one realized how important meteorology would prove to be in the execution of the next war.[282]

As war loomed, meteorologists in all the weather services realized that there were not going to be enough weather forecasters available to support the United States, domestically or militarily. The extent of their potential training mission became more apparent after President Roosevelt's May 1940 announcement that 50,000 aircraft would be added to the military arsenal. The first accelerated (three month long) training course in meteorology was conducted at MIT by Norwegian-born and trained Sverre Petterssen (1898-1974).[283] That course graduated its first class of Army and U.S. Weather Bureau-Civil Aeronautics Administration (CAA) members in September 1940. Within the year, more extensive nine-month courses were either underway or planned for the "Big Five" meteorology programs.[284]

All applicants were expected to have strong technical backgrounds regardless of their eventual assignment. Depending on the branch of service, applicants had to be either seniors or college graduates with majors or degrees in

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[281] Bates and Fuller, *Weather Warriors*, 31-48.
[282] Brig. Gen. D. N. Yates, "Remarks Made During the Washington Meeting Discussion on Problems of Industrial and Commercial Applications of Meteorology," *BAMS* 28 (1947): 410.
[283] For more information on Petterssen's early life in Norway as well as his later professional life, see Sverre Petterssen, *Weathering the Storm: Sverre Petterssen, the D-Day Forecast, and the Rise of Modern Meteorology*, ed. James Rodger Fleming (Boston: American Meteorological Society, 2001). Since Rossby had departed for the Weather Bureau, Petterssen became the chair of the newly created Department of Meteorology at MIT. For a short description of meteorology at MIT during World War II see John Burchard, *Q.E.D.: M.I.T. in World War II* (New York: The Technology Press, 1948). Burchard does not address the existence of Rossby's program within aeronautical engineering that had been in the place since the late 1920s.
[284] Bates and Fuller, *Weather Warriors*, 52.

science or engineering. All had to possess knowledge of differential and integral calculus and have completed one year of college physics. Potential trainees sent their applications to the participating universities, which then provided the appropriate military forms for officer programs.[285] By the end of 1942, there were still not enough university applicants. Becoming more aggressive in their search for potential training candidates, the UMC's recruiting board asked universities to provide the names and addresses of potential candidates so they could be contacted and asked to apply.[286] As of November 1942, the estimated manpower was 3000 men for the "A" courses starting September 1943 and March 1944.[287] The few women attendees were usually slated to backfill positions at the Weather Bureau, whose own ranks had been decimated as its forecasters were called to active duty.[288] The Navy brought women into the reserves (WAVES) for purposes of providing meteorological support, but unlike their male counterparts, most of them were required to have at least a master's degree in a scientific area to merit selection to the meteorology training program.

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[285] Charles F. Sarle, 15 January 1942 (Wexler papers, B1, Gen. Corr. 1942).
[286] Kaplan to Smith, 10 Nov 1942 (WU President, Accession 71-34, University of Washington Manuscripts, Special Collections, and University Archives,, B110, Met Training) [Hereafter **WU President**].
[287] Church to Sieg, 11 Nov 1942 (WU President, B110, Met Training).
[288] Philemon Church to Lee Paul Sieg, 11 November 1942 (WU President, B110, Met Training). Whitnah (p. 21) reports that the WB had 1494 full-time employees in 1938; 3218 in 1944 and 4727 in 1946. By late 1944, over 700 had left for active military service. According to "Women in the Weather Bureau During World War II," edited by Kaye O'Brien and Gary K. Grice (National Weather Service, ca. 2000) at http://www.lib.noaa.gov/edocs/women/html, there were only two women listed as either observer or forecaster in 1941. Office staffs were all men. By 1945, 900 women worked for the WB as either clerks or junior observers; most were temporary employees.

Even with this influx of men into the training pool, there still were not going to be enough forecasters to fill the need. By the fall of 1942, another group of potential recruits was being eyed: men attending junior colleges and those just graduating from high school who had strong mathematics and science skills. The junior college students would have been recruited through academic departments, while the high school graduates would most probably have been identified at either a local recruiting office or during basic training.[289] The former, needing at least one year each of physics and mathematics, were placed into "B" courses (Pre-meteorology): accelerated six month preparatory training courses to prepare them for the more advanced "A" course. The "B" course did not involve any meteorology – just prerequisite physics and calculus.[290] The high school graduates were placed in "C" courses (Basic Pre-meteorology): 12 months of all required mathematics and physics, plus writing and other humanities-type courses.[291] Those that did well advanced through the other courses. Some of those selected for the "B" and "C" courses were already military enlistees who passed written tests for selection.[292]

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[289] Rossby to Col. D. Z. Zimmerman, 28 September 1942 (University Meteorological Committee Collection, MC 511, Institute Archives and Special Collections, MIT Libraries, Cambridge, Massachusetts, Box 1) [Hereafter **UMC**].
[290] Kaplan to Sieg, 27 October 1942 (WU President, B110, Met Training).
[291] Rossby to Sieg, 3 November 1942 (WU President, B110, Met Training).
[292] There were eleven Pre-meteorological Centers: Brown University, MIT, NYU, State University of Iowa, University of California (Berkeley), University of Michigan, University of Minnesota, University of New Mexico, University of North Carolina, University of Washington, University of Wisconsin. There were twelve Basic Pre-meteorological Centers: Amherst College, Bowdoin College, Carleton College, Denison University, Hamilton College, Haverford College, Kenyon College, Pomona College, Reed College, University of Chicago, Vanderbilt University, University of Virginia.

A major problem facing course directors was a lack of qualified instructors. For example, the University of Washington's President Lee Paul Sieg (1879-1963) needed to find several mathematics and physics instructors in order to offer the "B" prep-course. They were just not available. As it was, UW was having a hard time covering the courses already on the books.[293] Rossby recognized and acknowledged that this was a problem. Even he was being forced to staff his instructor pool with recent graduates from his own program. As a result, there was a "notable lack of maturity" on Chicago's Institute of Meteorology's payroll.[294] Most of the meteorologists available were very inexperienced – having just completed their own graduate educations.[295]

When the United States entered the war in December 1941, the Army Air Force had 400 weather officers and 2000 enlisted weathermen. The Navy had 90 aerologists (weather officers) and 600 aerographer's mates (enlisted personnel). By early 1945, the AAF numbers were up to 4500 officers and 14,800 enlisteds, while the Navy had 1318 aerologists and 5000 aerographers.[296] In all, between 7,000 and 10,000 men and women were trained as professional meteorologists and another 20,000 as observers and meteorological technicians during World War II. (Over two thousand of the officer trainees were diverted to flight controller and navigator

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[293] Sieg to Rossby, 5 November 1942 (WU President, B110, Met Training). Ultimately, UW and other schools hired high school mathematics teachers to instruct lower level courses.
[294] Rossby to Sieg, 7 November 1942 (WU President, B110, Met Training).
[295] Rossby to Sieg, 10 November 1942 (WU President, B110, Met Training).
[296] Bates and Fuller, *Weather Warriors,* 57.

training and never served as weather officers.)[297] More people received meteorological training during fiscal year 1942 than in the previous ten years combined – and that was before the largest training classes met.[298] Even if most of these people returned to their original occupations, or switched into different ones, by the end of the war, there would still be a marked increase in the number of professional meteorologists. By one estimate, the number of professional meteorologists at war's end was approximately 20 times greater than before 1940.[299] They would come to make substantial changes in the field – changes required for the advent of numerical weather prediction.

THE UNIVERSITY METEOROLOGICAL COMMITTEE

In January 1941, the three weather services combined their resources to begin making domestic and military meteorological support plans in anticipation of a formal declaration of war. This Interdepartmental Committee on Meteorological Defense Plans would undergo two more name changes before becoming the Joint Meteorological Committee – the official advisory group to the Joint Chiefs of Staff on weather matters – in December 1941. The Committee was concerned not only with the training and employment of civilian and military meteorologists. It also worked on standardizing weather codes to promote more efficient data-sharing,

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[297] Rossby to Major General B. M. Giles, 10 September 1943 (Bowles papers, B30, F4).
[298] F. W. Reichelderfer, Chief, USWB, "Summary for Secretary's [Commerce] Report, Fiscal Year 1942," ca. summer 1942 [F. W. Reichelderfer papers, Library of Congress, Manuscript Division, B7, F10) [Hereafter **Reichelderfer papers**].
[299] Koelsch, "From Geo- to Physical Science," 531.

arranging research in long-range weather forecasting, developing background material in support of amphibious invasions, and developing a historical northern hemisphere weather map series to be used in weather typing.[300]

Rossby had convinced military planners early on that the training project should rest with university meteorology departments instead of being offered as in-house military and Weather Bureau courses.[301] Consequently, by the fall of 1942 virtually all meteorology community leaders outside of government service were involved in the training programs to the exclusion of most other work. Unfortunately, that meant that there was no apparent effort to use scientific knowledge of weather and climate in military planning and operations. In a telegram to Vice President Henry A. Wallace, Rossby – an acquaintance of Wallace from Rossby's tenure with the Weather Bureau while Wallace was Secretary of Agriculture – offered his assistance and that of his academic colleagues. Rossby wanted to help overcome what he saw as duplication of effort between the military services, and to develop some kind of cooperative, coordinated plan of attack for weather services to the nation. He noted that he had already offered his services directly to Colonel Donald Zimmerman, who was in charge of weather services for the Army Air Forces.[302] Within a couple of weeks, the War Department requested the formation of a standing committee to coordinate the recruitment and training of

[300] Bates and Fuller, *Weather Warriors,* 51. In weather typing, current weather conditions are matched to ones that have occurred in past years. Once a good match has been found, the subsequent maps are examined and used as forecasts for the next few days.
[301] Koelsch, "From Geo- to Physical Science," 530.
[302] Rossby to Wallace, 6 October 1942 (UMC, B4, Reichelderfer, F. W.).

meteorologists for the Army and Navy air forces as well as the Army ground forces.[303]

Called the University Meteorological Committee (UMC) and chaired by Rossby, its members were drawn from the "Big Five." The UCLA representative was upper air specialist Joseph Kaplan who, as the Personnel Director, was responsible for recruiting young men for meteorology training. Henry G. Houghton, a physical meteorologist, represented MIT. South African Athelstan F. Spilhaus, from NYU, had studied meteorology with Rossby at MIT, but was best known for his development of the bathythermograph for measuring the temperature of ocean water with depth. Caltech's representative, Paul E. Ruch, held a M.S. from Caltech and was an associate professor. He directed Caltech's meteorology program during World War II while Krick was in uniform providing forecasting services for the Army Air Force. The UMC was originally designed to provide guidance for recruiting and training issues, but it would go on to influence research agendas and the professionalization of the field.

By December 1942, the UMC was in full control of all meteorological training and had the full "confidence and cooperation" of the Army. The "Assistant Director of Weather" was the military head of the program, but Rossby was the dominant figure in the organization.[304]

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[303] Rossby to Sieg, 3 November 1942 (WU President, B110, Met Training).
[304] A. F. Carpenter, University of Washington representative at the Conference on Army-Sponsored Meteorological Training, University of Chicago, 18-19 December 1942 (WU President, B110, Met Training).

Even though meteorology training had been in progress for a couple of years, the rapid increase in student numbers dictated a more coordinated approach among all the schools providing instruction. Since all the schools needed to provide virtually the same curriculum, decisions had to be made about course prerequisites and content. A significant debate ensued during a meeting held in January 1943 – specifically about which mathematical approach should be taken while teaching physics disciplines. Some faculty members maintained that calculus and mechanics should be taught simultaneously. Others thought that students should study algebra/trigonometry-based physics followed by vector algebra, vector calculus and then mechanics using vector analysis. At issue was the mathematics skill level of entering students. While some had finished calculus, others had not. The question then became one of correlating the physics instruction with the correct mathematics level so as not to lose the students. Additionally, there were problems with physics preparation. Despite entering the program with more than the minimum requirements, many of the men were deficient in sophomore level physics. This lack of physics knowledge was slowing their progress through the courses.

Civilian faculty members were also concerned about their ability to prepare students adequately if they could not control the students' waking hours. Participants worried that if they did not set strict limits, military authorities might appropriate students' time for military matters. Therefore, a minimum of 49 hours per week had to remain under academic control. Although the "A" course was devoted to meteorology, the "B" course included mathematics, physics, mechanics, geography and English – both written and oral. The latter was included to prepare the men to make clear and concise radio transmissions between ground and aircraft in order to reduce the possibility of accidents due to misunderstandings. All schools were expected to follow the assigned curriculum. If time had to be made up, it would come from English and geography. Interestingly, any cuts from geography were to be taken from climatology first.[305] Although this might seem counterintuitive, physical geography would be much more important for these students than climatology. Topographic features significantly affect resultant weather and therefore students would be well served by realizing where tall mountains, deserts, valleys, etc., were in relation to militarily important sites and how they could impact their ability to make a forecast. On the other hand, climatology for an area could be looked up in tables and on graphs located at their assigned stations. If time were tight in the training program, it would be assumed that they could figure it out for themselves when they reached their new duty station.

The UMC was in place, but the "Weather Directorate" had been dissolved by April 1943 when Rossby expressed his concerns about the meteorological support services to communications engineering pioneer Dr. Edward L. Bowles (1898-1990), Special Assistant to the Secretary of War Henry L. Stimson. Rossby told Bowles that the demise of the Weather Directorate meant there was once again

[305] Report of the University of Washington representative at the conference of representatives of colleges participating in the Army-Sponsored Meteorology Training Programs, University of Chicago, 8-9 January 1943 (WU President, B110, Met Training).

no overall direction of military weather activities. Further, there were very few professional meteorologists in places of authority. This was not surprising since there were not that many professionals to start with. Meteorologists were assigned to the Weather Information Service, but it was not involved in training policies or the needs of military aviation. The Pacific campaign was being waged in tropical areas. Little was known about tropical meteorology and few training materials were available. They were desperately needed if men undergoing training were to be competent forecasters when they arrived on station. With no overall coordinator within the War Department, specialized meteorological areas – including tropical and oceanographic meteorology, i.e., weather over oceanic areas, – could become victims of in-fighting between special interest groups. Additionally, the continued presence of non-meteorologically trained personnel in the decision-making pipeline was delaying prompt action on new training ideas.

Rossby closed his discussion by pointing out that as far as he knew the United States was the only country where top academic meteorologists were being used to provide basic meteorological training while no one was being tapped for policy advice. The unfortunate result: ground forces were operating without adequate weather services due to the emphasis on aviation needs. Closer cooperation between Army and Navy weather services could overcome this problem.[306] Although Rossby did not point it out explicitly, there was another issue: scientists in other fields, most notably the physicists, were being used to

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306 Rossby to Edward Bowles, 18 April 1943 referenced in UMC Meeting Minutes of 4 June 43 (Wexler papers, B2, F1943).

significantly advance the war effort through their work on weapons and weapons countermeasures. They were being consulted by the highest levels of government. Other scientists had not been relegated to training large numbers of military men.

For Bowles, facing serious problems related to communications circuits handling weather data and information, Rossby's letter must have appeared as an answer to prayer. Less than two weeks after Rossby penned his letter, Bowles appointed him as an "expert consultant" to the office of the Secretary of War. Rossby's letter of appointment was followed up by a letter from Stimson to University of Chicago President Robert Maynard Hutchins. Stimson requested that Hutchins make Rossby available for this mission which was "vital to the war effort."[307] Bowles needed advice on how to balance the requirement for meteorological information against the ability of communications facilities to carry them in a timely manner.[308] As the war continued, Rossby's expertise would be tapped numerous times. He was asked for personnel recommendations, ideas on the best utilization of newly trained meteorologists, and the necessity for encrypted and coded transmission of weather data. Rossby also undertook inspection trips of the standard air routes to determine how best to support them. He coordinated meteorological support for ground forces, which had been left out with the focus on aviation missions, and investigated problems with instrument development and

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[307] Henry L. Stimson to Robert Maynard Hutchins, 1 May 1943 (Bowles papers, B30, F2).
[308] Bowles to Rossby, 30 April 1943 (Bowles papers, B32, F1).

procurement.[309] Thus, Rossby came to be tied in to the highest level of the military command structure. He had a tremendous influence over all aspects of the provision of meteorological services during the war. When the end of the war was in sight, he continued to use his connections to advance his personal agenda for both meteorological research and meteorology as a professional discipline.

Rossby attacked the "big picture" problems for the military services, but there was no shortage of smaller, practical problems that adversely impacted the university training units. Training centers were unable to obtain current weather data via teletype because real-time data could not be sent "in the clear." In order for the students to have access to the data, the universities would need to have a "secure drop," i.e., an encrypted communications link, guarded room, cryptographic equipment, and the correct clearances, because weather data were being handled as classified material. Therefore, students had to work with "canned," i.e., old, data. Old data had no intelligence use and therefore could be used without accompanying security considerations. There were valid concerns about using canned data. Meteorology students were generally more attentive to real data – plotting and analyzing data from several months before was not nearly as exciting as watching the current weather unfold on the chart while watching it unfold outside. Real-time data also allowed students to make the connection between what they saw on a map and what they saw outside. Yet, from the instructors' point of view, old data were easier to handle. Once the instructors

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[309] Rossby to Houghton, 25 May 1943 (Bowles papers, B40, F2); Bowles to Secretary of War, 23 August 1943 (Bowles papers, B30, F3).

analyzed a map themselves, they knew the "right" answer and could tell at a glance where the student had gone wrong. If new data were continually clicking in on the teletype, then the instructional staff would literally be just a step ahead of the students – not always the place a meteorology professor wanted to be when dealing with hundreds of students on a compressed schedule. Instructors had another reason for using canned data: case studies could be selected in advance which were instructive of different types of weather systems. The live data had ties to the real weather, but depending on the weather systems that passed by while the students were training, might not offer them the opportunity to see certain types of systems develop.

Additionally, the Army protested it was receiving the most immature graduates because the university programs were keeping their best students on as instructors instead of sending them out to field activities. In order to protect the reputation of the schools, Rossby recommended that some of their best graduates be sent to field units no matter how much they were needed in the training arena. Besides being needed for on-site forecasting at bases world-wide, the newly minted meteorologists were needed to provide weather training for pilot trainees, the Chemical Warfare Service, and other branches of the ground forces. Rossby estimated that the chemical warfare branch alone needed about 200 weather officers. Since no provisions for meteorological support had been made for the ground forces, their manpower needs were unknown. Unfortunately for the meteorology program, some of the new meteorologists were being siphoned off for pilot training and never served as meteorologists.

The specific needs of government units also created challenges for the meteorology training program. For instance, leaders of the Chemical Warfare Service had come to realize that they needed to determine the diffusion of smoke and fumes when either launching or receiving chemical warfare attacks. They had a two-fold requirement: assistance in interpreting research problems and help in the operations division using chemical warfare materials. However, the Chemical Warfare Service had no meteorologists on staff to provide advice. Rossby suggested that the UMC select men for weather training who already possessed degrees in chemistry and/or chemical engineering to fill this particular mission.[310] To meet the needs of its chemical warfare community, the Navy established the U.S. Navy Chemical Warfare Training Unit at the Dugway Proving Ground (Tooele, Utah) to provide micrometeorology training focused on the weather conditions within a few feet of the ground – the area where the impact of gas warfare would be the greatest. The Army Air Force took advantage of this instruction to train their chemical officers to understand the meteorological conditions necessary for successful offensive gas operations.[311]

Efforts were also being made to obtain needed information about weather conditions in critical operating regions. Funds had been secured to establish an Institute for Tropical Meteorology (under the joint control of the University of

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[310] UMC Meeting minutes, 3-6 June 1943 (Wexler papers, B2, F1943).
[311] Brophy and Fisher, *Organizing for War*, 356.

Chicago and the University of Puerto Rico) in Puerto Rico which would address some of the deficiencies in that sub-discipline. Additionally, several senior meteorologists were to be sent to Newfoundland, Greenland, Iceland, Labrador, Alaska, and India to obtain more realistic information on weather conditions for the students.[312]

While the UMC's primary mission – training – had some continuing problems, these remained under control. Students flowed smoothly through the courses and received operational weather station experience before being sent to their first activity. Yet, the UMC also had a research mission – and it was definitely in the applied category.

A joint meeting of Army, Navy and Weather Bureau representatives advising the Joint Chiefs of Staff established meteorological research priorities in early 1943. The weather service representatives assigned the highest priority to developing upper-level forecasting charts (in support of aviation interests), and developing techniques for five-day and longer range forecasts in areas of strategic interest. Next in importance was the extension of forecasts over ocean areas by making use of observations from isolated stations (in support of both afloat and aviation missions). This was followed by exploiting the possibility of weather typing, i.e., creating map series that could be matched with current conditions and then extrapolating the forecast for the surface and upper levels.[313]

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[312] UMC Meeting minutes, 3-6 June 1943 (Wexler papers, B2, F1943).
[313] Research Projects prepared for JMC Research Committee Conference (Army, Navy and Weather Bureau cooperating), 23 January 1943 (UMC, B4, Reichelderfer, F. W.).

The research programs being carried out by each of the "Big Five" U.S. meteorology departments were thus directly connected to the war effort. These efforts usually fit into one or more of several categories, including analysis/atlas projects, climatology, tropical meteorology, upper air charts (in support of dynamic meteorology), and long-range forecasting. *Analysis/atlas* projects involved re-analyzing weather maps after including all available data (without the time constraints of operational meteorology). Meteorologists could then use the resulting collections of weather charts to study atmospheric patterns and the weather that resulted. *Climatology* projects involved compiling many years of observational data from sites that were important to military operations (for instance, Greenland, Iceland, Europe), and determining long-term averages for temperature, precipitation, pressure, and other weather elements. This information would then be used by planners to determine, for example, the best locations for landing strips and the best (or worst) times for launching certain kinds of military operations.

Tropical meteorology studies were important because a considerable amount of the Pacific Theater was in the tropics for which there was very little meteorological knowledge. Efforts involved gathering as many observations as possible and analyzing the resulting patterns to determine forecasting rules. The last category – *upper air observations and their study* – was important because it was impossible to determine the dynamic structure of the atmosphere by just looking at the surface data. With the inclusion of large amounts of upper air data , for the first time meteorologists could better study the general circulation of the atmosphere. They could also use the resulting knowledge to better predict flight conditions and to make attempts at longer range forecasts, i.e., over several days instead of just one or two.

In short, these war-time research efforts were focused on optimizing military operations by incorporating the latest meteorological techniques – including new instrument development – and knowledge in geographical areas which had been outside routine military operating areas before the war. Methods of providing long-range forecasts, including an analysis of ozone content and its relation to general atmospheric circulation patterns, were especially important to military planners.[314] The results of applied meteorology research conducted during the war served to advance theoretical studies in general circulation after the war. Long-range forecasting after the war was enhanced by this preliminary work driven by military requirements.

In late 1943, with war's end still over eighteen months away, the members of the UMC were considering its future. With sufficient manpower trained, its role in meteorological training was coming to a close. Therefore, the time had come to turn attention elsewhere and create a more permanent entity. However, having been responsible for the professional training of the majority of active meteorologists who would be practicing the science in the postwar years, committee members wanted to ensure these new additions to the community were appropriately

[314] Thomas B. Marshall to Colonel Thompson, 26 Aug 1943 (UMC, B7, Research).
[316] UMC Meeting Minutes, p. 19, 6 December 1943 (UMC, B2, Blue Folder).

employed. Rossby was convinced that the UMC should have an important role in promoting and developing the meteorological sciences.[316]

One possibility proposed by Rossby entailed the UMC becoming the meteorology section of the National Defense Research Committee (NDRC). Created in 1940 in response to the national emergency and focused on weapons research, the NDRC was subsumed under the Office of Scientific Research and Development (OSRD) in 1941. The latter was created to mobilize scientific personnel for the war effort.[317] If such a meteorological section were established, it would be responsible for sponsoring meteorological research and have the funds to do so on a large scale. Rossby had approached Bowles with this idea, and according to Rossby, Bowles strongly opposed this move. Further, Rossby was concerned that Bowles thought the Army had enough funds to support "*all legitimate* meteorological research" (emphasis mine).[318] Apparently any research worth doing would be of direct benefit to the Army [Air Force] and other research was not worth doing.

Rossby also suggested, based on information he had received from a number of sources, the possibility that the UMC could become the basis of a new professional society which would either supplant or augment the American

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[317] Dupree, *Science in the Federal Government*, 369-371. The NDRC is also discussed in Alex Roland, "Science and War; Stewart, *Organizing Scientific Research for War*; Baxter, *Scientists Against Time*; A. Hunter Dupree, "The Great Instauration of 1940: The Organization of Scientific Ideas for War," in *The Twentieth Century Sciences: Studies in the Biography of Ideas*, ed. Gerald Holton (New York: Norton, 1972), 443-467; and Carroll Pursell, "Science Agencies in World War II: The OSRD and Its Challengers," in *The Sciences in the American Context: New Perspectives*, ed. Nathan Reingold (Washington, D.C.: Smithsonian Institution Press, 1979), 359-378.
[318] UMC Meeting Minutes, p. 24, 6 December 1943 (UMC, B2, Blue Folder).

Meteorological Society. As currently configured, the UMC only spoke for the universities. The Weather Bureau could only represent (civilian sector) government meteorology. And the AMS could not speak for meteorology as a whole because of the way it was organized – presumably in this case since the AMS mixed amateurs with professional members.[319]

Some very important questions were at issue for Rossby and other leading members of the UMC. With the creation of a large cadre of highly-educated meteorologists, who would be considered as a "meteorologist" in the postwar years? Who, or what, would control the meteorological research agenda? Who were the possible patrons? How might they influence the conduct of research? How would the burgeoning private sector meteorologists be controlled? What entity would be responsible for protecting the professional standing of the scientific community by licensing practitioners? How could meteorology be sold as a technical profession? All of these questions conflate to one primary issue: the professionalization of meteorology.

PROFESSIONALIZATION OF THE METEOROLOGICAL COMMUNITY

In the United States, meteorology had always been unique among scientific disciplines because the vast majority of its practitioners were employed not in universities or industrial settings, but by the government. Those employed by the government were almost exclusively attached to the Weather Bureau. Although

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[319] Jörgen Holmboe (UCLA), UMC Meeting Minutes, p. 25, 6 December 1943 (UMC, B2, Blue Folder).

there were other scientific agencies, e.g., the Coast and Geodetic Survey, the U.S. Geological Survey, the Bureau of Mines, the Naval Observatory, none of these organizations employed the majority of the scientists in their disciplines. However, with the atmosphere extending beyond the boundaries of any state or region, and data collection and processing being beyond the capability of a business concern, the Weather Bureau by national necessity had a stranglehold on meteorological practice in the United States. Thus, before World War II, meteorologists in the United States were divided into two very distinct camps: the academic theoreticians, who were generally found within university physics or geography departments, and the forecasters, who generally worked for the Weather Bureau. While the former had advanced degrees from colleges in the United States or Europe, the latter had received most of their training on-the-job. These two groups intermingled very little. The theoreticians considered themselves to be practicing a science and thought the forecasters were pursuing an art only peripherally related to science. For the most part, academics were not involved with making forecasts at all.

Despite these differences, anyone who was interested in the study of weather – for academic or practical purposes or just out of personal desire – was eligible to be a member of the American Meteorological Society. No distinction was made between those who were theoretical, applied, or amateur meteorologists. Indeed, the Society was about evenly split between amateurs and those who were either theoretical or applied meteorologists. With membership in 1940 at a little more than 1400, thousands of potential new members with formal meteorology training stood ready to change this mix.[320]

The academics composing the UMC considered themselves and their colleagues to be professionals. As Horace Byers of the University of Chicago's Institute of Meteorology put it, "[This] Committee may consider itself as perhaps more deeply engaged in some of the better aspects of meteorology...Certainly meteorology in this country outside of this esoteric bunch is a small proposition."[321] Besides the academics, they acknowledged that there was just a handful of professionals scattered within the ranks of the Weather Bureau – at most perhaps 200 to 300 (or less than 10% of the total). Byers estimated that only 25-30% of those holding professional grades at the Weather Bureau had any kind of college degree. Indeed, the Weather Bureau preferred to train its own forecasters. Prospective forecasters needed to have completed two years of college with mathematics and physics and have passed a Weather Bureau placement test. Alternatively, they could have one year of college and an outstanding record at the Bureau – probably as a sub-professional plotter or observer. Thus by the academics' definition, the majority were not "professionals."[322] In contrast, everyone they had trained during the war was a professional by virtue of their course work no matter the extent of their practical experience. Uncomfortable with

[320] "Secretary's Report," *BAMS* 22 (1940): 34.
[321] UMC Meeting Minutes, pp. 26-27, 6 December 1943 (UMC, B2, Blue Folder).
[322] Ibid., 33, 43, 44. Summary of the Forecaster's Conference, 15-20 May 1944, issued 1 August 1944 (National Archives and Record Administration II, College Park, Maryland, Weather Bureau papers, RG 27, Entry 11, B2, Multiple Address Letters) [Hereafter **Weather Bureau papers**].

requiring new meteorologists to meet a higher standard of professionalism than current practitioners once the war was over, they pondered who would have the authority to make that kind of decision.

In 1943, the UMC spoke on behalf of academic meteorologists. The Weather Bureau spoke on behalf of government meteorologists – both civilian and military. For everyone else, there was the American Meteorological Society.

The AMS, founded in 1919, was open to anyone who paid $3.50 in annual dues.[323] The UMC members anticipated that with the end of the war, the combined effects of a rapid increase in commercial aviation and the meteorology training centers established by the UMC would result in a tremendous growth of interest in meteorology. Those people intrigued by meteorology would want an organization that would support their amateur interests. The AMS would be a good place for them – the "National Geographic Society of meteorology," as Rossby put it.[324]

The professionals, no matter for whom they worked, would need their own separate organization. The UMC members wanted a professional meteorological society (much like the American Society of Mechanical Engineers) that would guarantee standards: standards for entering the profession, and standards for remaining within it. By expanding their group to include other academics and those with advanced degrees in meteorology, they could create such a professional society that would be responsible for setting educational standards, accrediting university curricula, and licensing private consultants. It had to be a strong society

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[323] UMC Meeting Minutes, p. 26-27, 6 December 1943 (UMC, B2, Blue Folder).
[324] Ibid., 40, 58.

whose words carried weight so as to "do away with the embarrassment already existing" in the profession – an embarrassment stemming from the perception of both the scientific community and populace at large that meteorology was not a scientific endeavor. Unfortunately, such a society would exclude a large number of current practitioners. The idea of setting up a competing group struck Byers as "snobbery."[325]

The problem, as Rossby saw it, was one of rampant professional opportunism. He was convinced that when the war ended and the men returned home, meteorology would "blossom out as a field of consulting meteorological engineers." Without adequate professional standards, Rossby worried, a "lot of people" who did not possess minimal professional educations would set themselves up as consulting meteorologists.[326] A licensing venue had to be established to prevent that from happening. The Weather Bureau was a possibility. However, the UMC members did not view the Bureau as being capable of maintaining professional standards within its own ranks, much less a wider spectrum of meteorologists. Furthermore, Rossby did not think that Reichelderfer wanted to step into the licensing void.[327] If the UMC members set up an alternative professional organization, how could they license meteorologists and not extend those same licenses to Weather Bureau members who were already serving in professional positions? How could they say "no" to private sector meteorologists

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[325] Ibid., 26-27.
[326] Ibid., 26-27.
[327] Ibid., 46-47.

and not say the same thing to Weather Bureau personnel? After all, there were already many people working as "meteorologists" in private industry who only possessed high school educations.[328] It appeared that setting up a separate professional society was the only method of controlling the potential problems of non-professionals providing meteorological services to an unsuspecting public.

But there was a problem. Rossby had been nominated as the next AMS president. As AMS president, he could hardly agitate for a new professional society. That meant that AMS leaders would need to modernize the existing AMS structure in order to turn it into a more professional society.[329] By January 1944, Rossby concluded that there was no need for another organization whose only purpose would be to safeguard professional standards, ethics and privileges – much like a trade union. The AMS Council met four times on this issue between the end of January and the middle of July 1944. Its members decided to promote the public acceptance of meteorology as a technical profession by establishing standards, issuing a new technical journal, bringing meteorology to the attention of industry, and providing a placement service for the employment of meteorologists. There would also be a new category of professional members who would be "actively engaged in professional phases of meteorology who see their obligation to the science and who are therefore willing to support measures that will apparently best meet these responsibilities and insure to the general benefit."[330]

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[328] Ibid., 48.
[329] Ibid., 25.
[330] C. F. Brooks to Wexler, 8 August 1944 (Wexler, B2, F1944).

Thus by the end of 1944, Rossby joined Henry G. Houghton in leading the AMS to become a more professional scientific society. As part of that process, the new *Journal of Meteorology* was introduced as the technical counterpart to the *Bulletin of the American Meteorological Society.*[331]

Another issue in the professionalization of the field concerned curricular issues. The UMC members were apprehensive over what they feared would be "wild growth" in meteorological institutions before they could get their professional organization started. However, it would be difficult to "meddle" in the business of professional schools of meteorology if they could not decide what constituted an acceptable course of instruction. Rossby proposed that representatives from the Weather Bureau, Army, Navy, and UMC draft a statement outlining minimum content and staffing requirements for a legitimate meteorology course of instruction. Once completed, the UMC would mail such a statement to all college presidents, "many of whom are now thinking of establishing a professional course in meteorology."[332] These expectations were clearly unrealistic. Certainly there were universities planning to establish meteorology departments at the end of the war – the University of Washington and the University of New Mexico among them. The former already had an oceanography department and wanted to expand into meteorology as a related area. Indeed, UW faculty member Philemon Church, working with Rossby in Chicago, had advised President Paul Seig that the

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[331] Warren M. Washington, "Foreword," in Fleming, *Historical Essays in Meteorology,* vi. However, the Society did not establish its Certified Consulting Meteorologist Program until 1957.
[332] UMC Meeting Minutes, p. 70, 6 December 1943 (UMC, B2, Blue Folder).

university needed to make its move into meteorology if it wanted to secure the possibility of expanding into the field after the war. That was the primary reason Seig actively pursued offering the "B" course during the war years.[333] Even if Rossby and the UMC were anticipating unfettered growth of meteorology curricula, at least the University of Washington realized that the market to support such programs was limited.

METEOROLOGY AT WAR'S END

By mid-1945, World War II had made a radical impact on the meteorology community in the United States. The military exploitation of aviation assets to deliver armaments, and to transport personnel and material had given an unprecedented stimulus to the science of meteorology. To ensure the safety of aviators and their aircraft from the vagaries of the weather, meteorologists had sought and obtained the establishment of world-wide reporting and forecasting stations. This observational network would prove crucial to the development of meteorology. Those sectors of meteorology directly related to the war effort – tropical, high-latitude, oceanographic, and high-altitude meteorology – had received a real boost due to government financed research. And, of course, large numbers of newly trained meteorologists had entered the field.[334]

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[333] Church to Sieg, 11 November 1942 (WU President, B110, Met Training).
[334] Carl-Gustav Rossby, "A Message to Members from President Rossby," *BAMS* 25 (1944): 268-269.

For the United States, this was an especially profound change. Before the war, Germany and Scandinavia had considered meteorology and other geophysical sciences to be more important than had the United States.[335] Indeed, most research advances in meteorology had come at the hands of the Scandinavians working under the inspiration of Vilhelm Bjerknes and his colleagues – including Rossby – the so-called Bergen School. Although some ground had been gained in the pre-war years in the United States, the years of the Great Depression were not a good time to advance new academic fields.[336] Advances that did come were very much influenced by the Scandinavians – at first just Rossby; later Jacob Bjerknes, Sverre Petterssen, Jörgen Holmboe, Bernhard Haurwitz – all Scandinavians and Germans caught in the United States when the war broke out. Their leadership was crucial not only in the training of thousands of military men and women, but also in attracting those who might have gone into the physical sciences or engineering had the war not changed their plans.

As a consequence, the professional meteorological community not only grew from a total of 400 persons before the war to 6000 afterwards, but those new members tended to come from physics and mathematics backgrounds that led them

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[335] See Gregory A. Good, "The Rockefeller Foundation, the Leipzig Geophysical Institute, and National Socialism in the 1930s," *Historical Studies in the Physical and Biological Sciences* 21 (1991): 299-316; Ronald E. Doel, "The Earth Sciences and Geophysics," in *Science in the 20th Century,* ed. John Krige and Dominique Pestre (Amsterdam: Harwood Academic Publishers, 1997), 361-388.

[336] Horace R. Byers, J. Kaplan, and E. J. Minser, "The Teaching of Meteorology in Colleges and Universities; Recommendations of the Committee on Meteorological Education of the American Meteorological Society," *BAMS* 27 (1946): 95. See also Dupree, *Science in the Federal Government,* Chapter 18; Friedman, *Appropriating the Weather,* for a discussion of the Bergen School during the interwar years.

to take a very much different approach to the science of meteorology.[337] These were men who depended on physical laws and mathematical manipulation to define the state of the atmosphere, rather than men with a sense of the atmosphere based on some kind of gut instinct. They were looking for rigor. If they did not find it, they expected to create it.

Equally important was the new perception of meteorology in both the scientific community at large and the general public. There was no mistaking the importance of meteorology to the war effort. Military operations – airborne, amphibious, ground, and afloat – all depended on accurate weather forecasts. Airborne operations not only needed weather forecasts for safety of flight, the pilots and their crews needed to know in advance if they could count on clouds for cover, or if those same clouds would prevent them from finding their target. Likewise, amphibious operations depended on accurate wave and surf forecasts for safe beach landings and effective operations. One particularly well-known instance of the importance of weather forecasting was during the planning of Operation Overlord – the invasion of Normandy on 6 June 1944 which became known as D-Day.[338] The conditions under which military operations were carried out were brought to the general public through newsreels, radio and newspaper reports. No longer as quick to make snide comments about the local "weather guesser," citizens

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[337] Ibid.

[338] The following books all contain discussions of the importance of weather forecasting for military operations. For discussions of the D-Day forecast, see Bates and Fuller, *Weather Warriors*, 88-95; Fuller, *Thor's Legions*, 85-101; and Sverre Petterssen, *Weathering The Storm*, 191-255.

became more accepting of the discipline as a scientific profession and the meteorologist as a "reliable professional man."[339]

Community leaders realized that they had to capitalize on this new-found respect for their science and do so quickly. Under Rossby's leadership, the AMS acted swiftly to take its place beside other engineering and scientific professional societies. To make sure that meteorology did not lose war-time gains and that it continued to advance, the AMS sought broad exposure of the discipline's possibilities. The value of meteorology to aviation and agriculture was already well known. Rossby wanted the wide spectrum of industries to recognize that meteorology could make their businesses more profitable. To make this happen, the AMS took two important steps. First, it became a "potent" factor in soliciting funds and fellowships for research. In this way, the science could – as Sverre Petterssen put it – be "lifted out of a state of neglect and place[d] on a level of prominence amongst the other physical sciences."[340] Second, it established strict ethical standards to prevent the exorbitant claims by those who might wish to profit at the expense of an uninformed public, thus leaving the meteorological community open to criticism, which could destroy the very gains made towards credibility during the war years.[341] In particular, the Society was worried about private sector meteorologists who sold very long-range forecasts based on doubtful scientific

[339] Rossby, "A Message to Members from President Rossby," 268-269.
[340] Sverre Petterssen quoted in Thomas F. Malone, "The Atmospheric Sciences and the American Meteorological Society – The More Recent Past," *BAMS* 51 (1970): 218.
[341] Kenneth C. Spengler, "From the Executive Secretary," *BAMS* 27 (1946): 255.

reasoning to agricultural, utility, and other industrial interests who made business decisions based on these forecasts.

"There has been little except war and the needs of the general public to promote advancement of [meteorology]," Army Air Force Brigadier General D. N. Yates, Chief of the Air Weather Service later noted. "There has been practically no incentive to individuals for entrance into the field of meteorology on a career basis."[342] As unfortunate as the war had been, it had opened the door for huge advances in meteorology. Horace Byers may have summed it up best: "It is an unfortunate characteristic of meteorology that its great forward strides depend on disasters. Catastrophes and wars result in increased meteorological financing and activity and World War II was an outstanding example of growth bred from disaster."[343]

World War II had expanded the atmosphere for the meteorology community – more money for research which extended knowledge into new areas, newly designed equipment, more observing stations, and more professional scientists. This critical mass of well-trained, ambitious, and forward-looking men entered the post-war era – a time when virtually everything seemed scientifically possible. They were ready to take meteorology from a small, marginalized, sometimes scorned, backwater field, to a scientific discipline of importance both within the sciences themselves and within the realm of public opinion. Within a few months

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[342] BGEN D. N. Yates quoted in "Remarks Made During the Washington Meeting Discussion on Problems of Industrial and Commercial Applications of Meteorology, "*BAMS* 28 (1947): 410.
[343] Horace R. Byers, "Recollections of the War Years," *BAMS* 51 (1970): 217.

of VJ Day the technology that meteorologists needed to provide the scientific break of the century arrived on their doorstep: the digital computer.

CHAPTER 5
INITIAL ATMOSPHERIC CONDITIONS: SCIENTIFIC GOALS, CIVILIAN MANPOWER AND MILITARY FUNDING (1944-1948)

By 1944, the United States had turned the corner towards victory. The meteorologists who had been training thousands of men to support the military mission were faced with empty classrooms. For them, the end of the war was in sight. Many meteorologists were more than ready to abandon the applied meteorological questions they had pursued in the support of the nation's defense for more theoretical pursuits. Their interests and concerns were not just limited to research topics. They extended to research *funding*. Government funding had dominated the war years. It remained to be seen if the free-flowing funding of the war years would continue.

Other scientific communities faced the same kinds of questions. Physics and engineering disciplines in particular had benefited from the needs of the war effort, stimulating rapid, ground-breaking advances in radar, electronics, proximity fuses, and nuclear power. Applied physics needs had also stimulated development in another significant area. The need for fire control solutions, i.e., the information used to aim very large guns and rockets, had encouraged the first steps toward the creation of primitive electronic computers.[344]

[344] See, for example, Williams, *A History of Computing Technology*; Ritchie, *The Computer Pioneers*; Campbell-Kelly and Aspray, *Computer: A History of the Information Machine*; and Ceruzzi, *A History of Modern Computing*.

One of those involved in developing the modern computer era was the distinguished Hungarian-born mathematics prodigy John von Neumann (1903-1957) of the Institute for Advanced Study in Princeton. Von Neumann, who had developed the theory of games in the 1920s before immigrating to the United States, had worked on the Manhattan Project, among other efforts, during the war.[345] He sought to pursue his goal of a digital electronic computer once the war was over. This new "stored-program" computer would allow for significantly faster solutions of complex mathematical problems – in particular those that had non-linear solutions solvable only by numerical analysis. Instead of taking days, weeks or months of work by human "computers," these new "electronic brains" could produce a solution in hours or days. Thus, investigators could rapidly revise formulas, change variables, alter input, and re-compute as many times as necessary to reach the desired solution.

Despite the dark cloud cast by the atomic bomb and the subsequent concerns about the dangers of radioactivity, the prevailing view was that the sciences had had a positive influence on the outcome of the war.[346] This reinforced the tremendous faith of Americans, already evident in the Progressive Era, in the ability of science to solve all problems – natural or man-made. Therefore, it seemed

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[345] There have been many articles and a number of books written about John von Neumann and his many accomplishments both within and outside the government. See, for example, Aspray, *John von Neumann*; Norman Macrae, *John von Neumann* (New York: Pantheon Books, 1992); William Poundstone, *Prisoner's Dilemma* (New York: Doubleday, 1992). For a short biographical sketch, see J. Dieudonné, "John von Neumann," in Charles Coulston Gillispie, editor in chief, *Dictionary of Scientific Biography* (New York: Scribner, 1990), 14: 88-92.

[346] See Spencer Weart, *Nuclear Fear: A History of Images* (Cambridge, MA: Harvard University Press, 1988).

very reasonable for people to be able to control nature and their environment.[347] Perhaps nothing quite so constitutes an environment to be controlled as much as does the weather. If people could outwit the weather – prevent droughts and floods, enhance rain and snow when needed, disperse fog, reduce hail damage, dissipate or change the paths of hurricanes, and prevent tornadoes from forming – that would be a huge achievement. Never before had such a possibility seemed so much within reach.[348]

But before people could control the weather, they would need to understand how it worked, and be able to consistently forecast it with tremendous accuracy. In 1945, theories which could define the general circulation of the atmosphere were extraordinarily weak. Forecasting techniques remained, as they had been for decades, primitive at best. The steps taken to aid weather forecasting during the war, such as expanding the data network and adding many more upper air observations, could now be exploited for theoretical work.

The return to peacetime helped fuel a dramatic expansion in many physical science fields. Meteorologists from all parts of the community – academic, Weather Bureau, and military – found themselves freed to tackle long-term projects of importance to the overall advancement of the atmospheric sciences. The "Big Five" meteorology departments moved ahead with their research agendas. Weather

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[347] See, for example, Hirt, *A Conspiracy of Optimism*; Worster, *Rivers of Empire*; Scott, *Seeing Like a State*; White, *The Organic Machine*.

[348] Weather control has been a longstanding dream of mankind since the earliest days of appealing to the gods for relief of undesirable weather. The few historical treatments of early weather modification efforts in the United States include Spence, *The Rainmakers* and Jeff Townsend's *Making Rain in America: A History* (Lubbock, Texas: ICASALS Publications, 1975).

Bureau Chief Francis Reichelderfer looked forward to taking back some of the traditionally civilian roles usurped by the military. Military agencies looked at ways of influencing scientific development. The immediate post-war period would see each group maneuvering to solidify its position in a strengthened professional community. The emergence of the electronic digital computer would prove a vital ingredient to the meteorologists' advancement of their discipline. However, the forward-looking efforts of Reichelderfer, Rossby, and the military meteorologists moved numerical weather prediction forward.[349]

THE POST-WAR RESEARCH AGENDA FOR METEOROLOGY

Theoretical meteorologists such as NYU's Hans Panofsky, Chicago's Horace Byers, and MIT's Henry Houghton, were all eager to get back to their own research projects, put on hold since the war had begun. At the same time, they were concerned about who, or what, might influence or control the post-war research agenda. During wartime, the research agenda had been heavily influenced by military requirements. The academic meteorologists were now faced with the possibility that post-war research would be controlled by the government as well: not as overtly, perhaps, as during the war, but certainly as a result of making funding available through contracts.

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[349] This story, as told in Chapter 2 of Aspray's *John von Neumann* and Chapter 10 of Nebeker's *Calculating the Weather,* was almost completely about von Neumann. The archival record shows that meteorologists physically located a considerable distance from Princeton, in particular Rossby and Reichelderfer, were critical to the success of "von Neumann's" Meteorology Project.

Although generous funding for basic research in the post-war era would indeed become available – first through the Office of Naval Research, and later through the National Science Foundation – this was obviously not known in early 1944. The worried academics of the UMC were quite frankly panicked by the thought of continued government control of their research projects. Prior to the war, a large percentage of meteorological, and other scientific, research had been funded by private sources. Leading a discussion on potential research funding during a University Meteorological Committee Executive Meeting, Chicago's Byers argued that funding from private sources appeared to be on the wane, with government funding taking its place.[350] If government agencies were providing the funding, they would in turn dictate the problems to be solved, present them to universities, award contracts, and expect results. He did not anticipate that funding would be awarded for general research. The path would be laid out for a specific result and the contract awarded to the school best equipped to provide that result.[351] That scenario was problematic for these meteorologists who were looking forward to the opportunity to conduct fundamental research – they needed the freedom to explore and follow where their research took them. In general, these academics did

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[350] See Wang, "Liberals"; Kohler, *Partners in Science*; Forman, "Behind Quantum Electronics"; Sapolsky, *Science and the Navy*; Owens, "Science in the United States"; Dennis, "Historiography of Science: An American Perspective"; Lowen, *Creating the Cold War University*.
[351] Byers, 29 February 1944, UMC Executive Meeting Minutes, p. iii (UMC, Box 1, UMC Meeting at USWB).

not view decision-making personnel in the military weather services or the Weather Bureau as being cognizant of how that research was done.[352]

Although some government agencies had allowed considerable latitude in how contracts were handled – in particular, the National Advisory Committee for Aeronautics (NACA) – the Army had not. [353] UCLA's Jörgen Holmboe protested that what the Weather Bureau really wanted were improvements, not basic research. Therefore, it was in their best interest to help the Bureau and not worry about getting tied down in long research projects for them. But Byers argued that while it was one thing to help the Weather Bureau and the Army with their projects, keeping academic departments fully occupied with contracted research would greatly reduce research freedom – "the life blood of any university."[354] Houghton was blunter still. Taking on government contracts, he growled, was "selling out."[355]

Despite the prevailing evidence that private funding was a thing of the past, the UMC meteorologists were not sure that government "subsidies" would be a sure thing after the war.[356] Caltech's Beno Gutenberg flatly rejected the idea that there would be government funding. He argued that once the war was over, private

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[352] Houghton, Loc. Cit., iv.
[353] H. J. Stewart, Loc. Cit, iv.
[354] Byers, Loc. Cit., iv.
[355] Houghton, Loc. Cit., iv.
[356] After 1945, scientific disciplines responded to the availability of government funding in different ways. Physics, for example, expected to receive significant government funds, while astronomers still depended on private patrons. See Doel, *Solar System Astronomy in America*, 44-77; David DeVorkin, "Organizing for Space Research: The V-2 Rocket Panel," *HSPS* 18 (1987): 1-24; Toby Appel, *Shaping Biology: The National Science Foundation and American Biological Research, 1945-1975* (Baltimore: Johns Hopkins University Press, 2000).

foundations would resume their roles as patrons of basic scientific research. The government would fund research by its own people in its own labs. Gutenberg was not even convinced that funding would be available at all. He thought there was a good possibility that once the war ended, Congress would divert funds earmarked for scientific research to other needs. As UCLA's Joseph Kaplan argued, if they could get the private funding scenario in place well in advance of the war winding down, there would be less interference in what research was being done. With research freedom preserved, they could make more progress. But Byers and others remained unconvinced. The shift to government funding had preceded the war and that pattern could continue.[357]

The Weather Bureau's research budget had always been small. During the war, what there was quickly switched to military control. As in most situations involving money, once an organization has gained control of funding at the expense of another, it is very difficult to return the situation to its previous status. Indeed, in the mid-1940s, the Weather Bureau had problems in need of solutions, but no funds to pursue them. The military services also had problems that needed solutions, but they had plenty of funds. To avoid being cut out of the picture entirely, Weather Bureau Chief Francis Reichelderfer recognized that he would need to place himself in a position to influence the allocation of funds.

A "top-ten list" of the "most useful research to bring results in the shortest amount of time" provides a tantalizing piece of evidence that the Weather Bureau

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[357] Gutenberg, Kaplan, Byers, 29 February 1944, UMC Executive Meeting Minutes, p. v (UMC, Box 1, UMC Meeting at USWB).

was trying to prioritize, and perhaps, influence the post-war research agenda. The list, a result of a 1944 survey of Weather Bureau staffers, military, airline, and university meteorologists, is interesting both for what it includes and what it does not. Seven of the ten items were related in some way to forecasting (development of forecasting rules, studies of orographic influences, studies of factors controlling movement of high and low pressure areas). Two of them dealt with getting better upper air data from radiosondes. And the last one called for "descriptive" studies of convergence, divergence, vertical motion and vorticity, i.e., physical processes in the atmosphere.[358]

This period was ripe for theoretical developments and research in meteorology. Both surface and upper air observation density had increased as a result of the war. There were more scientifically trained personnel possessing advanced technical capabilities. And yet, not one theoretical topic appeared in the "top ten." Granted, one might not expect theoretical projects to "bring results in the shortest period of time." This seems to be yet another indicator of the divide that existed between the theoretical and practical sides of the meteorological house. The advancement of the science depended on developing a mathematical and physical theory of atmospheric circulation. It did not appear on the list. Certainly, despite comments to the contrary as meteorologists looked back to reminisce on this period, there were no projects that indicated an interest in using a numerical

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[358] "The Weather Bureau Questionnaire on Research Needs," *BAMS* 25 (1944): 434. First published in *Weather Bureau Topics and Personnel* (Washington, D.C., August 1944): 318. Orography is the branch of physical geography which deals with mountains.

approach to solving the non-linear equations defining atmospheric movement. Knowledge of the difficulty in solving such equations was probably one of the reasons. But as the war came to a close, the means for their solution was waiting in the wings.

HIGH SPEED COMPUTING MEETS METEOROLOGY

Numerical weather prediction was dependent on the availability of a high speed computer. The path to the creation of such a machine began in Philadelphia during the war. Electrical engineers John W. Mauchly (1907-1980) and J. Presper Eckert (1919-1995) of the University of Pennsylvania's Moore School started working on the ENIAC (Electronic Numerical Integrator and Computer) in June 1943 under contract to Army Ordnance. Its purpose was to compute firing tables much more quickly than was possible with calculating machines.[359] As work was being wrapped up on ENIAC prior to delivery to the Army's Aberdeen Proving Ground, Mauchly visited the Weather Bureau's Washington headquarters in April 1945 to ascertain the possible meteorological uses of high speed sorting and computing devices. His initial visit with the Assistant Director for Scientific Research,

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[359] See Aspray, *John von Neumann*, chapter 2, for a short discussion of ENIAC. For a longer treatment of ENIAC, other Eckert-Mauchly computers, and the roles of Eckert and Mauchly in computing, see Nancy B. Stern, *From ENIAC to UNIVAC: An Appraisal of the Eckert-Mauchly Computers* (Bedford, MA: Digital Press, 1981). Scott McCartney's *ENIAC: The Triumphs and Tragedies of the World's First Computer* (New York: Walker and Company, 1999) gives a more popular account of its development and the personalities that played a role in it. Contemporary descriptions include H. H. Goldstine and A. Goldstine, "The Electronic Numerical Integrator and Computer (ENIAC), *Mathematical Tables and other Aids to Computation* 2 (1946): 97-110; and E. C. Berkeley, *Giant Brains, or Machines that Think* (New York: John Wiley and Sons, 1949), Chapter 7.

statistician C. F. Sarle, yielded something fairly routine: the sorting of data punched onto IBM cards for climatological studies. (The Weather Bureau was routinely behind in computing climatological data, due to lack of personnel.) Sarle also expressed interest in weather map extrapolation, i.e., the creation of a new map by shifting weather features in the direction of the general atmospheric flow. Such an extrapolation method would do the same thing that human forecasters were already doing to create a forecast map – moving frontal features several hundred miles downstream depending on how fast the steering level winds were blowing. As such, it did not incorporate the use of physical laws in anticipating atmospheric motion. Mauchly was not sure his new machine, the EDVAC, could extrapolate weather maps, but did point out that it would be able to solve partial differential equations, i.e., the type describing atmospheric motion. Sarle was not interested. The Weather Bureau – burdened by increasing demands and a shrinking manpower base – was primarily interested in controlling some of its vast mounds of data by automation.

A much different response greeted Mauchly at the Air Weather Service. There he met former, and returning, Weather Bureau meteorologist Major Harry Wexler. Recall that Wexler had been hired from Rossby's MIT program after the Science Advisory Board urged the Weather Bureau to adopt the Bergen School methods. Wexler was one of three new Ph.D.s familiar with air-mass analysis hired to spread the technique throughout the Bureau. Thus, he was not only interested in practical forecasting – he was interested in meteorological theory. Wexler enthusiastically recognized the importance of applying such a machine to the integration of the hydrodynamic equations. He directed Mauchly to other weather officers who were working on a variety of meteorological problems of interest to the Army Air Force in the days just preceding the fall of Germany. They, too, were convinced the machine could have a very important use in weather forecasting.[360]

The difference between the perceived uses of the computing machine by WB and AAF personnel in spring 1945 is striking. Sarle – bogged down with data waiting to be analyzed for climatological studies and very few people to do it – saw only pedestrian uses. In stark contrast, Wexler and his mathematically savvy AAF colleagues immediately sensed an application to the forecasting problem – *the* major problem for both the military and the WB. Upon Wexler's return to the Weather Bureau at the end of 1945, he would vigorously pursue this new technology.

Mauchly was not a lone wolf trying to convince the Weather Bureau of the possible uses of the computer for meteorological purposes. Russian-born physicist Vladimir K. Zworykin (1889-1982) of the Princeton RCA Laboratory, also envisioned meteorological applications. The inventor of the electronic-scanning television camera, Zworykin was involved with the development of meteorological instruments at RCA and had become enamored of meteorological problems

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[360] John W. Mauchly, "Note on Possible Meteorological Use of High Speed Sorting and Computing Devices," 14 April 1945, copied by F. W. Reichelderfer on 24 January 1946 and marked "Confidential." (Wexler papers, B2, F1945). What Reichelderfer might have thought about Mauchly's ideas in spring 1945 is unknown. Based on Mauchly's notes, he did not visit with Reichelderfer.

including, perhaps, the ultimate meteorological problem: weather control. During an evening which included a liberal supply of vodka, he explained to an astonished Wexler that hurricanes could be prevented from forming by putting oil on the ocean surface under cumulonimbus build-ups and setting it on fire. Zworykin thought this would "bleed" the energy out of the system and prevent hurricanes from having sufficient energy to form.[361]

Reichelderfer first heard of Zworykin's proposal for the use of "modern electronic devices" in meteorological analysis during a September 1945 visit to the RCA Lab. Much interested, he requested a copy of Zworykin's forthcoming written proposal.[362] E.U. Condon (1902-1974), the ambitious physicist and then Director of the National Bureau of Standards, was also extremely intrigued. Having already obtained copies of the Zworykin proposal, he forwarded copies to Reichelderfer and suggested that they cooperate on work with electronic computers. Reichelderfer observed at the time that although Zworykin's proposal was unproven, it "should not be taken lightly."[363] In early December, Reichelderfer pursued the possibility of using electronic computers in meteorological analysis and extended forecasting by inviting Zworykin to Weather Bureau headquarters to discuss the issue in more depth.[364] As that letter was leaving the Weather Bureau,

[361] H. Wexler to Reichelderfer, 18 October 1946 (Wexler papers, B2, F1946). Weather Bureau personnel actually considered this idea, albeit very briefly, before rejecting it.
[362] Reichelderfer to Zworykin, 4 December 1945 (Wexler papers, B2, F1945).
[363] Reichelderfer notation on letter from E. U. Condon to Reichelderfer, 26 November 1945 (Wexler papers, B2, F1946).
[364] Reichelderfer to Zworykin, 4 December 1945 (Wexler papers, B 2, F1945).

Reichelderfer was in contact with Condon who suggested that they also invite von Neumann.[365]

Originally planned for the end of December, the conference was finally set up for 9 January 1946. Attendees would include a small number of WB staffers and military meteorologists in addition to Bureau of Standards representatives. Reichelderfer noted that Zworykin's ideas constituted a "startling, but noteworthy proposal."[366] In his invitation to von Neumann, Reichelderfer stated that the purpose of the conference was to discuss "the ways and means for improving the techniques of weather analysis and forecasting…"[367]

While the meteorologists were expressing tentative interest in this proposed computer's application to forecasting problems, the Navy's Office of Research and Invention (ORI, later the Office of Naval Research) was expressing interest in funding von Neumann's machine. During their meeting with IAS Director Frank Aydelotte, the Navy's Chief of Naval Research, Admiral Harold G. Bowen, and the head of the ORI, Captain Luis de Florez, were "very enthusiastic" about the computing machine. Their "purely scientific" interest came with a commitment to make a substantial "no-strings attached" contribution to the effort.[368] Whatever de Florez had in mind when expressing enthusiasm for this plan, the Navy's claim of

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[365] Reichelderfer to Sarle, 4 December 1945 (Wexler papers, B2, F 1946).
[366] Reichelderfer to Weather Bureau staff, 29 December 1945 (Wexler papers, B2, F1945). Emphasis in the original.
[367] Reichelderfer to von Neumann, 29 December 1945 (Wexler papers, B2, F1946).
[368] Frank Aydelotte to von Neumann, 9 November 1945 (John von Neumann papers, Library of Congress, Manuscript Division, B12, F1) [Hereafter **von Neumann papers**]. Aydelotte was the Director, Institute for Advanced Study.

"purely scientific" interest is questionable. Several years later, de Florez publicly proclaimed himself a strong supporter of weather control and undoubtedly saw computer development as a step towards making that happen.[369]

A well-placed Navy "leak" to *The New York Times* blew the possibility of a weather-predicting computer out into the open within two days of the Weather Bureau-brokered meeting. Sources were quoted as saying that there had been discussion of a new super calculator that would not only be able to predict the weather, but would make it possible to "do something about the weather" by using "counter-measures" against unfavorable conditions. Navy meteorologists thought sufficient theory existed, but the complicated calculations could not be solved quickly. The new computer would eliminate that problem. The *Times* reported that some scientists thought that the threat of tornadoes, hurricanes, and other severe weather could be reduced with advance knowledge. For example, atomic energy (i.e., nuclear weapons) might be used to divert hurricanes away from populated areas.[370] The Weather Bureau was interested in analysis and forecasting applications. The Navy, which heretofore was only involved in funding the computer, seemed to emphasize the weather control aspects of Zworykin's proposal in its off-the-record comments.

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[369] In a speech given in New York in January 1961, then Rear Admiral de Florez called for a $100 million/year program of meteorological research with the ultimate aim of weather modification and control. (Wexler papers, B35, Weather Modification – 4).

[370] Sidney Shallet, "Weather Forecasting by Calculator Run by Electronics is Predicted," *The New York Times*, 11 January 1946: 12.

The January 9 conference participants, Reichelderfer in particular, had thought the conference was "confidential" and were very unhappy with the *Times* coverage.[371] The War Department's Ordnance Research Office actually thought the newspaper content violated military security.[372] Zworykin could not understand why the Navy released the information without consulting anyone.[373]

The "why" of the Navy leak almost certainly was related to mustering support for developing a meteorological application for von Neumann's computer among the Navy's top leaders. Navy meteorologists, like their Weather Bureau and Air Force counterparts, realized that the computer had the potential to do two things for them: speed up the availability of predictive charts and increase their accuracy. This new tool would allow on-site forecasters to spend more time on actual weather prediction. For the military war fighters, weather was only an issue when it got in the way. When it was not a problem, no one gave it a second thought. To assure continued support from the war-fighting interests, the meteorologists would need something more appealing than a faster forecast. Weather control, with its possible application as a weapon, was clearly very appealing. Thus, the leak indicated that a comprehensive meteorological theory existed (when it most certainly did not) and emphasized the weather control aspects. In order to sell a project that could forecast, or control, the weather, the meteorologists needed to have a plausible theory to back it up.

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[371] Reichelderfer to V. Zworykin, 11 January 1946 (Wexler papers, B2, F1946).
[372] Record of telephone conversation between Reichelderfer and Col. Gillen, 21 January 1946 (Wexler papers, B2, F1946).
[373] Zworykin to Reichelderfer, 14 January 1946 (Wexler papers, B2, F1946).

Though ruffled and embarrassed by this unanticipated public relations fiasco, Reichelderfer continued to pursue the possibilities that electronic computing might offer. Wexler visited Zworykin and von Neumann in Princeton to discuss potentially computer-solvable meteorological problems. Having no meteorological background, von Neumann needed advice on the mathematical and physical requirements that had to be considered.[374] So did Mauchly and Eckert when Wexler sounded them out on the feasibility of designing an ENIAC-type machine to forecast the weather. They were convinced it could be done "once specifications were laid down by meteorologists."[375] This was going to be difficult. Neither "electronic engineers nor the meteorologists" were able to answer the question, "How can the electronic computer be applied to meteorology?"[376] Establishing the specifications would be impossible without first establishing the extent of the meteorological questions. Establishing those questions would be a problem. The Air Weather Staff wanted to know if Wexler had any ideas, other than a reconstruction of the Richardson method, which they could think about and discuss. Lewis Fry Richardson's (1881-1953) World War I-era attempt at numerical weather prediction was to solve the so-called "primitive equations" of the

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[374] H. Wexler and Jerome Namias to Reichelderfer, 8 February 1946 (Wexler papers, B2, F1946). Namias, who had worked under Rossby at MIT on Bankhead-Jones Act-funded long-range weather forecasting techniques, continued that work throughout his career at the Weather Bureau. A childhood friend of Wexler's, he was also his brother-in-law. For a short biographical sketch of Namias, see John O. Roads, "Jerome Namias," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1998), 76: 242-267. Also see Hessam Taba, "Jerome Namias," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1988), 379-391. AAF meteorologist and mathematician Gilbert Hunt also attended this meeting.
[375] H. Wexler and Namias to Reichelderfer, 26 February 1946 (Wexler papers, B2, F1946).
[376] Reichelderfer to the Secretary of Commerce (Henry A. Wallace), 18 February 1946 (Wexler papers, B2, F1946). The Weather Bureau had formerly been under the Department of Agriculture.

atmosphere by making one 6-hour time step, i.e., the change in time in the equations of motion was 6 hours, and doing all the calculations by hand. He published his results, a huge failure, in his book *Weather Prediction by Numerical Process* in 1922.[377] It had attracted very little, if any, attention (other than by book reviewers) at the time, but in 1946 was briefly considered as the first point of departure. For his part, von Neumann expressed his intent to examine the fundamental theories of meteorology – a necessity if the computer were to be able to solve atmospheric problems. Further he expected to spend about 25 percent of his time on the meteorology part of the project – a figure that Chicago's Rossby thought would more realistically amount to five percent given von Neumann's other obligations.[378]

After meeting with Wexler and others in early February in Princeton, von Neumann turned to Rossby for advice. Von Neumann informed Rossby that he was "considerably attracted" by the problem of the general circulation of the atmosphere, and proposed that it first be attempted in its most "simplified and

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[377] Gilbert Hunt to H. Wexler, 18 March 1946 (Wexler papers, B2, F1946). Richardson, *Weather Prediction by Numerical Process*. For a short discussion of Richardson and his numerical modeling attempt see Brian Hayes, "The Weatherman," *American Scientist* 89 (2001): 10-14. For a look back at Richardson's method, see Sydney Chapman, "Introduction" to *Weather Prediction by Numerical Process*, by Lewis Fry Richardson (New York: Dover Publications, 1965). For a discussion of Richardson's eclectic interests – a Quaker, he also studied conflicts – see Oliver M. Ashford, *Prophet – or Professor?: The Life and Work of Lewis Fry Richardson* (Bristol and Boston: A. Hilger, 1985).

[378] Notes of H. Wexler dated 12 April 1946 (Wexler papers, B2, F1946). Von Neumann suggested that a meteorology department be established at Princeton, but that never happened. Whether it would have helped the project could be debated. With many academic meteorologists skeptical of the usefulness of the computer, it may have been a hindrance. Since there were not enough academic meteorologists to go around anyway, it is not clear where they might have found the personnel to staff it. The dearth of practicing meteorologists in the post-war years, despite the thousands trained during the war, is an interesting topic that needs to be explored.

schematic form." He wanted to consider a homogeneous, rotating earth which included some corrections for the amount of solar radiation received by latitude and assumed zonal symmetry, i.e., that physical data were independent of longitude. Did Rossby think that an approach using partial differential equations to describe the general atmospheric circulation would be reasonable? Because it was unlikely that he could get to Chicago in the near future, von Neumann asked Rossby to come to the IAS so they could discuss it in more detail.[379] Rossby accepted.

Rossby's discussions with von Neumann at Princeton furthered the pursuit of the meteorology-computer connection. He then reported the result of the meeting to the Weather Bureau's Reichelderfer. Rossby recognized that von Neumann was interested in hydrodynamic problems and their solutions, i.e., basically researching the general circulation of the atmosphere, but that he was uninterested in the kind of empirical time and space lag correlations which could have an immediate practical impact on weather forecasting. Although not aiding forecasting in the near term, the development of working models would allow for changes of input variables like solar radiation to evaluate their effect. Rossby thought meteorology would be better served by letting a team work on the general circulation problem first – a theoretical issue. Solving the equations of motion as they related to weather prediction – an *applied* issue – could come later.

A master at recognizing fruitful opportunities, Rossby shrewdly viewed von Neumann's new-found interest in theoretical meteorology as a potentially huge

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[379] Von Neumann to Rossby, 6 February 1946 (von Neumann papers, B15, F7).

asset to meteorological progress and wanted to "stimulate him further" by surrounding him with a "small and versatile" group of theoretical meteorologists which would serve to provide the foundation for this new computational approach. At a minimum, as Rossby wrote to Reichelderfer, they needed to find "some highly competent young man" to help von Neumann. However, everyone was already engaged in his own work.

Instead, Rossby proposed the forming of a team of meteorologists that could assist in the project. His proposed list included German-born Walter M. Elsasser (1904-1991), an atmospheric radiative processes specialist with the Princeton RCA labs; Chaim L. Pekeris (a former Rossby student) working on radiation and hydrodynamics; AAF Captain Gilbert Hunt, trained as a meteorologist during the war and then a Ph.D. student in mathematics at Princeton; and someone familiar with large-scale turbulence problems – perhaps Raymond B. Montgomery (1910-1988) of Woods Hole Oceanographic Institution (WHOI) or Hans Panofsky of New York University.[380] Others who could potentially make substantial contributions were Bernhard Haurwitz of MIT, Victor Starr from the University of Chicago (another Rossby protégé), or Morris Neiburger of UCLA. However, bringing in any of these people would be "robbing Peter to pay Paul" because they were already engaged in other research projects. As Rossby ruminated on this list, he feared the models this group top-heavy with mathematicians would choose to attack. A synoptic meteorologist skilled in both descriptive and

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[380] For a biographical sketch of Elsasser, see Harry Rubin, "Walter M. Elsasser," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1996), 68: 103-166.

theoretical approaches had to be added to this mix if the group were to be effective. (Finding people with a sense of the atmosphere as well as a mathematical bent emerges as a critical issue in the long-term modeling process.) To bring this about, Rossby recommended that the IAS reach an agreement with a governmental agency, e.g., the Navy's Office of Research and Invention (ORI), which could supply sufficient funds to allow von Neumann to assemble this proposed group to attack the problem.[381]

Since von Neumann was not concerned with efforts which would aid forecasting in the near term, Rossby also recommended that a second group of people be assembled from the ranks of the "mathematically, statistically, and synoptically competent and ingenious" from the Weather Bureau, Air Weather Service and Naval Weather Service for the purpose of examining how the ENIAC could be used to compute time lag correlations which would aid day-to-day forecasting.[382] And so, Rossby, theoretician, researcher, and entrepreneur, capitalized on the interest of both Mauchly and von Neumann by proposing projects that would attack theoretical and forecasting problems at the same time.

Reichelderfer strongly backed the proposed project, even though well aware there was no guarantee of useful results. While preferring that the Weather Bureau should be the governmental organization to take leadership, he was realistic enough to acknowledge that financial constraints might require interdepartmental cooperation. However, he advised Rossby that he would be putting together a

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[381] Rossby to Reichelderfer, 16 April 1946 (von Neumann Papers, B15, F7).
[382] Ibid.

program plan with the enthusiastic Wexler in the near future and hoped that Rossby would continue to be able to provide advice.[383]

Rossby saw the Princeton project as a way to advance theoretical meteorology and very much wanted it to move forward. Within a week of the letter to Reichelderfer, he negotiated a tentative contract proposal and funding arrangements with the ORI's staff meteorologist, Lieutenant Commander Daniel F. Rex, and then provided von Neumann with a draft proposal. [384] The proposed research objective was to examine ideas concerning the general circulation of the atmosphere so as to determine its steady-state characteristics and subsequent response to externally applied influences. If sufficient support was forthcoming, the project might even be able to "throw light on the nature of climatic fluctuations." Leaving nothing to chance, Rossby continued with a complete budget description which included numbers and types of people, salaries, travel, and overhead expenses. Noting a lack of suitable candidates for the project, he recommended that the Weather Bureau's Wexler be brought in to manage the project. In addition to those he had named in his earlier letter to Reichelderfer, he added the name of Paul Queney, Director of the "Institut du Globe" at the University of Algiers and another Rossby protégé at Chicago. The proposed project starting date: 1 July 1946.[385]

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[383] Reichelderfer to Rossby, 24 April 1946 (Wexler papers, B2, F1946).
[384] Rossby to LCDR D. F. Rex, 23 April 1946 (Wexler papers, B2, F1946). Rex, a Navy Aerologist (as they were then called), later earned his Ph.D. in meteorology under Rossby in Stockholm. In the late 1940s, Rossby would return to Sweden to establish a Department of Meteorology at the University of Stockholm.
[385] Rossby to von Neumann, 23 April 1946 (Wexler papers, B2, F1946).

On 8 May 1946, IAS Director Frank Aydelotte, signed out a contract proposal to the Navy's ORI. However, the research objective had now become "[an] investigation of the theory of dynamic meteorology in order to make it accessible to...computing." Specifically, the project proposed to investigate:

- the mechanism and flow pattern of the general atmospheric circulation;
- the necessity of considering stratospheric as well as tropospheric contributions;
- the stability of the polar front and other fronts in general;
- the mechanism and flow pattern of major cyclones including their formation, progress and stability; and
- the release mechanisms of local instabilities.

However, it did not stop with atmospheric theory or forecasting. It continued by claiming that with this computing project they would take the "first steps towards influencing the weather by rational, human intervention...since the effects of any hypothetical intervention will have become calculable."[386] This theme would be continued in the Justification Memorandum, which stated that the research program would enable the goals of rapidly predicting both short- and long-range weather conditions as well as *controlling* the weather.[387]

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[386] Frank Aydelotte to LCDR D. F. Rex, 8 May 1946 (von Neumann papers, B15, F6). A definition of terms is in order here. The stratosphere (the second "layer" in the atmosphere) extends from the top of the troposphere (approximately 10-17 km above the earth's surface) to the bottom of the mesosphere (approximately 50 km above the earth's surface). The troposphere occupies the space between the earth's surface and the bottom of the stratosphere. The "polar front" is the semi-permanent front separating tropical and polar origin air masses. "Cyclones" are large-scale regions of low pressure which turn in a counter-clockwise rotation in the northern hemisphere (clockwise in the southern hemisphere) – in meteorological usage they are not tornadoes or similar small-scale circulations. A system is "stable" if small disturbances have only small effects; it is "unstable" if a small disturbance generates a large effect.

[387] Justification Memorandum, PD #EN1-22/00028, The Institute for Advanced Study, 6 June 1946 (von Neumann papers, B15, F6). The idea of "control" over the weather is a recurrent one throughout the project, but it generally comes from those who are not meteorologists: Zworykin and

However, with the machine not yet built (its anticipated completion was two to three years away), the project would need to focus on meteorological *theory*. Indeed, meteorological theory had not yet reached the point where the problem was ready for this new computational approach. Without this new computer which could perform calculations at a rate 1000 to 100,000 times faster than had been possible, there had been no motivation to address the relevant theoretical considerations.

The proposal indicated that if enough meteorological personnel could be assembled by the fall of 1946, they would spend six months to complete a preliminary analysis of the basic dynamical meteorological problems (those listed above). Once the preliminary results had been reviewed by meteorologists outside the Project, Project members would then determine the most promising direction to take in their solution. This part of the project would extend towards the end of 1947 and then the computations could be worked out in parallel with the machine development over 1948. By 1949 both the machine and the required theoretical work would be complete and model testing and subsequent modifications would be underway.

von Neumann. During this same period Nobel Laureate Irving Langmuir, his assistant Vincent Schaefer, and Bernard Vonnegut, actively worked on weather control through seeding. None of these men were meteorologists. The meteorologists were struggling just to understand the atmosphere and get a prediction out for the next day – control of the general circulation was not an issue for them even though work on "smudging" orchards to keep fruit from freezing was. The theme of control over nature and its manifestation in weather modification for agriculture, military or diplomatic reasons recurs throughout the Cold War and needs a close examination.

Desired personnel for this project would be "first-class *younger* meteorologists" (emphasis mine) led by Wexler. The "younger" meteorologists would be required because it was this group that had the mathematics and physics background to advance the work. Although "older" theoretical meteorologists also had these attributes, they were already committed to other academic pursuits. As a complement to this group of young meteorologists, the project would assemble a "prominent group of advisors and consultants" including meteorologists and oceanographers such as Rossby, Norwegian Harald Sverdrup (1888-1957) and Jacob Bjerknes; physicists with radiation and molecular physics expertise such as Hungarian-born Edward Teller (b. 1908) and Indian-born Subrahmanyan Chandrasekhar (1910-1995), an aerodynamicist like Hungarian-born Theodore von Kármán (1881-1963), and other experts from a variety of technical fields to number a total of eight to ten.[388] With the funding to cover the personnel costs for the meteorological group, the total proposal came to $61,000 per annum.[389] Following negotiations carried out by Aydelotte, von Neumann, and Rex, the Justification Memorandum was signed on June 6, 1946.

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[388] For a short biographical sketch of Sverdrup, see William A. Nierenberg, "Harald Ulrick Sverdrup," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1996), 69: 339-374. Theodore von Kármán (with Lee Edson), *The Wind and Beyond; Theodore von Kármán, Pioneer in Aviation and Pathfinder in Space* (Boston: Little, Brown, 1967) is Kármán's self-congratulatory view of his life. Teller is a senior research fellow at the Hoover Institution. For his autobiography, see Edward Teller, *Memoirs: A Twentieth-Century Journey in Science and Politics* (Cambridge, Massachusetts: Perseus Pub., 2001). Two books providing insight into Teller's role in physics and the development of atomic weapons are Stanley A. Blumberg and Louis G. Panos, *Edward Teller: A Giant of the Golden Age of Physics: A Biography* (New York: Scribner's, 1990) and Gregg Herken, *Brotherhood of the Bomb: The Tangled Lives and Loyalties of Robert Oppenheimer, Ernest Lawrence, and Edward Teller* (New York: Henry Holt and Co., 2002).
[389] Frank Aydelotte to LCDR D. F. Rex, 8 May 1946 (von Neumann papers, B15, F6).

The goals of the project supporters were different right from the beginning. The Weather Bureau's Reichelderfer – who had sparked the Project – was interested in forecasting. Military meteorologists, even those theoretically trained, also had practical forecasting goals. Rossby and the academics were firmly in the theoretical camp, although the more intuitive Rossby was not against applied research. Zworykin and von Neumann wanted a meteorological theory amenable to an attack by computer, an advance that would ultimately allow for weather control. And the funding source, the now Office of Naval Research (ONR), seemed content to support basic research with the hope of a practical result in the future.

CALLS FOR MANPOWER AND ADVICE

All major research projects, boiled down to their essence, contain three necessary ingredients: funding, equipment, and manpower. The IAS Meteorology Project was no exception. The funding was assured by the Navy; the equipment was being designed and built. Manpower, however, was problematic. Meteorologists tended to come in two varieties: theoretical and applied. Theoreticians had mathematics and physics backgrounds, tended to think in equations, and viewed the atmosphere as something "out there," not something that affected daily life. Applied scientists might not be mathematics- and physics-savvy in a technical way, but they had a sense of the *physical factors* that influenced the weather. This project needed people who could handle both parts of the problem.

Attracting meteorologists to Princeton was difficult. The more experienced academic meteorologists, i.e., those whose professional careers started before the war, were primarily theorists. Happily settled on their campuses, they were not only committed to other projects, but extremely skeptical that the entire computer scheme would work.[390] There were also pressures to remain in their current positions due to an overall shortage of theoretical meteorologists.[391] Others were concerned about the length of time it would take to develop von Neumann's new computer.[392]

Rossby applied his considerable charm to persuade his hand-picked candidates – and sometimes their bosses – that the Meteorology Project was the perfect place for them to use their many talents.[393] Von Neumann obtained commitments from Paul Queney (University of Algiers), Albert Cahn (University of Chicago), and Chaim Pekeris (Columbia University). Wexler would lead the project.[394] The team combination was a telling one: everyone involved was part of a younger, more mathematically-grounded subset of the meteorological community, they were all theorists, and they all had ties to Rossby. Although having its advantages – openness to new ideas being one – their lack of a physical sense of the atmosphere would later prove a handicap. With their theoretical bent, it

[390] Raymond P. Montgomery to H. Wexler, 14 May 1946 (Wexler papers, B2, F1946).
[391] Haurwitz to H. Wexler, 21 June 1946 (Wexler papers, B2, F1946).
[392] H. Wexler to Hunt, 3 May 1946 (Wexler papers, B2, F1946).
[393] In his efforts to get Paul Queney on the team, Rossby asked that the University of Algiers extend the leave of absence that had allowed Queney to spend time at the University of Chicago. Rossby to Pierre Auger, Directeur de l'Enseignement Superieur, Ministère de l'Education National, Paris, 11 May 1946 (von Neumann papers, B15, F7).
[394] Von Neumann to Queney, 18 May 1946; von Neumann to Haurwitz, 6 June 1946; von Neumann to H.Wexler, 14 June 1946 and 29 June 1946 (all from the Wexler papers, B2, F1946).

was clear that this project was only going to pursue theoretical development, not the applications desired by the military and Weather Bureau meteorologists.

While others sought people, Reichelderfer sought advice. In letters sent to a number of prominent meteorologists and oceanographers, he described the Meteorology Project as one that would "advance our science materially." He asked for their feedback. The responses were almost universally skeptical of the possible success of the undertaking, based on a realistic assessment of the state of meteorological theory as it existed then in the spring of 1946. Respondents wondered how the computer could positively influence meteorological problems when there was little understanding of the principles underlying atmospheric behavior.[395] They had little knowledge of the governing equations of the atmosphere. What about the "little known terms?" Shouldn't those be determined first?[396] What were the roles of friction and heat sources and sinks? Wouldn't they need to be determined before developing the equations? Perhaps the computer could play a limited role in solving some dynamical meteorology problems, but nothing more. There would be no solution to the forecasting problem in the near future. And weather control? They all agreed *that* was an absolute pipe dream.[397]

At least one person had a positive opinion on the proposed computing project: Caltech meteorologist Robert D. Elliott. Elliott had spent some time considering numerical methods while reworking Richardson's World War I era

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[395] Sverdrup to Reichelderfer, 2 June 1946 (Wexler papers, B2, F1946).
[396] Hans Panofsky to H. Wexler, 22 July 1946 (Wexler papers, B2, F1946).
[397] Haurwitz to Reichelderfer, 14 May 1946 (Wexler papers, B2, F1946).

attempt to forecast by numerical means. At first glance, he thought Zworykin's ideas were "overly optimistic." Upon further reflection, he thought that perhaps these were possibilities overlooked by forecasters desperate to get forecasts out. The increased data density available since the war would make a direct attack on the problem feasible.[398]

Ideas were flowing in, and at least a few meteorologists were agreeing to join the project. Von Neumann invited a number of meteorologists to a conference to discuss the project in late August 1946. Wexler, in charge of the agenda, set aside the first two days to discuss scientific questions and the last day to deal with organizational issues.[399]

Von Neumann opened the August conference with a discussion of the electronic computer's capabilities. He was followed by Rex, who discussed the Navy's interests (since it was providing the funding). Rossby then led the discussion of meteorological research. He was followed by meteorologists discussing research issues from their own specialties: Haurwitz on dynamics, Willett on synoptics, Namias on (long-range) weather forecasting, and Cahn on the Richardson-Elliott approach to numerical forecasting.[400]

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[398] Robert D. Elliott to Reichelderfer, 4 June 1946 (Wexler papers, B2, F1946).
[399] Von Neumann to Jule Charney, 14 August 1946 (Jule Gregory Charney Papers. MC 184. Institute Archives and Special Collections, MIT Libraries, Cambridge, Massachusetts, B16, F516) [Hereafter **Charney papers**].
[400] *Dynamic meteorology* is the study of atmospheric motions as solutions of the equations of hydrodynamics. *Synoptic meteorology* is the study and analysis of surface weather observations made at periodic times (usually in three- or six-hourly intervals as dictated by the World Meteorological Organization). Examples of observed elements are: temperature, wind velocity, atmospheric pressure, and sky cover.

As the conference progressed, problems were laid out and assignments made to those who would head up the respective efforts. The first of these were "type problems," i.e., problems that need not be directly applicable to meteorology, but because of their prototype characteristics could be used to test existing or planned numerical techniques and computers. Pekeris suggested that they address stability questions associated with turbulence, one of which was the Heisenberg-Lin equation of stability: a one-variable, linear, total differential equation proper-value problem which could be surveyed for various combinations of parameter values. With progress on the linear version, non-linear extensions could be added. The meeting participants decided that Pekeris would start with the Heisenberg-Lin equation when he joined the project in early November.

Similar to the Heisenberg-Lin problem were meteorological stability problems. These problems were linearized stability questions superimposed on typical meteorological flow conditions (unlike the Heisenberg-Lin problem, which is superimposed on the Poiseuille flows, i.e., the laminar flow of a fluid through a circular tube.) Depending on the circumstances, these problems could be either simpler or more difficult that the Heisenberg-Lin. Haurwitz and Panofsky had some ideas on their solution and were assigned to work on them.

The conferees discussed general and specific circulation issues throughout the conference. They decided to put their efforts toward determining the significance of the stationary and zonally-symmetric atmospheric circulation, i.e., what circumstances lead to blocking situations during which time the flow becomes meridional with large north-south excursions of air and those which lead to air which flows generally parallel to lines of latitude. Panofsky temporarily received this assignment. Hurricane theory fell to Haurwitz, who had already given thought to the problem and possessed adequate empirical material to make an initial attack.

Meteorologist turned mathematician Gilbert Hunt proposed the analysis of basic meteorological parameters, e.g., velocity and pressure distributions in a large volume of air. He believed this would help to work out some problems of turbulent flow, in particular, eddy viscosity, represented by complex mathematics. All the attendees realized the importance of these very difficult problems which would require an extensive amount of data and probably use the entire capacity of the planned computer. The attendees agreed that Hunt should tackle the turbulence problem.

The meteorologists also discussed the continuation of Elliott's efforts to renew Richardson's attempts to directly integrate the equations of motion. Since the computing machines available to the group were considerably more advanced than those available to either Richardson or to Elliott, they agreed that the direct numerical attack should be repeated immediately. They were not sure whether continued efforts would be made to eliminate the flow velocities from the equations since that approach seemed to lead to analytical difficulties and questionable approximations.

At a final evening meeting of the working group, members discussed assignments and the role that each would play. Montgomery would serve as a part-time consultant and give his attention to the numerical forecast by direct integration. Elsasser, who was fully occupied at the RCA laboratories, would be available on a consulting basis. Cahn's task would be to undertake the Richardson calculations by formulating the dynamic equations and setting them up for a numerical approximation method. He would then familiarize himself with the ENIAC, and possibly with Harvard's computer, and then supervise the actual integration. Panofsky and Haurwitz would assist with the equations, von Neumann with the numerical approximation, and von Neumann and mathematician Herman H. Goldstine, the assistant director of the Computer Project at IAS, with the computing machines themselves. Hunt, an Army officer who had resigned his commission, was scheduled to remain in Princeton until his discharge on 1 November (then he would make a decision on whether to stay with the Meteorology Group). In the meantime, he would assist Cahn and also work on the general circulation problem. Panofsky, who worked at New York University, wanted to remain involved with NYU and with the Princeton group. Haurwitz was at MIT, which had a meteorology faculty shortage. Therefore, he was unable to commit full-time to the group until February of 1947. His intent was to concentrate on hurricane issues and to consider instability problems. Wexler was to continue at the Weather Bureau until late 1946 and then move to Princeton as soon as housing became available. He planned to split his time between Washington, D.C. and Princeton where he would supervise the working group. In the meantime, he would make frequent visits to Princeton to confer with von Neumann. Von Neumann would spend the last half of September at Los Alamos, but after October 1, 1946 would remain in Princeton where the project would be concentrated. Those associated with the project decided to hold periodic meetings to discuss problems and reassess the work.[401] For all the time and effort that went into this meeting, very little would come of it. The members were physically separated and occupied with other projects. What could have been a jump-start for the Meteorology Project, would turn out to be, unfortunately, a false-start.

Reporting to Reichelderfer, Wexler shared his conviction that the "abrupt discontinuity in speed" represented by the new computing machine would make a substantial difference in the discoveries of theoretical meteorology by reducing calculation times. Despite its theoretical path, Reichelderfer remained steadfast in his conviction that the Project was of the greatest importance and must be kept moving forward.[402]

Thus by the fall of 1946, the Meteorology Project had established its priorities and arranged for team members. Reichelderfer and Wexler, having thrown their complete support behind it, were clearly enthusiastic. But the theoretical bent of the Project was a problem. The Weather Bureau and the military weather services were not likely to have much use for meteorological theory. What they needed was a way to get out better forecasts faster, using less subjective techniques.

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[401] Minutes, Conference on Meteorology, August 29-30, 1946 (Charney Papers, B4, F134).
[402] H. Wexler to Reichelderfer, 12 September 1946 (Wexler papers, B2, F1946).

THE METEOROLOGY PROJECT FORMS UP

The original proposal had called for a fairly large group of collaborators to work on the Meteorology Project. A drastic shortage of housing in the Princeton area quickly derailed this plan, preventing several investigators from joining the group and subsequently becoming unavailable or less available than they had been. The combination of housing problems and a lack of investigators to supervise led Wexler to remain in Washington, D.C. and periodically commute to Princeton to check on progress.

Visiting in mid-October, Wexler found Queney, Hunt and Cahn already on-site. Von Neumann was very pleased with the progress made on the general circulation model and the setting up of the Richardson equations. He – rather optimistically, as it turned out – thought they would be able to put the equations on the underutilized ENIAC in the near future. However, the group was being hampered by a lack of office and living space, and as a consequence, of personnel. Wexler thought the group needed delicate handling at first to make sure it got off on the right foot. He was providing both advice and meteorological information to these theoreticians so that they would have evidence of actual atmospheric behavior. Included in these data were historical upper air reports, a virtual requirement for any atmospheric modeling work, prepared during the war.[403]

This happy situation did not last long. A surprised Wexler, returning to Princeton two weeks later, found von Neumann ready to abandon the Project.

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[403] H. Wexler to F. W. Reichelderfer, 18 October 1946 (Wexler papers, B2, F1946).

Living and working conditions had led to an unstable personnel situation.[404] The computer work had started in IAS's Fuld Hall boiler room in June 1946, but Institute members wanted it "out of sight and out of mind."[405] The temporary building being moved in to house the Computer and Meteorology Projects was not yet ready, so office space was unavailable. Likewise, old WPA housing being moved to Princeton to provide living quarters had yet to be installed.[406] A troubled Reichelderfer jotted in the margin of Wexler's report: "We must not allow this important project to lapse."[407] Indeed, in the Project's progress report for the period ending in mid-November was the comment that the larger group originally anticipated to compose the Project could not be assembled due to housing problems. They would form a smaller group instead in an effort to create a more cohesive unit.[408]

In mid-November a group of prominent meteorologists and oceanographers visited von Neumann at Princeton to share their ideas about the use of the computer and approaches to solving the numerical forecasting problem. Problems with computer forecasts which produced abnormally large changes in pressure tendencies, already seen by Richardson, Elliott, and an AAF officer at UCLA (Lt.

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404 Wexler to Reichelderfer, 4 November 1946 (Wexler papers, B2, F1946).
405 Ed Regis, *Who Got Einstein's Office: Eccentricity and Genius at the Institute for Advanced Study* (Addison-Wesley, 1987), 111. Regis's account of life at the Institute does not mention the Meteorology Project or that von Neumann was using his new computer to solve meteorological problems. Local Princeton residents were concerned that the new computer would make a lot of noise, and wanted it well away from Princeton's residential districts.
406 Aspray, *John von Neumann,* 58-59.
407 H. Wexler to Reichelderfer, 4 November 1946 (Wexler papers, B2, F1946).
408 Progress Report for the period of July 1, 1946 to November 15, 1946 on Contract N6ori-139, Task I (Meteorology Project) (von Neumann papers, B15, F16).

Philip D. Thompson) in their work, indicated that both a new mathematical approach and changes in the observational network would have to be made. Von Neumann thought that they would need to make trial runs on the ENIAC to make sure they were ready when his new computer came on line. The personnel onboard had been significantly reduced: Hunt was temporarily gone and Cahn had been fired due to numerous absences. However, von Neumann's spirits had been lifted by the visiting scientists. With Pekeris and Queney getting settled, he was no longer talking about leaving the Project.[409]

Wexler's next report (as ever, neatly typed single spaces with tight margins) noted that von Neumann was pushing for an objective method of determining the field of divergence, i.e., an area where air molecules are moving apart from each other, in the atmosphere. MIT and NYU, he argued, continued to use unacceptable subjective, non-mathematical methods. In order to use a more objective method, the Project would need very good upper air data – temperature, pressure, and wind velocities aloft. NYU possessed such data sets and would pick one set as a case study. Since the required calculations were extensive, they decided to ask the Bureau of Standards for assistance. The Bureau was happy to help.[410]

Maintaining the momentum building up in post-war research, Rossby organized a conference at the University of Chicago to discuss problems in meteorological research. The eighteen conferees at the December 1946 meeting

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[409] H. Wexler to Reichelderfer, 15 November 1946 (Wexler papers, B2, F1946). Visiting von Neumann were Athelstan Spilhaus and Hans Panofsky (both of NYU), Walter Munk (Scripps Institute of Oceanography), and Henry Stommel (Woods Hole Oceanographic Institution).
[410] H. Wexler to Reichelderfer, 22 November 1946 (Wexler papers, B2, F1946).

were primarily theoretical meteorologists from the University of Chicago, but meteorologists from NYU, MIT, the Meteorology Project and Weather Bureau attended also. Looking back at the list, there are two striking anomalies. There were no representatives from the west coast schools – UCLA and Caltech. And the final attendee was from the Soviet Union: Commander Ryshkov of the Hydrometeorological Service. Caltech's absence is not a surprise. Robert Millikan's meteorology program was not the least bit theoretical. It was focused on creating entrepreneurs in operational meteorology who could provide contract services to industry. The appearance of Soviet Commander Ryshkov in late 1946 demonstrates the still free flow of scientific ideas in the immediate post-war period.

The assembled meteorologists considered five major topics: the relationship of the wind and pressure fields, the scales and types of atmospheric perturbations, stability criteria, general circulation, and surface waves of finite amplitude. They held detailed discussions on the current and developing meteorological theory in each of these areas.[411] On the important topic of model development, Rossby argued that model conception needed a requisite physical nature in order to be useful. It appeared that the most fruitful path would be to create simple dynamical models which characterized the atmosphere.[412]

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[411] Notes on the Chicago Conference on Problems of Meteorological Research, December 9-13, 1946 prepared by S. Hess (Wexler papers, B3, F1947-1).

[412] Meeting for Discussion of Meteorological Problems, University of Chicago, December 9-13, 1946 – report of AAF Lt. Philip D. Thompson (Philip D. Thompson papers, National Center for Atmospheric Research Archives, Reports 1946-1948) [Hereafter **Thompson papers**]. The Thompson papers were being sorted in preparation for processing when I used them. Thompson's letters and other personal materials have now been filed and may no longer be in the folders noted.

Early winter had also heralded the arrival of AAF Lieutenant Philip D. Thompson (1922-1994) in Princeton. A man of extraordinary intelligence and unbridled ambition, Thompson received wartime meteorology training at the University of Chicago, but was then assigned to receive air traffic controller training. Thompson was not happy to be stuck in air traffic controller school after finishing an intensive course in meteorology. He was subsequently relieved of those duties due to a temperament "not suited to the high nervous tension...developed during this duty."[413] Thus, when Thompson requested reassignment to duty as a Weather Reconnaissance Observer he was almost denied that position for the same temperament issue.[414] Ultimately, he was returned to the Air Weather Service and assigned to the Army Weather Station, Long Beach Army Air Field, California. While there, he learned that the Army Research Weather Station at UCLA needed a couple more officers on its staff. Thompson applied, noting that his "greatest interest, and, in consequence, usefulness lies in meteorological research, rather than in operational forecasting."[415] The request approved, Thompson made his move into meteorological research.

Working on objective forecasting techniques, Thompson was much intrigued when he heard about the work of the Meteorology Project from UCLA meteorology professor Jörgen Holmboe. Although Thompson claimed Holmboe

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[413] Headquarters, North Atlantic Division, Air Transport Command to Commanding General, Air Transport Command, 30 October 1944 (Thompson papers, Army Orders 1940s).
[414] Headquarters, Eighth Weather Squadron to Station Weather Officer, 1388th AAF Base Unit, 22 November 1944 (Thompson papers, Army Orders 1940s).
[415] Thompson to Commanding Officer, Headquarters, Army Air Forces Weather Service, 17 December 1945 (Thompson papers, Personal 201 File).

shared a *New York Times Sunday Magazine* interview of von Neumann and Zworykin with him in the fall 1946, no such article exists. In any case, Thompson called his commander, General Ben Holzman, directly and convinced him to authorize a trip to Princeton to visit with von Neumann. He was subsequently assigned by the Air Weather Service to serve a tour of duty with the Meteorology Project.[416] At the time of these events, Thompson was a first lieutenant with a bachelor's degree from Chicago. First lieutenants do not have direct pipelines to generals. That Thompson had the chutzpah to call Holzman directly, much less request an audience with von Neumann is only a small hint as to the measures he would take to get what he wanted, when he wanted it. While this trait generally worked to advance his career, it would not always prove endearing to his colleagues.

With the arrival of Thompson on-scene at the Meteorology Project, the Air Weather Service made its first contact with numerical weather prediction. Moreover, the military weather services finally had a member on-site. Now the three constituencies – academic meteorologists, Weather Bureau civilian meteorologists, and military meteorologists – were all represented.[417] By bringing in a military member, the Project – perhaps inadvertently – opened its work up to military scrutiny in an unintended way. Although under contract to the Navy which required periodic formal reports, von Neumann had enjoyed complete control over

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[416] Philip Duncan Thompson, "A History of Numerical Weather Prediction in the United States," *BAMS* 64 (1983): 757-758.
[417] History of the 72nd AAFBU Detachment, IAS, November 22, 1946 – December 31, 1946 (Classification: Restricted) by Philip D. Thompson (Thompson papers, Reports 1946-1948).

what those reports said. He could not, however, control what Thompson said. Thompson would be able to report directly to his military superiors without being censored by von Neumann. Indeed, the nature of what kinds of statements were released about the Project would soon become an issue.

The Air Force's seeming inability to allow its personnel to fill positions without having an organizational designation required Thompson to be assigned as the Officer in Charge (and sole member) of the 72nd AAFBU Detachment (Special Projects Unit). As such he needed to create a "mission statement" for himself and report in on his activities on a regular basis. Despite being on the job only a few weeks, Thompson filed his first report (classified "Restricted") at the end of December 1946. His detachment's mission: to restate meteorological problems as hydrodynamical problems, to formulate them as mathematical problems, and to solve meteorological problems capable of physical analysis. To do this, he would need to coordinate information from meteorology, fluid mechanics, mathematics, and electrical engineering. Therefore, Thompson had reviewed the work of Richardson and Elliott who used finite differences to examine the underlying mathematical structure of the graphical methods promoted by V. Bjerknes at the Bergen School. However, finite differencing was not a viable approach because available data were not sufficiently representative. Therefore it would be necessary to "examine systems which have simple analytical form, but which may be identified with the real atmosphere."[418] Since the Harvard Computation Laboratory and the Naval Ordnance Laboratory were also working on a numerical solution to the hydrodynamical problem, even if in a more general sense that the Princeton group,[419] Thompson noted it would be important to stay in touch with them.[420] Because of the heavy military presence and mission at these labs, this was probably a more comfortable arrangement for Thompson than for Pekeris or Queney.

Thompson had not been there long when he heard from a former UCLA acquaintance and wartime-trained meteorologist much interested in the Meteorology Project: Jule Charney.[421] Charney (1917-1981) had completed his undergraduate work in mathematics and physics (Phi Beta Kappa) at UCLA, and was on-track to receive the first Ph.D. in mathematics to be awarded there when he first heard Jörgen Holmboe lecture on fluid turbulence. Intrigued by the subject matter, he accepted an offer to become Holmboe's assistant and simultaneously participate in the meteorology program being established at UCLA in support of the war effort. And so Charney made the switch to meteorology. More comfortable with mathematical explanations than the more descriptive techniques then the

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[418] Ibid.
[419] Thompson to H. Wexler, 14 January 1947 (Thompson papers, Wexler, H., 1946-1950).
[420] Report of Temporary Duty, AAF Lt. Philip D. Thompson of January 20, 1947 (Thompson papers, Reports 1946-1948).
[421] Jule Charney is a fascinating figure in twentieth century meteorology deserving of a full biographical study. For a short biographical sketch, see Norman A. Phillips, "Jule Gregory Charney," *Biographical Memoirs* (Washington, D.C.: National Academy of Sciences, 1995), 66: 81-114). See also Richard S. Lindzen, Edward N. Lorenz, and George W. Platzman, eds., *The Atmosphere – A Challenge: The Science of Jule Gregory Charney* (Boston: American Meteorological Society, 1990) for a number of short articles describing Charney's influence in a variety of meteorological fields, an interview conducted by Platzman shortly before Charney died of cancer in 1981, and five of his most important papers.

mainstay of meteorological studies, Charney went his own way and produced a masterful Ph.D. dissertation published in the *Journal of Meteorology* entitled "The Dynamics of Long Waves in a Baroclinic Westerly Current."[422] Having completed his degree, he was awarded a National Research Council fellowship and set his sights on the University of Oslo. There he would study with the leading mathematician of the Bergen School, Halvor Solberg. However, en route, he stopped off at Chicago and called on Rossby. Falling under the famous Rossby spell, Charney stayed for almost a year, taking part in the free-wheeling discussions of meteorological theory with the Chicago staff and the many foreign visitors Rossby attracted there. As a result, Rossby took him along to Princeton for the August 1946 meeting setting up the Meteorology Project. There Charney met von Neumann and was drawn in to the problem of numerical weather prediction. Now preparing, at long last, to leave Chicago for Oslo, he wanted some first hand information from Thompson on just what was happening with the Project.

Charney had been mulling over the numerical weather prediction problem since the August 1946 meeting. He wanted to share his ideas with Thompson about how to consider wave speed and other motion ideas with an eye to setting up a system of solvable equations for the atmospheric problem. Since it would be hard to share these ideas by mail, Charney suggested that Thompson invite him to Princeton for a visit since they had "all that Navy money lying around."[423] Indeed,

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[422] Jule Charney, "The Dynamics of Long Waves in a Baroclinic Westerly Current," *Journal of Meteorology* 4 (1947): 135-162.
[423] Charney to Thompson, 7 February 1947 (Thompson papers, Charney, Jule 1947-1950).

Charney was able to make the trip in March just prior to leaving the United States and dropped his detailed findings in the mail to an eagerly waiting Rossby in Chicago. In Charney's opinion, the Princeton Project was the "ugly duckling" of meteorology, but had the potential to become a "swan." There were several problems at Princeton, not the least of which was that meteorology was the "weak sister" of the group. With no cooperating meteorology department, the meteorologists were largely isolated, and worse, seemed to have no coordinated approach. Queney – with limited facility in English and little rapport with von Neumann – was working on a variety of wave motions, none of which seemed to have any relation to the real atmosphere. Pekeris and Panofsky (working off-site at NYU) had almost nothing to do with the Project. Thompson was very capable, Charney believed, but had little knowledge of other work. He was, however, the only person on the Project who realized the significance of rapid readjustment processes for large scale motion in the atmosphere. Von Neumann regarded scale questions as being of secondary importance and attributed instability in the calculations to computation processes themselves and not to the physics of motion. This concerned Charney. He thought Project members needed to consider the possibility of dynamic instabilities being inherent in the system, which could lead to computational errors. In his opinion, the instability that von Neumann alluded to was the same phenomenon already discovered by meteorologist Victor Starr when he was "playing around" with difference equations at the University of Chicago. Charney thought they would all be better off if the Meteorology Project were co-located with Rossby in Chicago.[424] However beneficial that might have been, it did not happen. Queney eventually left the Project, leaving Thompson on his own for almost a year.

In March 1947, Thompson wrote a "survey" of the IAS Project, primarily for Air Weather Service consumption, but also with the idea that the Project needed to get out some accurate information about its work to counteract some of the more sensational publicity that had been printed in the popular press. (Sample headline: "Scientists Get Ready to Do Something About the Weather; World-Wide Observation Planned; Force to 'Counter-Attack' Storms Considered" from the 20 January 1947 *Chicago Sun.*) He was extremely concerned about overselling the project and expressed the hope that readers of his survey would do so without "undue optimism, though certainly not with preconceived pessimism." He was aware that many forecasters were very skeptical about numerical techniques. Results produced in haste could ultimately lead to the downfall of support for this new approach to weather prediction. He then went on to explain the motivation behind NWP. There could never be an analytical solution to the hydrodynamical equations which describe the physics of the atmosphere. That being the case, a new line of attack had to be taken. With the introduction of the new high-speed electronic computers and the increased amounts of available data, numerical analysis techniques could now be applied to the problem. Thompson explained

[424] Charney to Rossby, ca. 19 March 1947 (Charney papers, B14, F460).

these methods in great detail and sent off copies of his report to the Air Weather Service for internal distribution only. [425]

More than anything else, Thompson wanted to get out some corrective publicity before the meteorological community looked upon the Meteorology Project as an unprofessional excursion into scientific hype. He sent a copy of the report to fellow AAF officer Robert Bundgaard with a note saying the survey was deliberately "conservative and vague" because von Neumann wished to publish his own paper on computational stability.[426] Wexler, having received a copy also, agreed with Thompson that the report should be published just so they could remove a "good deal of the mystery" surrounding the Project.[427] However, having been burned by inaccurate press reports, von Neumann did not want Thompson's survey published in a peer-reviewed journal (or any other kind of journal), so it was only published in restricted Air Force gray literature.[428] Consequently, there was little official information about the Project reaching the scientific community or the Weather Bureau staff for that matter. Indeed, after receiving another trip report from Wexler about his recent visit to Princeton, Reichelderfer noted at the bottom of the memorandum, "This project is still in the 'prospecting stage' but it represents a possibility which has general interest and perhaps our field service should be

[425] Philip D. Thompson, "Survey of the Electronic Computer Project at the Institute for Advanced Study," 6 March 1947 (Thompson papers, Electronic Computer Project, IAS). Thompson to Delmar Crowson, 11 March 1947 (Thompson papers, Crowson, Delmar 1947).
[426] Thompson to Robert Bundgaard, 27 March 1947 (Thompson papers, Bundgaard, Robert, 1947).
[427] H. Wexler to Thompson, 17 March 1947 (Wexler papers, B2, F1947-5).
[428] Thompson to H. Wexler, 21 April 1947 (Thompson papers, Wexler, H. 1946-1950).

informed. What do you think of a brief, factual (not visionary or over-optimistic item)..." for the Weather Bureau newsletter?[429]

Thompson was not the only one to pen a report on NWP. Albert Cahn, fired from the Meteorology Project, had subsequently joined the National Bureau of Standards. In June 1947 he sent his report on NWP to Wexler. Cahn noted that insufficient data density and poor data accuracy were huge problems. Indeed, Richardson named these same data problems as the ultimate source of the errors in his prediction in 1922. This had led to what Cahn labeled "a symbiotic inertia of form: there is no use developing methods to get extensive, accurate observations since there is no use for them; on the other hand, there is no point in funding computing techniques to do numerical forecasting since the required observations are not available." However, with new computers being developed, the balance had changed. Now they needed to determine which observations were truly prerequisite to good forecast output from models. After considering a number of questions which would guide the answer on observations, Cahn asked, "Do you think the problem of predicting the weather is worth all the effort we seem to be spending?"[430] Cahn was a theoretician, not an operational meteorologist providing daily forecasts, and this issue deeply troubled Weather Bureau leaders. How many other theoreticians felt as Cahn? How many forecasters, skeptical that the numerical methods would ever work, would also agree with him?

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[429] Reichelderfer to H. Wexler, 2 June 1947 (Wexler papers, B3, F1947-4).
[430] Cahn to H. Wexler, 12 June 1947 (Wexler papers, B3, F1947-7).

Thompson was the only person remaining in the Meteorology Project "team" after Queney departed in September 1947. In his Project report, Thompson noted that he continued on mathematical-physical research, while the NYU subcontract group under Panofsky was doing synoptic and empirical work. Cooperating government agencies were making the extensive numerical computations.[431] Reporting to Reichelderfer, Wexler was impressed that Thompson had managed to produce significant results on his own despite chronic staffing problems. The Weather Bureau was making a contribution by giving advice and suggesting problems. Wexler was working on a variety of theoretical problems himself, and the Weather Bureau staff was providing the hand-drawn analyses in support of the numerical weather forecasting project. Reichelderfer was very supportive of these efforts and very much desirous of keeping the Meteorology Project on track.[432] However, the Weather Bureau had a shortage of analysts and therefore could offer limited help. The Meteorology Project had no analysts at all.[433] This would prove to be a continuing problem for the Project. The only way to secure data to use for the initial conditions of the models was from an analyzed chart. Why? Because when raw data came in, they were plotted on a chart. The chart was then analyzed with the familiar lines of equal temperature, pressure, wind speed, etc. Analysts then placed a grid over the analyzed chart and extrapolated the

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[431] Philip D. Thompson, "Report of Progress, September 1947" (Thompson papers, Princeton Computing Project).
[432] H. Wexler to Reichelderfer, 21 November 1947 (Wexler papers, B3, F1947-6).
[433] Philip D. Thompson, "Concerning the Numerical Forecasting Project," ca. 1947 (Thompson papers, Reports 46-48).

values to the grid points. These extrapolated values became the initial values (or initial conditions) for the calculations. No analysts – no initial conditions. Therefore, the Project had to have access to enough qualified analysts to provide starting point data as well as analyzed charts to verify computer predictions.

Sharing Reichelderfer's desire to keep the Project moving ahead was Jule Charney. Since the spring of 1947, he had been at the Oslo Institute of Meteorology working to develop a solvable set of equations out of the basic hydrodynamic equations. He considered weather forecasting to be primarily a computing problem which required "one intelligent machine and a few mathematico-meteorological oilers." He thought the Meteorology Project would have von Neumann's machine soon, but it was lacking in "oilers". For that reason, and because he was convinced that he had found a solution to the forecasting problem by applying filters to get rid of unnecessary "noise," Charney wanted very much to join the team instead of accepting offers from the University of Chicago and UCLA.[434] His eagerness to try out his new filtering method on the computer was not his only reason for wanting to join the Meteorology Project. Charney's primary reason for going to Princeton was his misgivings about the way the Project was headed. "Unless some physical ideas are brought to bear," Charney confided to a close colleague, "the project will die out through mathematical sterility. I have no delusions of grandeur about my own possible contributions, but at least I may help to give it the right slant."[435]

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[434] Charney to Thompson, 4 November 1947 (Thompson papers, Early IAS Papers).
[435] Charney to J. Bjerknes, 14 January 1948 (Charney papers, B4, F120).

Von Neumann was delighted to hear from Thompson that Charney wanted to join the team. He invited Charney to become a member of the Project at the conclusion of his fellowship year in Norway and inquired of his requirements. He also wanted to know if the Norwegian meteorologist Arnt Eliassen (1915-2000), working with Charney in Oslo, would be willing to come too.[436]

Charney wrote to von Neumann in early 1948 to accept the invitation to join the group. He used the occasion to offer his opinion on what should be their future path, and advised von Neumann of the progress he had made during the previous year. Charney had determined that the dynamical equations of the atmosphere could be reduced to a single linear partial differential equation in the pressure and of the first order in the time, if it was assumed that the large-scale atmospheric motion is governed by the conservation laws of entropy and potential vorticity, and by conditions of quasi-hydrostatic and quasi-geostrophic equilibrium. That being the case, only the initial pressure distribution was required for its integration – good news since it was easily obtained. If the short wave motion could not be eliminated from the dynamical equations, it would be necessary to start with the initial vertical velocity and horizontal divergence or the initial pressure tendency. Although those quantities could be determined, they could not be determined to the accuracy required for numerical techniques.[437]

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[436] Von Neumann to Charney, 19 November 1947 (von Neumann papers, B15, F1). Little published information in English is available on Arnt Eliassen. See Brian Hoskins, "Arnt Eliassen," *Quarterly Journal of the Royal Meteorological Society* 126 (October 2000): 2985 for an obituary.
[437] Vorticity is the local rotation in a fluid flow. A fluid is quasi-hydrostatic if the vertical accelerations within it are small without being zero. A fluid is quasi-geostrophic if a system evolves

Charney also noted that there was no rule to distinguish large and small scale motions, but that his filtering scheme removed all wave motion smaller than those small-scale wave cyclones (extending several hundred miles) which appeared on weather maps. Further, such a separation was not strictly mathematically justifiable, since the equations were not linear. However, because the small-scale perturbations in the atmosphere could be considered as "turbulent fluctuations which give rise to small Reynold's stresses and transports of heat and moisture," they could be ignored in the first approximation. Therefore, although the transition motions might indeed be meteorologically significant, there were not sufficiently accurate data to handle them. Charney felt they could hope to "forecast the principle (sic) large-scale current systems" and then regard these as steering currents for the smaller scale motions.

Having dispatched small scale motions, Charney then set his sights on long waves. He pointed out that no one knew what mechanism controlled them. Therefore, he thought they should forecast large-scale perturbation movements for one to three days and see what happened. If they could accomplish that, then the money and effort would be worthwhile.[438]

Finally, turning his discussion to more mathematical considerations, Charney referred back to Richardson's efforts. He declared the importance of

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slowly compared to the rotation period of the earth, is larger than the distance cold pools of air can spread under the influence of the Coriolis force (the apparent deflection of a particle due to the rotation of the earth underneath it), and has only limited vertical movement.

[438] Long (or planetary) waves are found in the westerlies, have long length, significant amplitude, and periodicity in time and/or space.

looking at computational stability and correspondingly to the grid space and temporal scales to be used. Because the atmosphere is a dispersive medium, the influence of energy propagation from outside the area of interest must be taken into account. This outside energy does not propagate at the same speed as the disturbances themselves and as far as Charney knew, "the question of energy propagation in finite amplitude systems is unsolved." With meteorologists at Chicago, Oslo, and Stockholm looking at these perturbations, they had found "many examples" where the "influences of neighboring atmospheric perturbation on one another" are propagated faster than the disturbances themselves and even faster than the wind velocity. Models could help in the investigation, but selecting those models was a physical problem and therefore the problem of numerical forecasting would require a combined effort of mathematicians and physicists. But, perhaps most importantly, Charney realized that not even a mathematical and physical approach would be enough. The meteorology group needed people who knew enough about meteorology to know "when and how to make the approximations." This was an extremely important insight. The equations defining atmospheric motion were never going to be solved if all of the terms were left in. Therefore, the terms least likely to impact the solution would have to be removed or a value substituted in for them. Practicing meteorologists already made those approximations in their heads during the course of their forecasting day. Someone who had been forced to make tough decisions about what to keep and what to throw out would be crucial to the success of this project.

To this end Charney recommended bringing over Arnt Eliassen from the Norwegian Meteorological Institute. Eliassen possessed experience in both *synoptic* and *theoretical* meteorology and was also interested in numerical solutions. Head of the Norwegian Forecasting Service, Sverre Petterssen – who had spent the war years advising the Meteorological Office, British Air Ministry (and had participated in the D-Day invasion forecast) – had approved a year's leave of absence for Eliassen, who was "quite willing" to come. Charney thought it was important to combine theoretical work with empirical data, and therefore that it was important that some members of the group have "intimate experience with actual weather processes." For Charney, Eliassen was that man. To make sure Eliassen was "up to speed" on the latest synoptic work, Charney had already arranged for him to visit with Rossby at Chicago and to go to Weather Bureau Headquarters.[439]

Von Neumann responded to Charney in early February, telling him that he was "very anxious" to have him and Eliassen with the Project "next [academic] year." In response to Charney's inquiry regarding "professional and sub-professional help", he reiterated who would be in place: Thompson and Hunt. Panofsky and Haurwitz were collaborating from NYU and Wexler would continue to provide assistance from the Weather Bureau.[440] This meant the "group" would consist of Charney, Eliassen, Thompson and Hunt – significantly smaller than the initial plan set forth by von Neumann and even small by his revised plan.

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439 Charney to von Neumann, 2 January 1948 (von Neumann papers, B15, F1). See Petterssen, *Weathering the Storm* for Petterssen's account of his role in the D-Day invasion.
440 Von Neumann to Charney, 6 February 1948 (von Neumann Papers, B15, F1).

Nonetheless, the Meteorology Project was finally reaching some sort of critical minimum mass. Charney was very pleased to be joining the Princeton team and bringing Eliassen with him. His only concern was the salary, which was less than he had proposed. One reason for the concern: he viewed the Princeton job as "temporary" and anticipated that he would be moving his family from Los Angeles (where their household effects had been left during their time in Norway) to Princeton and back to Los Angeles within a year. Apparently Charney thought that his contribution would be over fairly quickly and he expected to rejoin UCLA's Meteorology Department based on his correspondence with Jacob Bjerknes.[441] Indeed, Bjerknes had asked Charney to let him know when he would be available after his "well accomplished job in Princeton."[442] Charney was to be in Princeton much longer than anyone anticipated.

THE END OF THE SLOW START

Despite the discussions of numerical forecasting, the Project was still primarily concerned with theoretical issues in the spring of 1948. Thompson, worried about his vulnerability to transfer due to the maximum length of time permitted on station, made it clear to his Air Force contacts that he wanted to remain with the Project until he was sure the fundamental problems vis-à-vis developing equations that described only the essential atmospheric phenomena were in place. Once that was done, numerical prediction would be within the reach of the group. In his

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[441] Charney to von Neumann, 29 February 1948 (Charney Papers, B16, F517).
[442] J. Bjerknes to Charney, 4 March 1948 (Charney Papers, B4, F120).

opinion, the Meteorology Project was the "first, and at present only direct and potentially successful approach to fundamental problems." He was completely convinced that the Project could succeed. Further, Thompson anticipated that funding from Navy contracts would continue indefinitely, although it appeared that the scope would be widened to include a broader range of geophysics topics.[443] This last comment probably referred to the request made by CDR Roger Revelle of ONR to expand the geophysics base of the Project.[444]

Little of substance had been accomplished during the first two years of the Project, but with the personnel situation about to improve, it was poised to take advantage of a new mix of talents. Hunt, having recently completed his doctoral program in mathematics, rejoined the Project. That meant Thompson and Hunt were the only team members in Princeton. Weather Bureau headquarters personnel handled plotting, analysis and data preparation as time permitted. Since there were no funds available to do this work, it could only be done once other work was finished. With von Neumann's computer still under construction, the meteorologists anticipated using either the Bureau of Standards computer or the ENIAC to perform the initial calculations.[445] Yet as the progress report for the six month period ending the middle of May 1948 indicated, there was still no organized and established meteorological theory.[446] Without such a theory, they

[443] Thompson to Joe Fletcher, 8 March 1948 (Thompson papers, Fletcher, J.).
[444] H. Wexler to Reichelderfer, 21 November 1947 (Wexler papers, B3, F1947-6).
[445] H. Wexler to Reichelderfer, 12 May 1948 (Wexler Papers, B3, F1948).
[446] Report of Progress, ONR Meteorology Project, IAS, Princeton, December 15, 1947 – May 15, 1948, 18 May 1948 (Charney papers, B9, F304).

would be unable to move towards the prediction phase. But help was on the way: by mid-summer Jule Charney and Arnt Eliassen would arrive from Norway and things would start to look up for the hard-luck Meteorology Project.

CHAPTER 6
AN INTERNATIONAL ATMOSPHERE: CARL-GUSTAV ROSSBY AND THE SCANDINAVIAN CONNECTION (1948-1950)

With the arrival in August 1948 of Arnt Eliassen – the first member of what might be called the "Scandinavian Tag-Team" – an international atmosphere returned once again to the Meteorology Project. By then Chaim Pekeris had moved to Israel, and Paul Queney had gone home to France. Both Pekeris and Queney, however, had been working in the United States before being asked to join the Project at the IAS. Eliassen, by contrast, was an imported scientist – imported to provide some measure of atmospheric reality to a project that was heavily theoretical. Indeed, he was imported because the de-facto head of the Meteorology Project, Carl-Gustav Rossby, was determined to have a significant influence on its outcome despite his physical distance from Princeton.

If this group was focused on the development of meteorological theory, why the need for personnel with synoptic experience? Synoptic meteorology relied on data collected worldwide and analyzed locally to make predictions. A very subjective endeavor, it was considered by theory-based dynamicists to be more an art than a science. However, Rossby recognized that any theory used as a basis for a computational solution had to include first those factors which were either consciously or unconsciously used by the forecaster. After all, they were adding significant skill to turn raw data into a representation of the atmosphere from which they could make a prediction. Any additional variables could be added once the approximations and assumptions of the forecasters had been included.[447]

Therein lay a potential problem for this still (even after two years of existence) fledging group – an issue touching on the fundamental reality of modeling. If the team members were looking strictly at elegant numerical solutions to the hydrodynamical equations, then they could develop internally consistent models. Such models could produce forecasts for conditions at multiple atmospheric levels correctly correlating with each other, but not necessarily having any relation to reality. As Rossby noted, the equations needed to be viewed as tools for studying problems suggested by the atmosphere, not as an end in themselves.[448] Without solid synoptic support, Charney's fear of the group becoming mathematically sterile would become a reality. Rossby's mission was to put those fears to rest.

ROSSBY'S OFF-SITE, ON-SCENE RESEARCH SCHOOL

Jule Charney had tremendous respect for Rossby and looked to him for guidance and intellectual stimulation. As Charney put it days before his departure to Norway: "[You] will see me in Sweden if I have to ski there from Oslo."[449] Thus, once Charney joined the Project, Rossby was given a free pass to influence it and he took every advantage of the opportunity. While Rossby was beginning to shuttle

[447] Rossby to Charney, 28 October 1948 (Charney papers, B14, F460).
[448] Rossby to Charney, 9 January 1949 (Charney papers, B14, F460).
[449] Charney to Rossby, 19 March 1947 (Charney papers, B14, F460).

between Chicago and Stockholm while setting up his meteorology department at the University of Stockholm in his native Sweden, Charney kept Rossby apprised of the Project's progress because he was "anxious" for him to "keep in touch with developments" at the Meteorology Project.[450] In turn, Rossby provided a steady stream of ideas, personnel, and encouragement to Charney. He also provided a publication venue: the new geophysical journal, founded and edited by Rossby: *Tellus*.[451] These developments point to the ways in which Rossby personally influenced the paths that meteorology in particular, and geophysics more generally, would take in the mid-twentieth century. In short, they underscore the importance of Rossby's far-flung, loosely-knit organization as a research school.

In 1981, historian Gerald Geison – drawing on the work of J. B. Morrell – described the fourteen attributes of a research school. Looking at research schools in history, e.g., Justus Leibig's in organic chemistry, or Enrico Fermi's in nuclear physics, he argued that successful research schools tend to possess a substantial number of these attributes, including a charismatic leader who possesses a research reputation, an "informal" leadership style, and institutional power. Such a leader inspires "discipleship," directs a focused research program, and has developed exploitable experimental techniques. Further, the research school has success in moving into new fields of research within its discipline, has a ready supply of potential recruits, a readily available publication venue, and students publishing early under their own names. The school is also able to produce a significant

[450] Charney to Rossby, 15 September 1948 (Charney Papers, B14, F460).
[451] Rossby to Charney, 25 September 1948 (Charney papers, B14, F460).

number of students and then find them positions (thus spreading techniques and ideas). Research schools are typically found in a university setting, and, importantly, have adequate financial support.[452]

The research schools examined in Geison's work were associated with three laboratory science disciplines: chemistry, physics, and physiology. Other studies have looked at research schools outside the laboratory, but none has considered the atmospheric sciences.[453] But it is clear that Rossby and his acolytes embodied the attributes of successful research schools. Atmospheric scientists who knew Rossby spoke of the "legendary Rossby charm."[454] When Charney stopped by the University of Chicago while en route from UCLA to Norway, as noted above, Rossby ultimately persuaded him to stay for nine months. As his Swedish colleague Tor Bergeron put it: "No one could withstand his infectious enthusiasm and personal charm; as a leader he could get even the least follower to realize his own worth, and he always met objects with gentle persuasion."[455]

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[452] Gerald L. Geison, "Scientific Change, Emerging Specialties, and Research Schools," *History of Science* 19 (1981): 24 (Chart II). J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix* 19 (1972): 3-7.
[453] For example see the following articles in Gerald L. Geison and Frederic L. Holmes, editors, *Research Schools: Historical Reappraisals, Osiris* Second Series 8 (1993): Pamela M. Henson, "The Comstock Research School in Evolutionary Entomology," (159-177); Joel B. Hagen, "Clementsian Ecologists: The Internal Dynamics of a Research School," (178-195); David S. Kushner, "Sir George Darwin and a British School of Geophysics," (196-223).
[454] Norman A. Phillips, "Carl-Gustaf Rossby: His Times, Personality, and Actions," *BAMS* 6 (1998): 1106.
[455] Tor Bergeron, "Carl-Gustaf Rossby," Obituary (in Swedish), *Kungl. Vetenskaps-Societetens Årsbok* (Stockholm): 17-23, quoted in Phillips, Loc. Cit., 1109.

Rossby already had an established research reputation by the late 1930s.[456] By the time the Princeton Project was set up and running, Rossby had previously established two meteorology departments in the United States (MIT and Chicago) and was beginning to set up another in Stockholm, where he moved permanently in 1950. He had also directed research programs for the U.S. Weather Bureau and been an active participant in the U.S. government's Research and Development Board. Notoriously lacking in attention to administrative details, he was happiest setting up programs, rounding up funding, and letting others take care of the day-to-day operations.[457] He never seemed to have a problem getting what he wanted no matter where he worked – he had the ear of people in high places and worked those connections. His research focus changed with the times – he worked on whatever he saw as the area with the most scientific potential. Therefore, he moved from aviation-related concerns, to dynamics, to numerical weather prediction, and later to tracking radioactive isotopes as a way of determining the general circulation of the atmosphere. In each case, he was at the cutting edge of a new field in the atmospheric sciences.[458] He drew students from all over the world, established his own geophysical journal (so that he could get research results in front of people who might not see them otherwise), and continually pushed his students and those

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[456] Rossby's most important papers include "Relation Between Variations in the Intensity of the Zonal Circulation of the Atmosphere and the Displacements of the Semi-Permanent Centers of Action," *Journal of Marine Research* 2 (1939): 38-55; "Planetary Flow Patterns in the Atmosphere," *Quarterly Journal of the Royal Meteorological Society* 66 (1940): 68-77; and "On the Propagation of Frequencies and Energy in Certain Types of Oceanic and Atmospheric Waves," *Journal of Meteorology* 2 (1945): 187-204.

[457] Thompson to Paul Worthman, 1 March 1949 (Thompson papers, Correspondence Worthman, P., 1949-1950).

[458] Byers, "Carl-Gustaf Rossby, the Organizer," 56-59.

associated with him to publish their work and publish it quickly – either in *Tellus* or other appropriate journals such as the *Journal of Meteorology* (which he also founded) published by the AMS.[459] He was responsible for finding the right people for the right job – keeping up with a huge number (based on existing archival evidence) of correspondents.

It was Rossby's vast network of contacts, and his influence over them, that would prove so critical to the success of the Meteorology Project.

MOVING TO A NEW LEVEL

The summer of 1948 saw a major infusion of enthusiasm and meteorological insight when Charney and Eliassen joined the Meteorology Project. Charney came armed with, and ready to try out, the techniques he had developed for filtering out 'noise,' e.g., sound and gravity waves that complicated the solution, while not influencing the weather. Eliassen, with his well-rounded combination of synoptic, theoretical and numerical skills, offered the promise of practical atmospheric experience to counterbalance the heavy theoretical emphasis. From this point on, the Meteorology Project maintained much closer ties with Rossby and, not coincidentally, made rapid progress towards a formal theory.

Before Charney's arrival, team members had individually taken on various problems to solve without first mapping out where they needed to go and how their

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459 See Phillips, "Carl-Gustaf Rossby," and John Lewis, "Carl-Gustaf Rossby: A Study in Mentorship," *BAMS* 73 (1992): 1425-1437 for discussions of Rossby as a meteorological leader and mentor.

individual projects might take them there. Once Charney took over, the emphasis of the four-man team shifted to mapping out a path, and then identifying and solving more general groups of problems along it. Charney, Eliassen, Hunt, and new arrival John Freeman focused on developing a method to mathematically integrate the meteorological equations so they could be solved by the new IAS computer. To help them reach this ultimate goal, they set up three preliminary goals:

(1) finding the governing laws of atmospheric motion;

(2) finding a way to numerically integrate those laws when written as differential equations;

(3) finding the requirements needed for solution.

To address these issues, the group proposed to consider a "hierarchy of 'pilot problems,'" each of which would contain more physical, numerical, and observational aspects of the general forecast than the preceding one.

Rossby had already shown that planetary circulations of the atmosphere were more amenable to quantitative techniques, so the team members decided to start there: more was known about large-scale than small-scale motions.[460] They could address this large-scale motion by using the hydrodynamical equations for a non-viscous, adiabatic fluid. As discrepancies appeared between the numerical solution and the actual observed state of the atmosphere, they could make changes to the equations by adding one parameter at a time so as to ascertain its individual

[460] C.-G. Rossby, "On the Propagation of Frequencies."

effect on the outcome. In this way, they hoped to avoid the problems that Richardson faced as a result of trying to do "too much, too soon."

The problem with the hydrodynamical equations was that they governed every type of atmospheric motion – including the sound and gravity motions, which were of no consequence to the meteorological situation. Therefore, the team had to filter out the smaller scale motions so as not to obscure the larger scale motions with noise.[461]

Because failure to get it right would doom the rest of the Project's efforts, Charney dealt with the noise problem first. Just a few weeks after his arrival in Princeton, Charney laid out his ideas on noise and other issues in a long, detailed, technical letter to von Neumann who was spending the summer working in Los Alamos. Charney wrote that the so-called "primitive equations" (those used by Richardson) were not going to work because there was no method of accurately measuring horizontal acceleration and divergence – both of which were very small differences between very large terms.[462] Therefore, the noise level in smaller-scale motions would mask the larger-scale components. No matter how much observational techniques improved (and they were not likely to improve *that* much), the noise problem would continue to exist. Since the horizontal divergence term appeared in both the continuity and vorticity equations, Charney's solution

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[461] Progress report of the Meteorology Group at the IAS, 1 July 1948 – 30 June 1949 (Charney papers, B9, F304).

[462] *Acceleration* is any change (either in speed or direction) of an air parcel. *Divergence* is the spreading out (if positive) or coming together (if negative) of the vector field representing motion of air parcels in the atmosphere. All definitions from Todd S. Glickman, editor, *Glossary of Meteorology (2nd ed)*, (Boston: American Meteorological Society, 2000), hereafter ***Glossary***.

was to eliminate it by combining the continuity and vorticity equations. The still unobservable horizontal acceleration term would remain, but Charney argued that it could be replaced by the geostrophic approximation (where the Coriolis force is equal and opposite to the pressure gradient force) which would filter out the gravity waves.[463] If the gravity waves were included, large initial data sets would be necessary to prevent an unstable computation. Using the filter would reduce the size of the required initial data sets.[464]

Charney acknowledged that two methods had already been proposed for solving the resulting system of equations: one by Thompson and one by von Neumann himself. Thompson's required one to make a "guess" as to the value of certain derivatives and thus created a situation where it was doubtful that the solution would be either stable or converge. Von Neumann's proposed that the kinematic boundary condition be used to determine the surface pressure change.[465] However, this approach required solving a three-dimensional equation as a two-dimensional one. Charney disputed that approach. Instead he proposed his own method which would be a direct integration for pressure by replacing the space derivative with finite differences. Assuming that the starting equations were correct, then both horizontal and vertical influences would propagate at a finite rate.

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[463] A *gravity wave* is a wave disturbance in which buoyancy is the restoring force on parcels displaced from hydrostatic equilibrium. *Glossary*, 346.
[464] In fact, it would later become known that models would have to resolve the gravity waves during the whole integration.
[465] The *kinematic boundary condition* is the condition that the fluid velocity directed perpendicular to a solid boundary must vanish on the boundary itself. *Glossary*, 432.

One would then only need to know initial data in a finite region around the forecast point of interest.

Studies of "pilot models" showed that the mechanisms of both horizontal and vertical propagation were very similar. Charney's proposal for an "immediate attack" on the numerical forecasting problems was to describe the initial pressure field in such a way that one defined the average motion as being two-dimensional (even though the atmosphere is a three-dimensional space) and replaced the actual atmosphere with a barotropic atmosphere. In a barotropic atmosphere, the surfaces of constant density or temperature coincided with surfaces of constant pressure. Thus, for any given pressure level, the analyzed lines of equal temperature and the lines of equal height would line up exactly.[466] The continued study of a two-dimensional problem would provide needed practice and experience to prepare for the eventual three-dimensional approach. Since the two-dimensional model would be less difficult, the team would be able to uncover modeling mistakes and data problems more quickly. And no less important on a project this large, it would provide a distinct psychological boost to the team members to be able to reach an intermediate goal along the way.[467] It was unrealistic to expect that they could model the atmosphere successfully on the first try. But if the team members could get a primitive form of the model to work, they could build upon that success.

When Von Neumann inquired about Thompson's approach, Charney restated his contention that Thompson's iterative method would *amplify*, not

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[466] *Glossary,* 76.
[467] Charney to von Neumann, 24 August 1948 (von Neumann papers, B15, F1).

eliminate, the noise. The atmosphere is, after all, three- and not two-dimensional, and must be treated as such. He also explained again that there was no other option than to eliminate the divergence term from the equations because it could not, under any circumstances, be measured.[468] So while von Neumann clearly had the upper hand when it came to computer design and numerical analysis techniques, Charney had the superior knowledge of atmospheric processes that would be needed if this project was to come to a successful conclusion.

Charney also had the ear, and the support, of Rossby. Finally settled in to the Project's routine, Charney wrote to his mentor. He apprised Rossby of the latest developments and included a copy of his letter to von Neumann. Charney was discouraged that things had been moving so slowly in Princeton: von Neumann was out of town and Eliassen was suffering from the distraction of finding housing in an impossibly tight housing market. However, he was happy to report that the objective analysis part of the project, which had been underway at NYU, was being dropped – along with its requirements for the bulk of the funding. But what Charney really wanted were Rossby's comments on the ideas he had presented to von Neumann.[469]

With his acolyte, Charney, on board in Princeton, Rossby had a built-in conduit to influence the Project's direction. He immediately started filling the pipeline with advice – technical and professional. Instead of providing the feedback

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[468] Von Neumann to Charney, 17 September 1948; Charney to von Neumann, 21 September 1948 (Charney papers, B16, F517).
[469] Charney to Rossby, 15 September 1948 (Charney papers, B14, F460).

Charney had requested, Rossby launched into his own views on atmospheric instability and concluded by saying that he believed that he had the "instability problem by the tail." It was unfortunate that they were separated by such a great distance, because Rossby really wanted to talk it all over with Charney in person. He also had a directive for Charney: "condense the letter to von Neumann for publication in *Tellus*." By doing so, Charney would be stating the principal problems which they faced with the computer project, including a discussion of the significance of noise, high signal velocities,[470] and the character of the barotropic model which would be "interesting and useful" to the geophysics community.[471] Rossby wanted to spread the word, and spread it quickly. Too little information had been coming out of the Project in the preceding two years. In true research school fashion, he wanted to get these important theoretical developments out in front of the geophysics community.

Capitalizing on yet another opportunity to bring the Project's work to the attention of the wider meteorological community, Rossby wrote again just two days later. This time Rossby wanted Charney to write a brief note – based on the Project's work on signal velocities – for the *Journal of Meteorology* to accompany a paper by Tu-cheng Yeh on energy dispersion. In short, Charney and his colleagues had had to determine under what circumstances a "perturbation" would be carried into the forecast region during the period of interest. Consider an

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[470] The *signal velocity* is the propagation speed of a hydrodynamic influence. *Glossary*, 684.
[471] Rossby to Charney of 25 September 1948 (Charney papers, B14, F460). Tu-cheng Yeh, "On Energy Dispersion in the Atmosphere," *Journal of Meteorology*, 6 (1949): 1-16.

extremely large (1000 miles x 1000 miles x 30,000 feet high) box enclosing part of the atmosphere. If one were to forecast what the atmospheric properties of the cube would be 24 hours later, one would need to know how quickly atmospheric energy was moving in from the west (assuming the flow is west to east as it is in the mid-latitudes). If the inbound (horizontal) flow was only moving at 10 knots (nautical miles/hour), then only those features within 240 nautical miles of the western edge of the cube would enter it. Everything more than 240 nautical miles west of the western edge could be ignored without adversely impacting the forecast. Therefore, in areas of extensive data coverage, there were sufficient data to make a one- or two-day prediction. Further, the vertical velocities were so slow, that any disturbances that were in the stratosphere could not work their way down to the lower troposphere in this short forecast period either. Therefore, available upper air data were sufficient for the task at hand – an extremely important consideration since they were not likely to get more (expensive) upper air reports just to satisfy numerical weather prediction requirements. Rossby thought that if Charney explained the significance of signal velocity to the computing project, he would educate the readership to get away from explanations "in situ." Rossby thought this was just the piece needed to enhance Yeh's work.[472] And, of course, just the piece to alert meteorologists around the world to Charney's work in Princeton.

Rossby had immediately grasped that the problems being faced by the Meteorology Project, and the solutions they developed, would be an important

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[472] Rossby to Charney of 27 September 1948 (Charney papers, B14, F460).

starting point in his drive to sell applied meteorologists on the importance of theory, and theoretical meteorologists on the importance of thinking physically. Pursuing the advancement of this agenda, Rossby once again wrote to Charney offering advice and direction. This time he urged Charney to tackle the internal wave[473] problem as Charney himself had suggested in his letter of 15 September. (Charney, an infamously poor correspondent, had not yet responded to Rossby.) Rossby himself had attempted to work on the problem for an incompressible atmosphere with either constant stability or a sharp density discontinuity. He had found stable waves and a small range of phase velocities. Again he urged Charney to write an article for *Tellus* about the computing project. He did not want to overload the journal with theory and thought that a "clearly written exposé" about a computable model might help meteorologists gain a better attitude towards theory. He also asked Charney to discuss the problem of measuring approximations because it seemed to Rossby that "the majority of theoretical meteorologists hide their inability to think physically behind absurd insistence on 'accuracy.'" Rossby closed his letter by reiterating his belief that the Meteorology Project was important. But he was concerned about the "vast amount of housecleaning required in the storehouse of ideas among theoretical meteorologists and partly over the vastness of the educational task among the so-called practical meteorologists."[474]

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[473] An *internal wave* is a wave in fluid motion having its maximum amplitude within the fluid or at an internal boundary. *Glossary,* 411.

[474] Rossby to Charney, 24 October 1948 (Charney papers, B14, F460).

Rossby wanted those studying with him in Stockholm to be thoroughly familiar with Charney's ideas and the progress of the Meteorology Project. And he wanted to ensure that Charney knew that his European-based brethren were taking his ideas seriously. Yet, his greatest concern was educating all meteorologists to the potential of numerical methods to the advancement of theory and forecasting. Thus, Rossby had visiting Chinese meteorologist H. L. Kuo lead a review of Charney's ideas on stability as presented to von Neumann. Penning yet another note to Charney, Rossby reported that they had held an "extremely stimulating discussion" of his current work. Why, even those skeptical synopticians in attendance finally understood that they meant "business with the computing project." Again he tweaked Charney: get the letter to von Neumann cleaned up for publication – this time for the *Journal of Meteorology* –for the "education of meteorologists." Moving on to theoretical considerations, Rossby wanted theory to get back to fundamentals, i.e., that theory needed to express the factors that forecasters use, either consciously or unconsciously, when making a forecast. Other terms, e.g., the divergence term, which either could not be considered, or were not considered, by the forecaster could be included at a later time, but for now should be eliminated from the equations. Assumptions that were already successfully used by forecasters, i.e., neglecting compressibility and non-adiabatic processes aloft, should be considered when formulating theory.[475] Rossby wanted to make sure that

[475] *Non-adiabatic* processes are those which involve an exchange of heat with, in this case, the atmosphere surrounding an imagined parcel of air. See "diabatic process" in *Glossary,* 214. Rossby to Charney, 28 October 1948 (Charney papers, B14, F460).

in their pursuit of theory, the Meteorology Project members – Charney in particular – took advantage of the knowledge already gained by those who actually dealt with the weather on a daily basis at the forecast desk.

Rossby had a wide network of contacts throughout the geophysical community with whom he was in regular dialogue. To keep his outgoing pipeline of information filled, he needed regular updates from Charney. He was not getting them. In December 1948 – having heard nothing from Charney in three months – he wrote again, chiding and nagging. Rossby was curious about the progress in Princeton. What was going on? He knew that Charney had not written the article for the *Journal of Meteorology* because Rossby protégé and editor, George Platzman at the University of Chicago, had neither heard from Charney nor gotten a manuscript in the mail. Rossby badgered Charney to get it out. And he pleaded once again for a summary for *Tellus*, not to compete with the *Journal of Meteorology,* but because the Swedish physicists and geophysicists needed to know that meteorologists were thinking in terms of calculating flow patterns.[476]

That Rossby was trying to get the word out to other scientific disciplines about the new, more theoretical approach in meteorology, was apparently lost on Charney. When he *finally* responded to Rossby with a long, newsy letter, he made it perfectly clear that he agreed with Rossby's philosophy of approaching meteorological problems, but was not going to write an article for *Tellus.* Charney assured him that he was indeed writing an article for *Journal of Meteorology*

[476] Rossby to Charney, 13 December 1948 (Charney papers, B14, F460).

because it was better for "propaganda purposes." He saw no point in writing two articles about the same thing – an attitude that probably did not please Rossby.[477]

Operating on Rossby's research philosophy – once you think you have it figured out, try it on actual data and see what happens – Charney had the group consider an actual case. The starting point was a 500 millibar (mb) constant pressure map, i.e., a map which represents a surface in the atmosphere where the pressure is everywhere 500 mb – considered to be the half way level between the earth's surface and the top of the atmosphere. The lines on the map represent the height above the surface where the pressure is 500 mb. It varies from place to place, with the average height being about 18,000 feet (5500 meters). (This is different from a surface weather map where the earth's surface is considered to be sea level everywhere and the lines (isobars) on the map create a pattern of pressure values that vary across the surface. High numbers represent higher pressures, i.e., the weight of the air above that part of the surface is high. Low numbers represent lower pressures, i.e., the weight of the column is less than in higher pressure areas.) The team members selected 500 mb height values at 45° N latitude and inserted them into the formula derived by Charney and Eliassen.[478] They were thus able to successfully predict the deepening of a major trough (an elongated area of low pressure) in the central United States and the intensification of a ridge (an elongated area of high pressure) in the eastern Atlantic. Since the technique was

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[477] Charney to Rossby, 20 December 1948 (Charney papers, B14, F460).

[478] This method was ultimately published in J. G. Charney and A. Eliassen, "A Numerical Method for Predicting the Perturbations of the Middle Latitude Westerlies," *Tellus* 2(1) (1949): 38-54.

quite simple and had given such good results, Charney was convinced that it might prove immediately useful to forecasters. A forecaster would be able to forecast a pressure profile for a given latitude in less than thirty minutes. It was particularly important that the intensification and weakening of pressure features were explained solely as a result of the horizontal dispersion of energy with conservation of absolute vorticity. This indicated that the use of two-dimensional finite amplitude methods would give even better results for the equivalent barotropic atmosphere. Although in some cases energy would come from above or below, it appeared that when considering the mean motion, one could predict many features by using the barotropic assumption – a major simplification of the problem.

However, one successful trial over the continental United States did not mean that the method could be generalized to other parts of the globe. So Charney had the team perform a similar trial run over the Pacific. Where the first trial was successful, the second was a complete disaster. The initial situation showed an extremely long trough which should have been moved rapidly to the west. Indeed, their model did forecast it to do so. Unfortunately, in the way these things happen in real weather situations, the trough itself did not move at all. It stayed where it was. That left the Project members puzzled – not about why their model predicted movement that did not take place, but about why the trough did not move!

While a barotropic model showed promise, Charney knew that it would not be the final solution. Why? Because the atmosphere was not usually barotropic. If it were, cyclonic systems and their accompanying fronts would fail to develop. It was when the thermal and height patterns were not in perfect agreement – a state called baroclinicity – that what the lay person commonly refers to as storm development, would take place. Therefore, at some point, Charney knew they would have to attack a baroclinic model. That was significantly more complex. An intermediate model would be one dubbed the "equivalent barotropic." It was an ideal model to attempt because it needed only height gradients as initial values – information easy to come by. Charney had found that this model worked as long they used a one-hour time step and 400 km grid spacing. Once the team thoroughly investigated this model, Charney was confident that they could expand it to the baroclinic case. No matter what the investigation, Charney continued Rossby's directions: try the simple version first and when it works increase the complexity. That is exactly what Charney intended to do.

Although Charney had not been hired as the director of the Project – he was a member like everyone else – he was the most "senior" person on-site and coordinated personnel issues, often times with Rossby. The staff remained small: Charney, Eliassen, Thompson, and Hunt. The Air Force transferred Thompson out after two years on the job – normal procedure. His hole was filled by Weather Bureau meteorologist John C. Freeman, a specialist in shock waves. That still made it a four man team. After checking the accounts, Charney determined that there was enough money to hire one more person. Eliassen was working out extremely well ("priceless"), but Charney thought he was homesick and might be made to feel more at home if they could bring in another Norwegian. He turned to Rossby. Was

Einar Høiland a possibility? Charney had already committed for another year with the Project. After all, he could not very well leave "just as things begin to get interesting." His happiness would be complete with the addition of one more person – Rossby himself. Charney asked him if he would consider joining the Meteorology Project.[479] After all, it was not going to close down any time soon. Von Neumann had requested, and received, an extension from ONR. They were now guaranteed funding through 30 June 1950.[480]

Rossby continued his mission of closely following the progress of the Meteorology Project, providing advice whether it was asked for or not, and encouraging quick publication in *Tellus* – especially because he wanted to get the word out on the new scientific meteorology to all those physicists who doubted their scientific intentions. Writing in early January 1949, he expressed much interest in Charney's work on the extension of the energy propagation equation. However, Rossby was having a difficult time accepting Charney's conclusion that the barotropic convergence was of little or no importance. This was largely due to the "absurd result" in non-divergence theory that the western edge of a solitary disturbance is displaced with the speed of the zonal wind eastward which would be, in Rossby's view, much too fast. Disturbances just did not move at the same speed as the wind. And again, he reminded Charney that he was not only welcome to publish in *Tellus,* he was most strongly encouraged to do so. Rossby was extremely

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[479] Charney to Rossby, 20 December 1948 (Charney papers, B14, F460).
[480] Von Neumann to M. R. Lipman (ONR), 10 December 1948 (von Neumann papers, B15, F2). John N. Adkins (ONR) to von Neumann, 3 January 1949 (von Neumann papers, B15, F2).

eager to show "pure" physicists that meteorologists were "getting out of fiddling" and developing significant theoretical approaches.[481]

Much to Rossby's consternation, Charney did not submit his "cleaned up" letter to von Neumann to the *Journal of Meteorology* until April 1949 – a significant delay in getting the word out to the skeptical meteorological community. But undoubtedly hoping for better cooperation from Charney on his next paper, Rossby continued to provide publishing advice on both venue and content. While visiting Chicago, Rossby jotted Charney a note about the new Charney-Eliassen paper. Rossby told Charney that von Neumann should write a preface for the paper giving a short explanation of the computer project itself and what it hoped to accomplish. The recommended publication venue: the *Journal of Meteorology.* In this case, Rossby was not pushing for publication in *Tellus,* because he thought it was more important to bring these new developments to the attention of the meteorological public in the United States than to publish in Sweden. He would, of course, print it immediately if Charney went the *Tellus* route. Rossby further advised Charney to include samples of numerically predicted pressure profiles so that readers could see how the output looked.[482] In a follow-up note written while en route to Sweden ("please mark coffee as gift"), Rossby suggested that the joint paper compare predicted and observed changes in the profiles because it would be a "severe test, but more fair" than comparing actual

[481] Rossby to Charney, 9 January 1949 (Charney papers, B14, F460).
[482] Rossby to Charney, 4 February 1949 (Charney papers, B14, F460).

profiles.[483] He also sent a telegram asking Charney to send a brief statement on what was happening at the Meteorology Project for the May issue of *Tellus* because it was "essential" to keep the progress of the computing project before the scientific public.[484]

By March, Charney had apparently put in a word with von Neumann about Rossby coming to Princeton, because Rossby expressed his appreciation to Charney and reiterated that he would very much like to join the Meteorology Project. However, Rossby desired to maintain a lectureship in Chicago. This would allow him to maintain close ties to its meteorology program and also work in Princeton with minimal travel back and forth to Chicago. Rossby did not anticipate a large experimental plant being required in Princeton because he was counting on extensive cooperation with other institutions, particularly Washington, Chicago, NYU, MIT and UCLA. He also wanted the opportunity to bring in a couple of younger meteorologists to do the needed synoptic investigations in order to continue the work in basic theory.[485] Rossby thought it was "absurd" to set up an organization at Princeton which would compete with meteorology departments at universities. Instead he envisioned a totally cooperative relationship with both academic departments and government agencies.[486] Rossby recognized that there were not enough academic meteorologists to go around as it was – nor enough graduate students to fill their programs. Adding another formal meteorology

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483 Rossby to Charney, undated, ca. late February 1949 (Charney papers, B14, F459).
484 Rossby to Charney, 5 April 1949 (Charney papers, B14, F459).
485 Rossby to Charney, 27 March 1949 (Charney papers, B14, F459).
486 Rossby to Charney, 23 April 1949 (Charney papers, B14, F459).

department would just take away from the others for no net gain to the community of researchers.

In the interest of getting their results out sooner rather than later, Charney and Eliassen decided to submit their joint paper to *Tellus* instead of to the *Journal of Meteorology.* Unfortunately, printing problems delayed its appearance until June 1949. The Charney-Eliassen article, which detailed the effectiveness of numerical weather prediction techniques for both theory development and possible weather prediction applications (the previously discussed forecast of 500 mb heights), was extremely important. Although it did not have an opening section written by von Neumann, as Rossby had suggested, it was the first paper to give positive, concrete results from the Meteorology Project. Rossby thought that the article presented a new era in meteorology where 'feeling' would be repressed in favor of computation." It also presented a more heuristic view because the team was willing to try an approach and see how it worked with actual data before determining the next move.[487] The theorists would be pleased that feeling was taking a back seat, at last, to more mathematical techniques. And the applied meteorologists would be glad to see that they were trying the newly developed theory on actual data for a reality check before moving on.

Rossby, being Rossby, had more pots to stir than just the Meteorology Project. In early May 1949, he invited a veritable who's who of European meteorology to Stockholm for a week of talks, discussions (and probably

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[487] Ibid. The paper: Charney and Eliassen, "A Numerical Method for Predicting the Perturbations of the Middle Latitude Westerlies."

arguments) on climatic fluctuations and other related problems – followed by the customary excursion to the Swedish countryside that accompanied all Rossby inspired meetings.[488] However, never passing up a chance to trumpet the results of the Meteorology Project's numerical weather prediction work to the unbelievers (or at least the skeptical), Rossby took time to brief the assemblage on the Charney-Eliassen paper and its predicted pressure profile diagrams.

Considering the current state of the Project and some concerns of his own and others that had been tossed around, Rossby wanted to know if it was possible to make a numerical study of stationary wave patterns since it could very well inform climatological questions. If it was, did Charney intend to attack it? If not, Rossby had a couple of young meteorologists ready to work on such a project, but Rossby did not want to start work on something that was already spoken for by either the Meteorology Project itself or the Weather Bureau.

When contemplating how best to put applied meteorology on a firmer, i.e., more scientific footing, Rossby had come to the conclusion that the Meteorology Project needed to "push the present approach" to its ultimate conclusion – an operational forecast – to swing the doubters into the NWP camp. He reported that the British meteorologist Reginald C. Sutcliffe, for instance, had wanted to know why they could not just simply extrapolate troughs and ridges from one day to the next based on past displacements, since they would come up with the same answer.

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[488] Participating in the meeting were Sutcliffe (U.K.), van Mieghem (Belgium), Godske and Høiland (Norway), Lysgaard and Andersen (Denmark), Palmén and Keräuen (Finland), Faegn (Norway - botanist), Ahlmann (Sweden), Kuo (China), Rex and Hutchinson (U.S. Navy officers), Namias (USWB), and Reuter (Austria).

To Rossby, the question was not even important. However, he thought it best if Charney knew the opposition – the very conservative people in the meteorology community who were going to be difficult to convince of the efficacy and desirability of numerical weather prediction. As a postscript, he recommended the Charney take a look at prominent Swedish oceanographer V. W. Ekman's current theory because it was similar to what Charney was trying to do more generally in the atmosphere.[489]

By the late spring of 1949, Rossby was spending most of his time in Stockholm. However, he continued to maintain close ties by mail with his Chicago colleagues – in particular *Journal of Meteorology* editor George Platzman. Writing to Platzman at the close of the climate change conference, Rossby wanted to sound out his friend on a number of ideas related to the Meteorology Project in general and the production of operational forecasts by numerical means in particular. He also knew that whatever he wrote to Platzman would ultimately get to Charney and thereby double the impact of his message. Rossby wrote Platzman that the Charney/Eliassen methodology – using a barotropic atmosphere with barotropic convergence and assuming a constant zonal current to develop a method of integration – seemed "extraordinarily promising." Their introduction of the frictional force had prevented resonance difficulties. Although Charney and Eliassen argued that the method had practical applications as it was because of the amazing agreement between the observed and computed results, Rossby thought

[489] Rossby to Charney, 8 May 1949 (Charney papers, B14, F459).

the method would break down when faced with rapidly deepening systems. However, he had shown the results to visiting Weather Bureau meteorologist Jerome Namias – Rossby's former co-worker and student at MIT. Namias had subsequently written to his brother-in-law Harry Wexler at the Weather Bureau, asking him to contact Charney and to try the method in the forecast section. Although Rossby himself would have preferred a more rigorous test carried out in an academic environment, he considered a Weather Bureau test to be better than no test at all. Additionally, Rossby wanted to find a way to expand the work. He thought the calculations should be done for all mid-latitude latitudes (i.e., 35°N, 45°N, and 55°N), not just 45°N, and for several different values of zonal currents. If the values were then computed for different altitudes, they could be pieced together. He also wondered if equal success would be reached by looking at moving versus stationary systems. If the method worked for moving systems, then there was a possibility of getting out of the "horrible subjectivity" that characterized "all or most" forecasting. Again, Rossby had pointed out that he had a couple of young men in Stockholm who could be employed on such a task, but claimed he did not want to "interfere" in U.S. efforts. However, if they could work cooperatively and obtain results faster, that seemed to make the most sense.[490]

The leaders of research schools are constantly on the move – making sure that their acolytes' works are spread far and wide, and keeping up the flow of advice and moral support. Rossby's aggressive sharing of the contents of the

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[490] Rossby to Platzman, 8 May 1949 (Charney papers, B14, F460).

Charney-Eliassen paper with the "younger people" (graduate students and post-doctoral researchers, presumably) and the many visitors who came through the Meteorological Institute, prior to its actual publication, is an important example of this trait. He was sure to let Charney know that everyone who had seen it was very interested, and Rossby was most anxious to get it on the street. (Unfortunately it had been held up by the printer, but those problems seemed to have been solved.)[491] Charney, for his part, genuinely appreciated the support, moral and technical, that he routinely received from his mentor Rossby. He acknowledged his debt to Rossby's influence when he wrote that if he and Eliassen had been successful in the application of the heuristic method it was because Rossby had taught them very well.

Rossby, recall, had also pushed the idea of an operational test in letters to both Charney and Platzman. By sharing the paper with Namias and suggesting the same operational test to him, Rossby was counting on Namias to make the same proposal to Wexler, and then execute the project once he returned to the Weather Bureau in the late spring. In doing so, Charney's hand was all but forced. Rossby knew that the chronically short-handed Weather Bureau would want to try out an objective technique that provided a prediction in half an hour. Once the possibility was out in the open, the Weather Bureau would be clamoring to try it out no matter how reluctant Charney and Eliassen might be to subject their new method to an operational test. After all, if it did not work as advertised, it could set back their

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[491] Rossby to Charney, 19 May 1949 (Charney papers, B14, F459).

efforts to convince the applied meteorologists of the ultimate usefulness of numerical techniques. Furthermore, Charney did not want the practical applications to overshadow the important theoretical results: topography and friction were only minor players in short-range atmospheric variations. Thus, the models could ignore them in short-range predictions. Perhaps even more important, the results indicated that non-linear barotropic models would lead to both practical and theoretically valuable outcomes.[492]

The testing which Namias had proposed to Wexler, was actually carried out at Princeton by Weather Bureau personnel. Charney and Eliassen went to Bureau Headquarters and delivered two lectures about their method. The response was so enthusiastic that Charney decided he needed to temper his remarks so as to not inadvertently oversell it. The testing team was to make pressure profile forecasts for several different weather types using the Charney-Eliassen equations for periods of two to seven days; some for even longer periods. Charney noted that the Rossby formula[493] yielded reasonable values for the displacements of the 5-day mean pressure systems and therefore might be able to approximate dynamically possible flow patterns. These tests would force them to concentrate on longer period phenomena for which new physical factors would have to be taken into account.

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492 Progress Report of the Meteorology Group at the IAS, 1 July 1948 – 30 June 1949 (Charney papers, B9, F304).
493 For a Rossby (or planetary) wave, the wave speed c is given by the equation $c = U - \beta L^2/4\pi^2$ where U is the mean westerly flow, beta is the Rossby parameter and L is the wavelength. See G. W. Platzman, "The Rossby Wave," *Quarterly Journal of the Royal Meteorological Society* 94 (1986): 225-248 for a discussion of Rossby waves in the atmosphere and ocean.

Charney thought, despite Rossby's desires, that there would be problems in extending their results. He and Eliassen had gotten around some problems by assuming a basic flow that was constant and zonal. But other assumptions did not seem terribly natural, and he was not sure how they would approach the problem. Friction turned out to be unimportant for short period weather changes, but was very important for stationary motions. Also, friction implied that system energy would be dissipated. However, the model could not dissipate energy unless it was provided with an energy source. The team decided to provide the energy by assuming the zonal current was maintained by a thermally driven meridional circulation. They also neglected any energy loss through perturbation flow.

Rossby had brought up the idea that baroclinicity of the atmosphere could be introduced as an external factor in the two-dimensional model. Charney was intrigued by this and would keep it in mind. He was also pleased that the plans he had already made to investigate the stationary perturbation pattern were similar to Rossby's ideas on the subject. However, Charney had not planned to determine the stationary pattern for a jet stream flow by superimposing the patterns for different parts of the stream and was delighted to let Rossby's Stockholm group work on that.[494]

Since his arrival in Princeton, Charney had enjoyed the relative luxury of a stable personnel situation characterized by an ideal mix of disciplinary expertise. The Project members had been simultaneously focused on two basic problems: the

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[494] Charney to Rossby, 24 May 1949 (Charney Papers, B14, F459).

physical description of the atmosphere such that prognoses could be developed from available data, and the invention of a computing technique that would be both stable and responsive only to meteorologically significant motions in the atmosphere. Eliassen had been particularly helpful in both the physical and mathematical aspects of the project.[495] But Jacob Bjerknes had offered Eliassen a position at UCLA for the remaining months of his one year leave of absence from the Norwegian Meteorological Office, and Eliassen – desiring to take his bride to another part of the country for awhile – accepted. Charney acknowledged that while that was good for Eliassen, he was going to miss him very much. The Project would need a replacement with the same combination of theoretical and practical experience possessed by Eliassen. Charney found it in Ragnar Fjörtoft (b. 1913) of the Norwegian Meteorological Institute.[496] Fjörtoft had worked with Charney during the latter's year in Norway and Charney knew that he would be able to fit nicely into the program they had underway in Princeton. Still looking abroad, Charney also invited British meteorologist Eric Eady. Having heard nothing, he asked Rossby to check on Eady and find out what his plans were. And as concerned Rossby himself joining the Meteorology Project, no decision had yet been forthcoming from the Institute of Advanced Study.[497]

Rossby had played a major role in the Meteorology Project since its conception, but he had done so from off-site. Charney, longing for the days in

[495] Charney to Bjerknes, 21 April 1949 (Charney papers, B4, F120).
[496] For an interview with Fjörtoft, see Hessam Taba, "Professor R. Fjørtoft," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1988), 361-370.
[497] Charney to Rossby, 24 May 1949 (Charney papers, B14, F459).

Chicago when he and Rossby would spend long hours discussing meteorological problems, very much wanted Rossby to come to Princeton and be the on-site director of the Project. Instead of waiting for letters to make their way back and forth across the Atlantic, they could sit down over strong, Swedish-style coffee and discuss their ideas face-to-face. Rossby's close ties with Reichelderfer at the Weather Bureau, the academic meteorologists associated with the MIT and Chicago programs he had founded, and geophysicists from many disciplines throughout Europe, would aid in bringing in outside advice and support when they needed it. And so Charney had encouraged von Neumann to bring Rossby to Princeton.

After several months of negotiations within the Institute, von Neumann, with the concurrence of IAS Director Robert Oppenheimer, extended an invitation to Rossby to become a member of the Institute for two years. As von Neumann noted, Rossby "more than anyone else" was responsible for getting the theoretical meteorology work started at the IAS under the auspices of the ONR contract. Although they had had a slow start, the pace had accelerated since Charney's arrival (and due to the "advice and encouragement" of Rossby). The computing machine was now due to be operational in early 1950. Von Neumann wrote, "[Our] work will need your advice, and to the extent to which this is feasible, your presence, more than ever. In fact, we embarked upon it originally in the inarticulate but definite hope, that we should have your help and guidance, when we had developed the necessary tools, and come really to grips with the main problem."

The proposed two year contract would give Rossby sufficient time to carry out a "well-rounded" portion of the research program in theoretical meteorology and would give all of them enough time to come to some agreement about their "mutual possibilities and plans."[498] In other words, the Institute was not willing to bring Rossby on contractually for too long a period in case it did not work out. Given the very strong personalities that were involved in this Project, von Neumann and Oppenheimer may have been reluctant to bring in yet another for an extended stay.

Charney, trying to convince his mentor Rossby to come to Princeton, worked to put the negotiations in a positive light. The permanent members of the Institute had had little or no knowledge of Rossby and his qualifications. Therefore, the process had been slowed down while von Neumann brought his work to their attention. Charney reported that after this "indoctrination" the decision to extend the offer was unanimous. Although von Neumann wanted the appointment to be permanent, it was thought best to leave the decision about the future open to both Rossby and the Institute leaders once they had become better acquainted. Von Neumann believed that "the going would be smoothed if an engagement period were allowed to precede the marriage." Rossby would become the head of the Meteorology Project, but would be able to maintain contact with the University of Chicago and other meteorological institutions. In his final pitch to persuade Rossby to come, Charney wrote "You know as well as I that meteorologists will continue to be frustrated at every turn as long as they lack the mathematical ability to carry

[498] Von Neumann to Rossby, 13 June 1949 (Charney papers, B14, F459).

their physical arguments to their logical conclusions. I would like nothing better than to be able to help you to break this dam."[499]

In the midst of this invitation, catastrophe struck the University of Chicago's Meteorology Department. Chairman Horace Byers, who had received his Ph.D. under Rossby at MIT in the late 1930s and had played a crucial role in establishing the new department at Chicago, suffered a heart attack. Rossby, still with ties to Chicago, volunteered to fill in for the summer before returning to Sweden to teach in the fall. Despite establishing a meteorology program in Stockholm, Rossby's intent in the summer of 1949 was to return to the United States permanently in early 1950.[500]

The Princeton group, anticipating the day when the new IAS computer would be ready, had, by early summer 1949, started to make tentative computations on desk calculators which would lead to computed 500 mb wind forecasts. Meanwhile down in Washington, the Weather Bureau's Joseph Smagorinsky (b. 1924) was coordinating the future Princeton-based tests on the Charney-Eliassen numerical forecasting method.[501] Since it was too difficult to do the needed calculations as the month unfolded, i.e., in close to real time, Smagorinsky and the Extended Forecast Section elected to use the data from June 1949 with a "normal" value for the June zonal current. Smagorinsky was convinced that the "normal" value would be unreliable because it would not be related to the actual

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[499] Charney to Rossby, 15 June 1949 (Charney papers, B14, F459).
[500] Alan T. Waterman (ONR) to von Neumann, 17 June 1949 (Charney papers, B14, F459).
[501] For an interview with Smagorinsky, see Hessam Taba, "Professor J. Smagorinsky," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1988): 149-162.

conditions for June 1949. However, if they were calculating forecasts each day in June as the month unfolded, they could not very well use the June 1949 average, which would not be available until the end of the month. Smagorinsky thought they ought to try the calculations both ways: with the "normal" and with the calculated average values, and then compare the results. If the "normal" values provided a solution that was close enough to that provided by the actual average values, then in the future it would be possible to use the "normal" values under operational circumstances. The Weather Bureau's analysis center eventually hoped to produce 36-hour prognoses, but in the near-term would work on 24-hour prognoses instead. Due to time and manpower constraints, they had not gotten as far as the Extended Forecast group. There was a tremendous amount of preliminary compiling, plotting, and analyzing of data which needed to be accomplished before making the actual forecasts.[502]

ENIAC TO THE RESCUE

By late summer 1949, the Meteorology Project's future progress depended upon the availability of an electronic computer for trial runs of their models. Unfortunately, the IAS computer was not ready. So as not to lose time while waiting for the new computer, Weather Bureau Chief Reichelderfer intervened with Army Ordnance on behalf of the Meteorology Project.

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[502] J. Smagorinsky to Charney and Eliassen, 28 July 1949 (Charney papers, B14, F473).

Army Ordnance controlled the ENIAC – the special purpose electronic computer designed and built at the Moore School of the University of Pennsylvania to solve ballistic problems. Unlike the new IAS machine, the ENIAC was not a fully stored-program machine. "Programs" had to be broken into small pieces and set on switches. Therefore, writing a program for the machine would take a considerable amount of time, as would actually putting it into the machine. However, a slow electronic computer was still much faster than a hand calculator, so ENIAC was the best alternative available to the Meteorology Project.

In September 1949, Reichelderfer formally requested the use of ENIAC (located at the Aberdeen Proving Grounds, Maryland) for the Meteorology Project's barotropic model run. He supported his request by pointing out that the work being done at IAS on numerical forecasting was of the utmost importance to both civilian and military interests. The war effort had led to many more surface and upper air observations – increasing the degree of complexity – which needed to be folded in to both meteorological analyses and prognoses. The electronic computer was, therefore, the best hope for helping to sort out all of these data and solve the relevant equations which govern atmospheric behavior. Although the Office of Naval Research had initially been the only fiscal supporter, they had now been joined by the Air Force. Military leaders had become increasingly aware that this was a project of strategic and tactical importance.

By using hand-calculators and human "computers," the Meteorology Project had already successfully predicted the 24-hour change in the 500 mb height field when treated as a one-dimensional problem. Attempts to solve the two-dimensional problem by hand had been abandoned because it was just too labor intensive. If the ENIAC were available, von Neumann estimated it could make a 24-hour forecast, calculated in one-hour time-steps, in six to eight hours. To check forecast accuracy, they needed to make daily forecasts over a two week period. Reichelderfer asked: could Army Ordnance make the ENIAC available for a two week period sometime in the upcoming three or four months for the first application of electronic computing to the weather forecasting problem?[503]

The Army swiftly responded. Noting "the importance of weather forecasting for military and civilian purposes," Army Ordnance granted permission to use the ENIAC for a two week period on a not-to-interfere basis.[504] Reichelderfer forwarded this response to von Neumann, adding his "personal appreciation of the interest" shown by von Neumann in the "solution of the meteorological problem."[505] Von Neumann warmly welcomed this development writing Reichelderfer that he felt "obliged" to him for his assistance in obtaining ENIAC and "this additional manifestation" of his interest in the work of the Meteorology Project. Von Neumann would be at Aberdeen for a meeting of the Scientific Advisory Committee of the Ballistic Research Laboratories in late October and would then make detailed arrangements for ENIAC.[506] Considering his close ties with ONR and the support he was receiving from the Air Force, it is

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503 Reichelderfer to MGEN E. S. Hughes, 22 September 1949 (von Neumann papers, B15, F2).
504 MGEN H. B. Sayler to Reichelderfer, 29 September 1949 (von Neumann papers, B15, F2).
505 Reichelderfer to von Neumann, undated, ca. early October 1949 (von Neumann papers, B15, F2).
506 Von Neumann to Reichelderfer, 5 October 1949 (von Neumann papers, B15, F2).

curious that von Neumann sought Reichelderfer's help – and not one of his military contacts – in securing the use of the ENIAC. Yet past accounts stressing that von Neumann rather than Reichelderfer made this crucial connection are in error.[507]

Meanwhile, back in Stockholm, Rossby was trying to get some information from Platzman and Charney. He decided to take an "I'll fill you in, if you'll fill me in" approach when he wrote to both of them in October 1949. He reported that visiting Belgian meteorologist Jacques Van Miegham had been giving lectures on hydrodynamic instability. Virtually all the others working at the Institute were addressing the formation and impact of "blocking" systems, i.e., high pressure systems that remain in place and "block" the movement of atmospheric waves. Navy Commander Daniel F. Rex – who three years earlier had arranged the ONR funding for the Meteorology Project – was now working on his Ph.D. with Rossby in Stockholm. His research focused on a comparison of blocking situations in Europe and North America. Rossby very much wanted an update on their non-linear attack on the forecasting problem. Finnish meteorologist Erik Palmén (b. 1898), who had switched from astronomy to meteorology at the Ph.D. level, was still skeptical. He was very much concerned that there was not enough

[507] Both Aspray, *John von Neumann*, 142, and Nebeker, *Calculating the Weather*, 146, credit von Neumann with making the arrangements to use ENIAC. The letters from the von Neumann collection referenced clearly show that Reichelderfer obtained the permission to use ENIAC. Von Neumann dealt with Aberdeen on using the ENIAC for the Meteorology Project's initial run only after Reichelderfer had cleared the way.

connection with real atmospheric conditions. But Rossby thought it was healthy to win over skeptics, and he wanted more ammunition with which to do so.[508]

Smagorinsky continued to work on making forecasts for the standard latitudes (35°N, 45°N, and 55°N). Having overcome some initial difficulties, he anticipated having significant results before Charney's two-dimensional, non-linear model was ready.[509] Charney invited Smagorinsky to join the Princeton group in the three weeks preceding the ENIAC test runs so that he could become acquainted with the planning and coding. He suggested that Smagorinsky obtain a publication on coding so he would be somewhat familiar with the process before he arrived.[510] Smagorinsky was already trying to do this. He had also identified a possible scenario for the test run: the period starting November 22, 1949, when a block suddenly appeared in the North Atlantic. It persisted until December 1st, then weakened and diminished to the point that, by the time he penned his letter a week later, it had all but disappeared. Smagorinsky thought this two-week period would adequately test the two-dimensional finite-amplitude forecasting technique and would allow a good test to see how the barotropic model handled the blocking scenario.[511]

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[508] Rossby to Platzman and Charney, 9 October 1949 (Charney papers, B14, F459). Palmén, who had known Rossby since 1930 when they met in Bergen, joined Rossby in Chicago for two years starting in 1946. Once Rossby left for Stockholm, he still visited Chicago to work for extended periods of time. For an interview with Palmén see Hessam Taba, *The "Bulletin" Interviews* (Geneva: The Secretariat of the World Meteorological Organization, 1988), 25-34.
[509] J. Smagorinsky to Charney, 24 October 1949 (Charney papers, B14, F473).
[510] Charney to J. Smagorinsky, 5 December 1949 (Charney papers, B14, F473).
[511] J. Smagorinsky to Charney, 8 December 1946 (Charney papers, B14, F473).

In order to become conversant with ENIAC, Charney had had to become, as he put it, a "servant" to the machine. Writing to Rossby four days before Christmas 1949, Charney informed his mentor that until such time as they had settled on one method and there were limited choices on a path, it made the most sense for the person formulating the problem to be the one doing the programming and coding, or at a minimum, to maintain close supervision over the efforts. He also made this prophecy: "[in the future] the training of every meteorologist will include a course in numerical methods and the use of large-scale computing instruments."

Preparations for the ENIAC "expedition" required writing a computation scheme, and then translating that scheme into machine code. However, the scheme had to fit the machine – which only had an internal memory of fifteen ten-digit numbers. Their model would require the storage of as many numbers as there were grid points. And there were many more than fifteen grid points. Therefore, they were going to have to use punch cards as external memory. A grateful Charney gave credit to von Neumann for his help in this regard.

Von Neumann and his wife, Klari, were also helping with the machine coding. For ENIAC, that meant one instruction for every ENIAC operation (in the order in which it occurred) had to be set on dials on the machine. The tentative plan was to go to Aberdeen and try it out in February 1950. Because Platzman had made major contribution to the coding, Charney hoped to entice him to come from Chicago to Princeton.[512]

The ENIAC plans were slowly coming together, but once again personnel problems loomed. Several people Charney thought were en route to Princeton had decided not to come. Charney was disappointed and surprised when von Neumann and Oppenheimer informed him that Rossby had declined the Princeton offer.[513] Likewise, von Neumann was disappointed that Eady would not be joining the Meteorology Project in the summer as previously planned. He still hoped that Eady would join them once the IAS computer was operational.[514]

Technical, as well as personnel, problems contributed to delays in preparation. Von Neumann continued to handle the numerical analysis issues, playing the principal role in solving problems impacting the computations. He found a solution method adaptable to the ENIAC, established the nature of boundary conditions, worked on stability criteria, and determined the influence of energy being propagated into the forecast area.[515] The ENIAC trials, scheduled for February, slipped into March. Charney, writing to Rossby, asked if he would be available to come to Aberdeen in mid-March, after they had made one complete computation, so he could look over the results. Charney really wanted to see Rossby and suggested that he might be able to get von Neumann to Aberdeen at the

[512]Charney to Rossby, 21 December 1949 (Charney papers, B14, F459
[513] Charney to Rossby, 21 December 1949 (Charney papers, B14, F459).
[514] Von Neumann to E. T. Eady, 9 January 1950 (Charney papers, B16, F517).
[515] The Institute for Advanced Study, The Meteorology Group, Progress Report July 1, 1949 – June 30, 1950, Contract No. N-6-ori-139 (Charney papers, B9, F304).

same time.[516] Rossby, then in Chicago, was eager to join them and tentatively scheduled himself to arrive on the 13th. However, he still reserved the right to change his plans at the last minute.[517]

Noon on Sunday March 5, 1950 was the starting point of the 33 day odyssey which became a major milestone in the history of the atmospheric sciences: the first computer-assisted attempt to forecast the weather by numerical means. The full-time "expedition" members (Charney, Fjörtoft, Freeman, J. Smagorinsky, and Platzman) ran three eight-hour shifts, five days/week for five weeks. The team, with the aid of ENIAC, produced two twelve-hour and four twenty-four forecasts from initial observed data. (Although Nebeker claims that von Neumann was not in Aberdeen, a photograph of the primary group and "visiting dignitaries" from this expedition shows von Neumann.[518]) As one participant later recalled, they encountered myriad difficulties with the ENIAC itself. On average, it could run error free for only a few hours and then took many hours to repair – with 20 accumulators of 550 vacuum tubes each, there were many potential problems. The card-punch equipment was also prone to failure, although its mean-time-to-failure rate was not nearly as high as ENIAC's. Coding problems surfaced. The original two-week window stretched to five weeks to allow time for

[516] Charney to Rossby, 21 February 1950 (Charney papers, B14, F459).
[517] Rossby to Charney, 23 February 1950 (Charney papers, B14, F459).
[518] Nebeker, *Calculating the Weather*, 146. Philip Duncan Thompson, "A History of Numerical Weather Prediction in the United States," *BAMS* 7 (1983): 760 (Figure 1).

additional runs.[519] Reichelderfer made sure that the Aberdeen staff knew of his appreciation for "making this historic occasion possible."[520]

Once the expedition was over, Charney and Fjörtoft spent almost three months analyzing the results.[521] By June 1950, Charney had something definite to report to Rossby. First of all, forecast accuracy varied greatly day to day. For example, ironically the forecast for January 5th was "quite bad" since that day had been chosen because the meteorological situation appeared to satisfy the equivalent-barotropic requirement well. On the other hand, the January 31st forecast, chosen because it did not satisfy the equivalent-barotropic requirement, turned out to be quite good.

Much of the time spent analyzing the results had been focused on determining which errors were due to the model itself, and which were due to the computational method. An early discovery was that the chosen spatial grid size was much too big. Therefore, the model underestimated the vorticity in small intense systems and exaggerated the change in anticyclonic vorticity. As a result, the absolute vorticity of a particle near the absolute vorticity minimum decreased. Charney thought that the reason the January 5th forecast was so poor was due to the large grid size, which failed to pick up the tendency contrast through the cyclone located in the United States. In effect, the cyclone was between grid points, and

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519 George W. Platzman, "The ENIAC Computations of 1950 – Gateway to Numerical Weather Prediction," *BAMS* 60 (1979): 302-312. According to Platzman, Rossby was present for part of one day during the entire five-week run.

520 Reichelderfer to Col. Alden P. Taber, 10 April 1950 (von Neumann papers, B15, F2).

521 The results were published a year later in J. G. Charney, R. Fjörtoft, and J. von Neumann, "Numerical Integration of the Barotropic Vorticity Equation," *Tellus* 3 (1951): 248-257.

therefore there was no way to identify the pressure differences in a horizontal cross-section of the system. Some of the forecast failure was due to the model which significantly tilted the U.S. cyclone with height and led to increased deepening.[522]

While the grid spacing was too large, the time increment – one hour – turned out to be too small. They were able to increase the time increment to three hours. Unlike having a too-large grid size, the too-small time interval did not lead to errors. It just led to fewer forecasts because as the time step decreased, the amount of time required for the forecast calculations increased.

The model also broke down between January 30th and 31st. Although the forecast for the 31st was good, there had actually been a strong height decrease on the 30th which the model had not predicted at all. Since these forecasts occurred on consecutive days, the scale of motion had to be the same. Therefore, Charney and Fjörtoft would have expected any computation errors to be the same also. When they analyzed the maps they found that vorticity had been added in a small area and could only have been due to baroclinic development. This result confirmed Rossby's contention that strongly baroclinic changes take place intermittently, and in-between the changes are pseudo-barotropic.

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[522] Given a low pressure center (cyclone) at the surface, if the corresponding heights with increased altitude are low and to the left of the surface feature (in the Northern Hemisphere), i.e., the system "tilts" left with height, the resulting advection (movement into an area due to the flow pattern) of positive vorticity (spin) causes the system to deepen, i.e., the pressure drops even further at the surface.

Based on these results, Charney thought that the relatively simple barotropic model was useful for its ability to explain the atmosphere qualitatively, but they would need to move to the more complex baroclinic model in order to produce the quantitative forecasts needed by operational meteorologists. The IAS machine under construction would have a 1024 word internal memory which would still restrict the kind of baroclinic model they would be able to run unless arrangements could be made for external memory. The Project members would try to run a partially advective model proposed by Fjörtoft (that was similar to Sutcliffe's advective model) on the same January 30th scenario to see if it would handle the baroclinic development of the system. They also planned to work on a primitive equation model as well as theoretical wave and vortex barotropic models even though Charney was personally more interested in pursuing baroclinic models.

Despite these important advances in model development and understanding, personnel problems again threatened the Meteorology Project. Sharing Rossby's philosophy on personnel, Charney thought that it was a much better idea to invite those who understood the problem and were glad to be part of a cooperative solution than those only willing to till their "own furrow." In Charney's opinion, Eliassen and Fjörtoft were solidly in the first group, while Hunt and Queney were just as firmly in the latter. The initial members of the Meteorology Project had all been in the latter group – that was why initial progress had been so slow. Eliassen and Fjörtoft also had the advantage of possessing a broad knowledge of synoptic meteorology which had prevented the Project from "degenerating into mathematical sterility" – a concern that had haunted Charney from the beginning. Freeman and Fjörtoft were both leaving in July – Freeman for Chicago – but Fjörtoft would return in September for just four months. No personnel additions had been planned for after that time which, Charney noted, was "bad." Rossby had suggested that British theoretical meteorologist Thomas V. Davies might be a good addition and Charney was sounding him out. Charney asked Rossby what he thought about bringing Namias in for a few months. Smagorinsky would be coming part-time to take Freeman's place, but they still needed two others at "the idea level." He asked Rossby to give them some advice and come himself if possible.[523]

Having considered the errors in the ENIAC results, Rossby was convinced of the importance of understanding the actual atmospheric processes that had been at work when the computations failed. Examining the maps with his protégés Swede Bert Bolin (b. 1925) and U.S. Navy officer Dan Rex, all were amazed at how good the results were, given the model's simplicity.[524] Rossby thought that the errors were probably due to cyclonic vorticity aloft that was not readily apparent. He proposed having Bolin conduct a synoptic study of the meteorological scenario because he was both an excellent analyst and a sufficiently well-trained theoretician to be able to come to a theoretically sound conclusion. Bolin was leaving for Chicago within a month and would be available to work with the Meteorology

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[523] Charney to Rossby, 5 June 1950 (Charney papers, B14, F459).

[524] Bert Bolin went on to have a distinguished career in geophysics, both in Sweden and internationally, after assuming the leadership of the International Meteorological Institute in Stockholm following Rossby's death in 1957. For a interview, see Hessam Taba, "The *Bulletin* Interviews: Professor B. Bolin," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1988), 393-403.

Project starting in early 1951. Rossby recommended Bolin because, like fellow Scandinavians Eliassen and Fjörtoft, he possessed both the desired synoptic and theoretical backgrounds that Charney needed. Desiring to get the results out quickly in the meteorological literature, Rossby strongly encouraged Charney and Fjörtoft to write a note for *Tellus* about the ENIAC calculations.[525]

Charney had also expected Platzman, who had played an important role both in the ENIAC preparations and expedition, to make the move from Chicago to Princeton. Unfortunately for the Meteorology Project, Chicago's Meteorology Department Chairman, Horace Byers, was pressuring Platzman to remain in Chicago. Ultimately, Platzman decided to stay in Chicago, and so informed a disappointed Charney. Platzman wanted to turn Chicago into a research center that would increase meteorology's standing as a science. As Platzman put it, "I feel that academic meteorology in this country is still suffering from the trade-school blues" – despite efforts by the American Meteorological Society and its leaders, most of whom worked in the academic sector, to turn meteorology into a professional discipline given the same respect accorded engineering and the physical sciences. He was hoping that with Dave Fultz (1921-2002) – another Rossby protégé who had earned his Ph.D. at Chicago in 1947 and was known for his "dishpan experiments" which provided tangible evidence of how the jet stream moved in the atmosphere – he could bring new blood into the field and raise the level of research. He wanted his students to look at programs like the Meteorology Project,

[525] C.-G. Rossby to J. Charney, 13 June 1950 (Charney papers, B14, F459).

and thus be inspired to pursue theoretical research.[526] As it turned out, Platzman was able to join the Princeton group for the fall quarter. Charney, was for his part, encouraged by the opportunity to work closely with the Chicago group.[527]

With the relative success of the two-dimensional barotropic model, the Project members continued on their heuristic path, setting their sights on three-dimensional models and the possibility of using the primitive equations in numerical weather prediction. In mid-summer 1950, Charney was investigating the upper boundary conditions for a three-dimensional model. Fjörtoft was making a theoretical study of a simplified three-dimensional model and, with Charney, was studying the statistical-mechanical properties of two-dimensional incompressible flows. The Princeton computer, which, when proposed, was supposed to be operational in mid-1948, was still not ready in mid-1950. Although the Meteorology Project members had no idea when the computer might be ready, as soon as it was, they planned to perform additional integrations of the barotropic equations with a smaller space lattice, and to begin programming the problem in three-dimensions. Most importantly, as before, they would continue their efforts to formulate a theory for the physical nature of atmospheric motion.[528]

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[526] G. Platzman to J. Charney, 18 June 1950 (Charney papers, B14, F451). For an obituary on Dave Fultz, see "Dave Fultz, meteorologist, 1921-2002," issued by the University of Chicago News Office (http://www-news.uchicago.edu/releases/02/020731.fultz.shtml).
[527] Charney to Platzman, 22 June 1950 (Charney papers, B14, F451).
[528] The Institute for Advanced Study, The Meteorology Group, Progress Report July 1, 1949 – June 30, 1950, Contract No. N-6-ori-139 (Charney papers, B9, F304).

While the Princeton team continued their work, another team was setting up shop in Cambridge, Massachusetts. This one was under the direction of former Meteorology Project member Phil Thompson.

RESEARCH IN A PARALLEL ATMOSPHERE

Since he was an active duty Air Force officer, Phil Thompson knew he would not be able to stay with the Meteorology Project indefinitely. By March 1948, after being on board for a little over a year, he was already negotiating for his next assignment. Thompson wanted to make sure that he stayed in Princeton at least until the end of the year so he could see his efforts "blossom and be assured that it will later bear fruit." One possible option was to become the military attaché in Norway, because he would be able to use that position to obtain, directly and indirectly, valuable technical information about meteorology and other more general hydrodynamical topics. The Bjerknes dynasty in Norway had established the Bergen school in the early part of the twentieth century and the Scandinavians continued to maintain a strong research program despite the fact that many of their students had settled in the United States as result of the war. Being in Norway would allow Thompson to tap into this research network and pass information of interest back to the United States. However, what he really wanted to do was to return to the Meteorology Project.[529]

[529] Thompson to Joe Fletcher, 8 March 1948 (Thompson papers, Fletcher, J.).

In August 1948, AAF Captain Albert Trakowski of the Air Force's Geophysics Research Division at the Watson Laboratories, Red Bank, New Jersey asked Thompson to recommend someone to lead the Division's Meteorology Section. Thompson, in response, managed to eliminate everyone who was a meteorologist from contention. Charney was out because "it's not his style" and he had no talent for administration. The same applied to Rossby's protégé from Chicago, Victor Starr, then with MIT's Meteorology Department. German-trained Bernhard Haurwitz – first invited to the United States by Rossby to join the MIT staff as a visiting faculty member in 1932, and who had recently become chairman of NYU's Meteorology Department – knew the literature, but was "old school" and lacked administrative ability and inclination. Indeed, Thompson could think of "no one in the field of meteorology" to whom he would entrust an organization dedicated to fundamental research in meteorology. He therefore suggested that Trakowski look for a geophysicist, especially one who had dealt with a hydrodynamical field like oceanography and had a "casual" interest in meteorology. According to Thompson,

> If I may make a couple of general remarks, meteorology seems to repel those sensitive souls who like mathematical or otherwise rigorously scientific treatment and draws a great many fools who, undaunted by the extreme difficulty of the problem, feel that metaphysical methods will yield results where scientific methods have not – whereas, several other fields of geophysics, better developed and more scientific (simply because the problems involved are less formidable), attract many able men.[530]

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[530] Thompson to A. Trakowski, 16 October 1948 (Thompson papers, Trakowski, Albert 1948). Underlining in the original.

This was not the only time that Thompson would comment on the abilities of prominent meteorologists in an attempt to have them removed from consideration for administrative positions In March 1949, when the now Geophysics Research Directorate (GRD) had been looking for a director for the new Cambridge Labs, Thompson provided his opinion on the candidacy of Rossby even after hearing that the position was going to someone else. While claiming that he respected Rossby as a "man and scholar," he then went on to strenuously argue that Rossby would not be appropriate because he was "too narrow" (despite the breadth of this published works – a completely ludicrous claim), that his ability to bring in new talent had three stages (in Thompson's words: "seduction … incest … degeneration") which totally stifled any original work, that his lack of attention to detail was counterproductive. Lastly, he cast aspersions on Rossby's moral character by requesting the recipient of this letter to check out the "dossier labeled 'Rossby, C.-G." for evidence of his "modus operandi." Continuing, "Furthermore, for your sake as well as ours, may I suggest that you look at these things from the viewpoint of those who will judge you?" Thompson closed with, "In short, it is my opinion that we should aggressively seek candidates potentially less dangerous to us, perhaps sacrificing a little of the vitality which we like in Rossby, and soon."[531] What should we conclude about Thompson, and his aims? All of the meteorologists dispatched by Thompson as being unacceptable leaders of the GRD's

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[531] Thompson to Paul Worthman, 1 March 1949 (Thompson papers, Correspondence Worthman, P., 1949-1950).

meteorological section were among the most prominent, creative meteorologists of their day. Any of them would have been able to fully understand – and criticize – Thompson's research projects. Perhaps the real problem with all of these distinguished meteorologists was that they had the potential to thwart the fundamentally insecure Thompson's ambitions.

In fact, no one was hired for the GRD Directorship. It was still in the hands of Air Force officers in the middle of 1950 when Thompson drafted yet another letter (stamped "Restricted"). The never mailed draft, complained that MIT's Henry G. Houghton (acting as the chairman of the Geophysics Panel of the Scientific Advisory Board) had urged the appointment of Norwegian meteorologist Sverre Petterssen to the post. According to Thompson, this was unhappy news for the Cambridge Lab personnel who had viewed Petterssen's candidacy as "unlikely." In all likelihood, only Thompson was upset. He did not consider that this move was within Houghton's purview and maintained that because of his efforts, morale had been significantly lowered. He pointed out that Petterssen was not a U.S. citizen and that some "competent" person would need to testify that there was not a U.S. citizen available with equivalent qualifications. As with Rossby, Petterssen had already pulled his name from consideration – as had numerous others during the previous 18 months of the Laboratory's existence. Despite that, Thompson felt it necessary to derail the appointment.[532] These letters, which provide insight into Thompson's apparent disdain for meteorologists who might be placed in authority

[532] ERG to Commanding General, AMC (drafted by Thompson, not sent), ca. June 1950 (Thompson papers, Correspondence Sverre Petterssen 1949-1950).

over him, are important. This attitude colored his efforts in pursuing numerical weather prediction as a strictly Air Force project of which he, Thompson, would have total control. And as we will see in Chapter 8, when the time came to go "operational" with NWP, Thompson's drive for absolute control would lead to an explosive confrontation and many bad feelings among the others who had worked for years developing the theory and techniques with the Meteorology Project.

Having eliminated everyone else from consideration, Thompson himself was offered and accepted the position as Chief, Atmospheric Analysis Laboratory (AAL) of the Air Force Cambridge Laboratories. Thompson wanted this move kept "fairly dark" so as to "reduce friction – and the heat engendered thereby – in a system whose viscosity is admittedly quite high." He had discussed his planned move and new assignment at the end of the year with von Neumann, who was supportive and wanted to maintain his association with Thompson.[533] However, Thompson would not be in Red Bank. The entire Geophysics Research Division was moving from New Jersey to Cambridge, Massachusetts in November 1948.

Reporting on this move to Wexler in late November, Thompson had disingenuously claimed that he was completely surprised by this offer, but that under the circumstances it was the best career move for him. He was sorry to leave Princeton and "a group of meteorologists whom I esteem as highly as any in the world," but that he would only be "divorced" physically from the Project. He planned for his new lab to complement the work being done in Princeton and

[533] Thompson to A. Trakowski, 16 October 1948 (Thompson papers, Trakowski, Albert 1948).

looked forward to closer cooperation between the research services of the Weather Bureau and the Air Force's Geophysics Research Division.[534] The latter was an interesting comment, considering that Thompson was on record as considering the Weather Bureau to have been of little help to the Meteorology Project.[535]

Once Thompson took over directorship of the AAL, the Air Force (in point of fact, Thompson himself, lacking institutional competition) started to develop its own extensive plan for meteorological research. In the "Proposed Plan of Air Force Sponsored Research on Meteorology and Closely Allied Sciences" written by Thompson and issued in January 1949, the Air Force staked its claim to being the savior of modern meteorology. It needed to synthesize a unified theory of meteorology because previous efforts in geophysics research had been uncoordinated and results were lacking. This would all change with the Air Force leading a "frontal attack" on the problem. Lest higher authority misconstrue this to be "pure science" unrelated to operational needs, the plan noted the "diverse operational requirements" of both the Air Weather Service and the other USAF agencies concerned with atomic energy, electronics and guided missiles. Thompson's plan was high-tech and high cost. Specialized rockets for upper level observations and photography topped the list. There were not nearly enough trained people in the field, so graduate fellowships needed to be awarded to entice students

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[534] Thompson to H. Wexler, 24 November 1948 (Thompson papers, Wexler, H. 1946-1950).
[535] Thompson to J. Fletcher, 4 June 1948 (Thompson papers, Fletcher, J.).

away from more glamorous physical sciences.[536] Clearly the Air Force had big plans for meteorological research and they sprang from the mind of Philip D. Thompson.

Thompson was thinking well beyond his own corner of the atmosphere to an overall military research policy. Technical information was essential to the military services, and the Air Force was a large consumer of those results. Thompson thought it would be more economical if the Air Force conducted its own research. The problem was that in order to be "scientific" it had to be reproducible by a team that had not conducted the original research. If the research were classified (either because the data, the technique, or the results were classified), that meant another similarly skilled team had to have appropriate clearances. If the research had to remain under military "control" due to security issues, then it would be in a different category than research which could simply be military-"supported." The Air Force would be required to carry out its own research plan as a matter of survival. To do that successfully, it needed to have officers who were also scientists and researchers. They would bridge the divide between the military side, which controlled the funds and assigned the problems to be solved, and the civilian side, which would spend many years working on longer-term projects.

[536] P. D. Thompson, "Proposed Plan of Air Force Sponsored Research in Meteorology and Closely Allied Sciences,"10 January 1947 (Thompson papers, Folder of the same name).

Although these officers would not stay in the research arena forever, their training, he argued, would make them valuable as intelligence officers and attachés later.[537]

Thompson's outfit, while still striving to work on meteorological theory and numerical weather prediction models, thus was very much concerned with making contributions to short- and long-range military objectives. It is also clear that the AAL wanted to fashion itself as the equivalent to the Meteorology Project at IAS. Just like the Princeton group, this Cambridge clone planned to operate under the guiding vision that a "unified theory of atmospheric motion would, of course, provide the perfect instrument for predicting weather." Unlike its Princeton counterpart, however, this laboratory had a full-fledged weather detachment (with probably 10-15 enlisted and one weather officer assigned) operating within its confines for the purpose of handling and storing large amounts of data. The synoptic analysis section took care of its analysis and verification needs.[538] This gave it somewhat of an advantage over the Meteorology Project which had no in-house analysis capability and had to rely on the Weather Bureau analysis section in Washington, D.C. to provide that function.

In addition to the continuation of Thompson's investigations of mathematical-physical methods (as he called them instead of numerical weather prediction, although they were functionally identical), other projects which the AAL considered included work being spearheaded by three German meteorologists

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[537] P. D. Thompson to J. Fletcher, ca. 1949 (Thompson papers, Cambridge GRD-AFCRL, 1947-1950).

[538] P. D. Thompson, "Historical Notes Atmospheric Analysis Laboratory," July 1949 (Thompson papers, Correspondence, etc., 1947-1950, Cambridge GRD, AFCRL).

who had moved to the United States after the war: on standing waves in the lee of mountain ranges [Joachim Küttner (b. 1909)], turbulence and diffusion [Heinz Lettau, (b. 1909)], and autocorrelation methods to long-range prediction of temperature and precipitation (Eberhard Wahl).[539] The Laboratory was also sponsoring research projects in university meteorology departments, in particular at UCLA and MIT, as well as being a co-sponsor with ONR of the Meteorology Project at IAS.[540]

By early April 1949, Thompson's group was preparing to produce forecasts for the one-dimensional "quasi-barotropic" atmosphere. The plan was to attack a large-scale problem and compare the numerical results with those of a standard forecast.[541] By the end of the month, Thompson reported that the results were looking "rather promising."[542]

Little of Thompson's correspondence for the rest of 1949 and into 1950 discusses his numerical work. What is most interesting is the *lack* of correspondence on the ENIAC expedition. For all his protestations of remaining involved with the Meteorology Project, Thompson was certainly not involved with the ENIAC runs, nor does his correspondence with Harry Wexler even bring the subject up. This is an important matter, for prior histories of the Princeton Meteorology Project have told a distinctly different story. Historian Frederik

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[539] For an interview with Küttner, see Hessam Taba, "Dr. J. P. Kuettner," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1997), 35-48.

[540]P. D. Thompson, "Historical Notes Atmospheric Analysis Laboratory," July 1949 (Thompson papers, Correspondence, etc., 1947-1950, Cambridge GRD, AFCRL).

[541] Thompson to von Neumann, 11 April 1949 (Thompson papers, von Neumann, J. 1948-1949).

[542] Thompson to R. G. Stone, 25 April 1949 (Thompson papers, Stone Robert G.).

Nebeker has written that Thompson traveled to Princeton every "two to three weeks" to keep in contact with the group. This information was based on an interview with Thompson conducted by historian William Aspray. Yet this also differs from an account Thompson gave in a separate 1987 interview. In that account, Thompson said he went to Princeton "fairly frequently" and that he and Dan Rex "of ONR" were monitoring the Meteorology Project, since they were providing the money.[543] However, during this period Rex was in Stockholm studying with Rossby. If Thompson were spending that much time in Princeton, it is odd that those trips do not appear in any of the (voluminous) correspondence left by either Thompson or Charney, or, for that matter, von Neumann. Neither does Thompson mention this close liaison in either of his written accounts of the early days of NWP.[544] More likely, the oral accounts of "frequent visits" on which past accounts were based were more likely revisionist spin, an effort to tie himself more closely to the Princeton group after the fact.

By the fall 1950, Thompson reported that the AAL had been doing a lot of work on the theory of large-scale motions and on verifying the corresponding prognostic equations. Thompson himself had incorporated some baroclinic effects into his large-scale motion theory and had found analytical solutions of the two-dimensional prognostic equations. He was working to write up this theoretical

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[543] Nebeker, *Calculating the Weather*, 154; P. D. Thompson, "An Interview with Philip Thompson" (conducted by William Aspray, 5 December 1986), Charles Babbage Institute, Minneapolis, MN; AMS TRIP "Interview of Philip D. Thompson, 15-16 December 1987" (conducted by Joseph Tribbia and Akira Kasahara), NCAR Archives, Boulder, CO (p. 16).

[544] Thompson, "A History of Numerical Weather Prediction"; Philip Thompson, "The Maturing of the Science," *BAMS* 68 (1987):631-637.

development for publication in a GRD publication by early 1951. The Lab, however, had moved on to advancing the work on two-dimensional models and was concentrating on extending one-dimensional theory.[545] Thompson had complained about the lack of "airing out" of ideas while he was with the Meteorology Project in Princeton.[546] Thus, it is interesting that he was publishing his new research in the "gray" literature without virtue of peer review. Although some research papers produced in military laboratories contained findings that were subject to security concerns (which created problems in getting military releases to publish), Thompson's work did not fall under that category. Therefore, publishing these findings in an Air Force publication seems to indicate that he was reluctant to have his work come under the formal scrutiny of his fellow meteorologists. His paper, "Notes on the Theory of Large-Scale Disturbances in Atmospheric Flow with Applications to Numerical Weather Prediction," was not published until July 1952.[547]

ONWARD AND UPWARD

The two years which elapsed between Charney's arrival at the Meteorology Project and the first ENIAC expedition (which finally showed the real promise of numerical weather prediction) were busy ones for the international network that

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[545] Thompson to J. Freeman, 20 November 1950 (Thompson papers, Freeman, John C. !950).
[546] Thompson to A. Trakowski, 16 August 1948 (Thompson papers, Correspondence Trakowski, Albert 1948).
[547] Philip D. Thompson, "Notes on the Theory of Large-Scale Disturbances in Atmospheric Flow with Applications to Numerical Weather Prediction," *Geophysical Research Papers No. 16* (July 1952).

was supporting this theoretical effort. While past accounts have focused on the computer aspects of this project, in fact it was the meteorologists who controlled and advanced it. Meteorologists from academia, the Weather Bureau, and the military – in Princeton, Stockholm, Chicago, New York, Cambridge, and Washington, D.C. – were busy trying to develop a workable, viable theory of atmospheric motion which could then be programmed into von Neumann's new machine. While von Neumann's efforts vis-à-vis computational issues and hardware development should not be underplayed, neither could he have successfully completed this effort without the leadership of Jule Charney on-site and of Carl-Gustav Rossby off-site, but intellectually and spiritually present. It was the continuous presence of the Scandinavian tag-team members – Eliassen, Fjörtoft, and at the end of this period Bolin – which provided the atmospheric reality necessary to keep the Project from being separated from the physical world and tumbling into a mathematical fantasy land.

In stark contrast, the Atmospheric Analysis Laboratory's numerical weather prediction section, headed by the intelligent, ambitious, and self-promoting Phil Thompson, basically had just Thompson himself. Without the direct path to publication sources enjoyed by his Princeton counterparts, Thompson's influence in numerical weather prediction was limited to the Air Force. In truth, he was only a minor player in Rossby's research school, and consequently had very little intellectual impact on the Meteorology Project after his departure in late 1948.

In the next period, both groups would seek to develop even better models as they moved toward operational NWP. Not to be outdone, another NWP group was forming up: Rossby's International Meteorological Institute in Stockholm.

CHAPTER 7
CREATING A REALISTIC ATMOSPHERE (1950-1952)

The success of the first ENIAC "expedition" gave the Meteorology Project a much needed boost. With their simplified model providing output that at least *looked* meteorological, it was time for the team to turn their sights to ratcheting up the complexity of each of the models which would follow. In an elaborate system of "guess and check," they would introduce new techniques and new variables, do a test run, compare the output against what was considered to be a valid description of the meteorological situation, and then "verify" the model.

The meteorology team would be developing more sophisticated models while the computer itself was still being completed. Von Neumann and his people on the computer side of the house would provide the assistance with the actual mathematical solutions. The question remained: how would they know if the model produced realistic output or not? While they were modeling, others would have to be generating a subjective (hand drawn) product against which they would compare their model output. No two subjectively created meteorological maps were (or are now) *ever* exactly the same. If they were going to decide on atmospheric reality by comparing their objective, computer-made chart against a subjective, man-made one, how would they know that either one of them was right? Which reality would they choose?

The Meteorology Project at IAS was not the only group attacking the numerical weather prediction problem. Rossby's Meteorology Institute was providing analysis support to the Princeton group, training meteorologists (military, Weather Bureau, and academics) from the United States as well as European meteorologists, and preparing its own eventual launch into NWP with assistance from the Swedish Air Force. Thompson's group at the Air Force's Geophysical Research Directorate in Cambridge was hard at work on Thompson's own models. Besides pushing his own model development, Thompson encouraged the expansion of Air Force research funding in meteorology into the European theater as well as into the Meteorology Project itself.

While there appeared, at first at least, to be no overt competition – no overt "race to the finish" among these groups – in the scientific arena those who reach the goal first often take all the credit. For numerical weather prediction there would be not only a theoretical goal of describing how the atmosphere worked, but an operational goal of using the computer to create usable forecasts for the military services and the Weather Bureau. Would the interagency and international cooperation which had been a feature of the developmental phase extend to the operational phase, or would one "winner" attempt to corner the market providing numerical forecasting tools to the nation? Who would get to make the choice?

OUT OF THE BLOCKS

The Princeton team members were not the only ones encouraged by the ENIAC expedition results. In August 1950, Reichelderfer asked Wexler about the Meteorology Project's progress since the team had returned home from Aberdeen. When would numerical weather prediction techniques reach the stage "that we should start a full operating unit here [WB Headquarters] to make use of the results of the Princeton research," he wrote, so that the Weather Bureau could request sufficient funds to cover the cost in that year's budget?[548] Unfortunately for Reichelderfer, it was going to be a few more years before there would be any operational output useful to the Weather Bureau forecasters.

Indeed, the ENIAC results took quite a while to sort out. Platzman (in Chicago) was in close contact with Charney about the model he was developing with Fjörtoft. While he was providing extensive feedback on the finer points of the model, Platzman was concerned that the addition of more and more physical influences into the quasi-geostrophic model was leading to a very complex computation – more complex than an all-out attack on the primitive equation might be. He wondered if they ought to just abandon the other models and start in on the primitive equations at once.[549] Of course the problems with the primitive equations were all too clear – instability being the primary one.

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[548] Reichelderfer to H. Wexler, 9 August 1950 (Wexler papers,B5, F1950-4).

[549] Platzman to Charney, 9 July 1950 (Charney papers, B14, F451). A system is called *quasi-geostrophic* if it evolves slowly in time compared to the rotation period of the earth, is of long length, and experiences limited vertical motion. *Glossary*: 609.

Given the importance of the ENIAC results for future modeling development, Charney was eager to get them published and circulating within the wider geophysics community. The Meteorology Project had been working on numerical weather prediction for four years, and the ENIAC results presented the first concrete examples of computer-produced pressure surface forecasts. That was an important achievement in its own right, but even more important, it would give those promoting NWP more ammunition against the skeptics among both theoretical and practical meteorologists. This had become especially critical because Charney's 1949 paper "On a Physical Basis for Numerical Prediction of Large-Scale Motions in the Atmosphere" – which had laid out the theoretical underpinnings for the Meteorology Project, had come under attack. British meteorologist R. S. Scorer of Imperial College, had written a letter to the *Journal of Meteorology* questioning a number of points in Charney's paper – in particular the adequacy of the barotropic model.[550] Charney had to craft a response that would diffuse the influence of Scorer's comments on the assumptions underlying numerical weather prediction. He asked for Rossby's reaction to Scorer's criticisms and to the draft of his response. Charney also inquired: could Rossby publish the new Charney-Fjörtoft-von Neumann paper in the November edition of *Tellus*?[551]

Rossby dealt swiftly with the publication issue, assuring Charney that the article would make the cut-off date *if* he actually mailed it in. The Scorer letter was

[550] R. S. Scorer, "Correspondence: Atmospheric Signal Velocity," *Journal of Meteorology* 8 (1951): 68-69. J. Charney, "On a Physical Basis for Numerical Prediction of Large-Scale Motions in the Atmosphere," *Journal of Meteorology* 6 (1949): 371-385.
[551] Charney to Rossby, 25 September 1950 (Charney papers, B14, F459).

more troubling to Rossby because it contained, in his view, numerous unproven assertions which Scorer then used to support his contention that the barotropic model should be abandoned. Particularly irritating was his contention that since the model did not contain all the details of the atmosphere including the effects of gravity waves (which Charney had pointed out only complicated and did not inform the solution), meteorologists should leave numerical methods behind. Rossby saw the influence of British meteorologist, and NWP opponent, Reginald C. Sutcliffe in Scorer's attack. Of primary importance, Scorer had not taken into account the difference between linearized and non-linearized wave theories. Even given that problem, linearized theory could give rise to sharp wave fronts which do not readily vanish. Rossby had found Charney's response to be too "soft," particularly since Scorer was being used as "[David] Brunt's hatchet man." Brunt (like Sutcliffe, a dynamicist with Imperial College, London) also opposed numerical weather prediction methods. Rossby was particularly disturbed by Charney's "concession" that it was a "happy coincidence" that the barotropic model approximated the atmosphere sufficiently to be of practical forecasting use. On the contrary, Rossby found that the approximation was not a happy coincidence at all, but a dynamic requirement based on the physical nature of the atmosphere.[552]

Charney's greatest fear was that those associated with the Meteorology Project, Rossby included, would oversell the practical forecasting aspects of the barotropic model in particular, and numerical weather prediction in general. This

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[552] Rossby to Charney, 28 September 1950 (Charney papers, B14, F459).

was the reason, he explained, that his response had emphasized the word "practical" in relation to forecasting. At that point in time, numerical output could not measure up to the skills of a solid forecaster. Charney did not want to give critics like Scorer any more ammunition in their war against numerical weather prediction. He did agree that the barotropic model was important in theory development. Charney's original response to Scorer, by his own account, had been "scorching" and he had toned it down considerably so as not to appear defensive.[553] Rossby and Charney both needed to consider questions of timing and presentation to the wider meteorological community if numerical weather prediction were to become credible to the majority of discipline practitioners accustomed to operating in a much more subjective manner.

Indeed, Charney reached a very wide audience of meteorologists and weather enthusiasts in the United States when the paper he had presented at the AMS Annual Meeting in January 1950, on the progress of research in dynamic meteorology, was published in the AMS *Bulletin* in September. Charney pointed out that numerical weather techniques were not likely to lead to a "revolutionary increase in forecasting accuracy." What he did anticipate was that numerical techniques could lead to more accurate forecasts of large-scale patterns in the upper atmosphere. The steering flows derived from these patterns could then be used to make forecasts of smaller-scale frontal systems that produce weather. While it was certainly possible that numerical techniques could someday be used to predict

[553] Charney to Rossby, 2 October 1950 (Charney papers, B14, F459).

cloud formation and resultant precipitation, existing techniques could not yet deal with that level of complexity.[554] Although the audience for his talk would have been relatively small, the print version was sure to reach several thousand people interested in the status of numerical weather prediction. Charney's carefully crafted remarks were planned to inform, without overselling capabilities. This was important to his, and Rossby's, overall strategy for winning skeptics over to the new meteorology.

By late October, Charney was working to finish his *Tellus* article before the 1 November deadline. He checked with von Neumann (in Los Alamos) to clarify the derivation of the computational stability criterion. Charney also advised von Neumann that they had completed some calculations of the three-dimensional field by using the three-dimensional model on a scenario which had produced a large error in the barotropic forecast. These new calculations explained the error, so Charney and Fjörtoft planned to include the results in the article. Since they had finished coding the barotropic problem for the Princeton machine, Charney wrote that he was ready to receive his promised reward. In particular, he was waiting for "that dinner that Klari" (referring to von Neumann's wife) had promised him. However, things must have been just a little hectic in Princeton. Charney admitted he would probably need Klari's meal in "liquid form" by the time they returned.[555]

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[554] Jule G. Charney, "Progress in Dynamic Meteorology," *BAMS* 31 (1950): 231-236.
[555] Charney to von Neumann, 24 October 1950 (Charney papers, B15, F516).

Indeed, at the end of December, Charney and Fjörtoft were still working on the manuscript and hoping to get it in the next *Tellus* volume.[556]

During the fall of 1950, Wexler visited the Princeton team. He reported to Reichelderfer that they had tested the barotropic model and were now moving on to the baroclinic version. The new computer was "almost ready" to go, but no one really knew when it would be ready for testing. Most of the Project members seemed to think that they would be ready to give it a test run by the end of the calendar year.[557]

The Weather Bureau was not the only agency with a stake in the Meteorology Project inquiring about its status as the year came to a close. Navy Aerologist and ONR staff member Lieutenant Max Eaton was interested too. The Meteorology Project's contract with ONR was due to expire at the end of May 1951. Eaton needed to discuss a contract extension with von Neumann and Charney. However, it appeared that the terms of the contract might change in the near future. Thompson, leading his own group in Cambridge, had contacted Eaton about the Meteorology Project contract. It appeared that the Air Force was taking an interest in providing funding, probably to increase their stake in the outcome.[558] Undoubtedly Eaton wanted to discuss with von Neumann the possibility of sharing

[556] Charney to von Neumann, 21 December 1950 (Charney papers, B16, F517).
[557] Wexler and Hermann B. Wobus to Reichelderfer, 24 October 1950 (Wexler papers, B5, F1950).
[558] Max A. Eaton to von Neumann, 27 November 1950 with annotations (von Neumann papers, B15, F2). This is the first indication in the archival evidence that the Air Force wanted to take part in the funding of the Meteorology Project.

the funding burden with the Air Force before ONR made the decision to accept the offer.

Rossby spent almost two months with the Meteorology Project as a consultant in early 1951. His presence would be particularly important given the decisions that needed to be made on future model development. The Project members expected that the IAS computer would be operational within a short period of time, and they wanted to consult with Rossby on how to approach this move to the new machine. In advance of his arrival in Princeton, Rossby had written to Charney and expressed his hope that Charney's long awaited corrected manuscript on the ENIAC results was en route to Sweden. Exercising his prerogative as a research school leader, Rossby suggested that Charney build up some interest in *Tellus*, which, he pointed out, was not "just another journal." As the editor, Rossby had three goals for *Tellus*: that it should be truly international (especially given the problems of the Cold War), mirror what was happening in geophysics, and serve as a bridge between the geophysical sciences. Since its international aspect was critical to spreading new meteorological ideas around the globe, this remained Rossby's greatest concern. He reminded Charney that he needed everyone's cooperation to make it happen. Rossby made clear he was not trying to compete with the *Journal of Meteorology* (which he had established during his time as AMS President) or any other research journal. On the contrary, the purpose of *Tellus* was to make known the research that was being published in other journals. He hoped that von Neumann's contribution to the ENIAC article would draw in more mathematicians to geophysical research.[559]

Rossby always drew members of his far-flung research school to him like a magnet. His presence in Princeton thus provided the perfect opportunity for his former doctoral student, Harry Wexler, to pay a visit from the Weather Bureau. Not only did he want to visit with Rossby, he wanted to assess the progress of the Project. Model preparation, as was then taking place, invariably depended upon analysis assistance from the Weather Bureau. Wexler needed to find out what the Project would need and when they would need it. He reported back to Reichelderfer that the Project members had decided to forecast, as much as possible, in real-time based on numerically determined 2-3 hour pressure tendencies aloft. Therefore, they would be needing twice daily standard pressure maps from the Weather Bureau-Air Force-Navy (WBAN) analysis center – a joint activity manned by all three weather services for the production of analyses and prognoses. The WBAN would mail the charts daily for the duration of the experiments, through the end of February. The Weather Bureau would also provide a copy of the 700 mb (3000 meters/10,000 feet) prognoses for comparison purposes.[560] These maps would be in addition to the ones that the Weather Bureau had been providing all along. The Meteorology Project did not have sufficient manpower to take care of its own plotting and analysis. Thus, it depended upon the WBAN to provide this service,

[559] Rossby to Charney, 7 January 1951 (Charney papers, B14, F459).
[560] H. Wexler to Reichelderfer, 23 January 1951 (Wexler papers, B5, F1951-5).

for which Charney was extremely grateful.[561] The Weather Bureau never had any money to provide to the Meteorology Project, but even when short of manpower its leadership invariably scraped together enough man-hours to get these necessary tasks done.

Wexler was not the only one drawn to Rossby. Leaders of the Research and Development branch of the Air Weather Service were interested, and wanted to provide funds. In his discussions with Rossby and von Neumann, AF representative Jim Fisk wanted to know: was Rossby planning to stay in Princeton, and what kind of expansion did they anticipate for the Meteorology Project? Rossby was tied up with his new Meteorological Institute in Stockholm, Fisk learned. Von Neumann and Rossby both envisioned only a modest expansion over the next six to twelve months. What they really wanted was some kind of "quasi-permanency" for Charney. The Air Force quickly responded that the Research and Development Branch could offer them a 5-year contract which could be extended each year so that the Project always operated under five years of guaranteed funding. Further conversations between the Air Weather Service's Colonel Benjamin G. Holzman (R&D), Rossby, and Charney, resulted in an offer to fund any expansion of the Project including sub-contracts to Rossby's Stockholm group. Rossby thought that such funding for Stockholm by the United States government would be fully justified because both Rossby and Eliassen could "profitably" spend several months each year in Princeton. In addition, other Europeans who were possible

[561] Charney to H. Wexler, 18 January 1951; H. Wexler to Charney, 22 January 1951 (Wexler papers, B5, F1951-5).

additions to the Princeton team could be hired by Rossby in Stockholm and evaluated on their potential usefulness before sending them to Princeton for a year or two.

Developments were progressing so quickly that not all interested parties understood these new arrangements. Von Neumann was not at all sure if Alan Waterman (ONR) had been briefed on the increased Air Force interest or the possibilities for a sub-contract to Rossby. Charney and von Neumann had previously briefed the Meteorological Panel of the Research and Development Board on 1 February 1951. The panel concurred in the concept that the Meteorology Project should receive additional support "within reason" whenever it was needed. However, it had not known of the possible five-year contract or extension to Stockholm at that time.[562] Their interest in finding additional funding for the Meteorology Project might have been lessened had they known about the Air Force's offer for funding on a five-year cycle.

In mid-February, Wexler visited Princeton again – this time accompanied by Jerome Namias of the Extended Forecast Section and J. R. Fulks of the WBAN Analysis Center. Holzman, of the Air Weather Service, met them there. Their purpose was to see how Charney's group was using the WBAN prepared maps and how those techniques might be applied by the Weather Bureau in advance of computer operations. The Princeton group used the charts to compute (by hand) the

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[562] Von Neumann to Oppenheimer, 27 February 1951 (von Neumann papers, B15, F2). In the letter, Platzman is referred to as "H. Platzman" instead of "George Platzman." For more about the role of Holzman in Air Weather Service R&D circles, see Fuller, *Thor's Legions*.

instantaneous height tendency field at 500 mb (5500 meters/18,000 feet) for the barotropic model and a special-case baroclinic model. The group members then created a 24-hour prognosis from the tendency fields for comparison with observed data. Situations with distinct, large amplitude troughs and ridges showed a good match between the prognosis and the observed values. The four- to six-hour computation time would be significantly reduced when the computer was ready. In the meantime, Charney suggested that the WBAN perform the same procedure as an aid to their analysis and prognosis routine. But, Wexler noted, they would need to find additional manpower before it would be possible. Fulks promised to look into it.[563]

Here again, an old problem re-emerged: Fulks determined that since the personnel situation was at an "irreducible minimum," the WBAN analysis center would need to find more people.[564] Following further consideration, Fulks recommended that the Weather Bureau supply one additional person to work on the tendency computations and then ask the military services to provide one additional enlisted man each. To this end, he suggested submitting a proposal to a subcommittee of the Joint Meteorological Committee (Joint Chiefs of Staff) which served as the venue for coordinating the provision of meteorological services among the weather services.[565] However, when this request finally came before the committee, the Air Force and Navy declined to participate. Why? Because the

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563 Wexler, Namias, G. Brier, J. R. Fulks to Reichelderfer, 21 February 1951 (Wexler papers, B5, F1951-5).
564 Fulks to Reichelderfer, 26 February 1951 (Wexler papers, B5, F1951-2).
565 Fulks to Reichelderfer, 12 March 1951 (Wexler papers, B5, F1951-2).

WBAN was not supposed to be used for "research projects." Therefore, a Weather Bureau employee not associated with the WBAN was detailed to perform these calculations.[566] The fact of the matter was that this was not exactly a "research project" and it was coming from within the organization itself. Indeed, the Air Force and the Navy were both sponsoring the Meteorology Project. Apparently, decision-makers in those organizations considered any technique that was still being "checked-out" to be in the "research mode," and therefore not worthy of support with additional personnel. Those supporting meteorological research and those in the operational arms in both military services were probably not keeping posted on the other's issues.

At the end of March 1951, the group submitted a progress report that encompassed the period since 1 July 1950. It did not include the ENIAC expedition results since Charney, Fjörtoft, and von Neumann had published them in *Tellus*.[567] However, the Project members did report that the computations had been sufficiently good to confirm in their minds the usefulness of numerical methods for weather forecasting. The coarseness of the grid used on ENIAC had most probably obscured model errors. They intended to rerun the computations with a finer mesh grid on the new Princeton computer when it became available. The new machine would require a modified program and the recalculation of the stability criteria – both tasks completed by von Neumann. Platzman and Margaret Smagorinsky (wife of Joseph Smagorinsky) completed the coding in December and were ready to run

[566] I. R. Tannehill, Memorandum for the Record, 9 July 1951 (Wexler papers, B5, F1951-2).
[567] Charney, Fjörtoft, and von Neumann, "Numerical Integration."

on the new computer as soon as it came on-line. They intended to run forecasts from actual and idealized situations, e.g., various wave and vortex models.

The ENIAC runs made clear that conventional map analysis and subjective interpolation of grid values were not good enough for numerical computation. Therefore, Platzman and Margaret Smagorinsky would work on an objective analysis technique and the accompanying programming for the new machine.

The group had also spent a considerable amount of time "re-tooling" for three-dimensional forecasts. The two-dimensional models were helpful for understanding the physical workings of the atmosphere; they had limited applicability for forecasting. The three-dimensional quasi-geostrophic equations were difficult for the machine to handle, so Fjörtoft worked on a simpler model which used an advective assumption. This model was superior to the barotropic version. Bolin's three dimensional calculations for the two-day scenario yielded better 500 mb height tendencies, but produced poorer surface tendencies than the two-dimensional model.

Like all experimental models, eventually even the advective three-dimensional model would prove to be inadequate. In anticipation of that event, the Project members were already turning toward the solution of the general three-dimensional equations. Von Neumann and Charney were investigating relaxation techniques to use with these equations and had successfully tried a modified Liebman method on the two-dimensional model.[568]

The team members calculated initial 500 mb height tendencies for a 12 hourly sequence of weather maps to test the applicability of the two-dimensional barotropic and simplified two-dimensional baroclinic model. The initial tendencies were good indicators of a successful finite forecast. They then compared the calculated tendencies with the observed 24-hour change in a total of 25 cases. They wanted to develop *a priori* criteria for determining the success or failure of a barotropic forecast and hoped that other research groups would also move into this type of work.

The Princeton group was still looking ahead to long-range forecasting even though they were having enough problems with the 24-hour forecasts. However, team members were making progress. They had found that quasi-stationary perturbations which appeared on seasonal 500 mb charts might be explained by the movement of air over large-scale topographic surface irregularities. If true, this discovery could possibly help with extended weather prediction. If it turned out that the mean perturbation was just an orographic response to a mean zonal current varying with latitude alone, then the problem would be reduced to a simpler one of predicting variations in the current. Joseph Smagorinsky worked on this project.

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[568] A *relaxation method* is one where successive approximations are used starting with an initial guess. The error of the guess is reduced by an improved guess until the error falls below some preassigned value. A *Liebman (or sequential) relaxation* converges to a solution more rapidly because each new guess is used immediately in computing the new guess of an adjacent point in the grid. For a discussion of relaxation techniques, see G. J. Haltiner, *Numerical Weather Prediction* (New York: John Wiley and Sons, Inc., 1971), 111-115.

Other projects included ones by Thomas V. Davies (visiting from the U.K.) to investigate the process of formation of "cols" and cut-off vortices in the atmosphere, and by Charney on a statistical theory of barotropic vortex motion.[569] Even without the new computer, the Project had continued to expand its efforts beyond its first simple barotropic model. Project members would not be able to go much further though unless they could actually test their ideas out on a computer.

ENIAC TO THE RESCUE – AGAIN

In April 1951, the Meteorology Project members were ready to run their models. However, the new computer was still not complete. Once again Weather Bureau leaders made the initial overtures to Army Ordnance to arrange for computer time. Quickly they passed the affirmative response to von Neumann.[570] But while the first ENIAC expedition had been "free," this time the Project would have to pay for the computer time. The Bureau of Ordnance budget no longer had sufficient funds to cover runs for outside agencies.[571] Von Neumann worked on getting the funds from ONR to pay this cost.[572] The Ordnance Department was willing to make the computer available for about 10 days before the first of June. However, changes to ENIAC's converter code since the first expedition meant some code changes of

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[569] The Institute for Advanced Study, The Meteorology Group, Progress Report, July 1, 1950 to March 31, 1951, Contract No. N-6-ori-139 (Charney papers, B9, F304). A "*col*" is the intersection of a trough (an area of low pressure) and a ridge (an area of high pressure) on a surface pressure map. A *vortex* (plural, vortices) is an area characterized by vorticity or spin.
[570] Wexler to L. S. Dederick, 6 April 1951; copy with note from Wexler to von Neumann, same date (Wexler papers, B5, F1951-5).
[571] Ray A. Pillivant to Reichelderfer, 11 July 1950 (Wexler papers, B5, 1950).
[572] Von Neumann to H. Wexler, 9 April 1951 (Wexler papers, B5, F1951-5).

their own for the Princeton group. The computer charges: $800 for each 24-hour day of use.[573] The time it took to arrange a transfer of funds from the Office of Naval Research to the Bureau of Ordnance delayed the arrangements. And ONR wanted a summary of the work to be performed on the ENIAC.[574] If von Neumann and the Project members were frustrated by this delay, they did not so indicate in their correspondence.

Charney and Rossby were jointly planning the second set of ENIAC runs, Von Neumann advised ONR. They planned to study the development of idealized two-dimensional flows using three model types: wave, vortex, and jet. Charney and Rossby wanted to find out the kind of interaction that would occur between a wave of finite amplitude and a mean zonal flow – in one case stable and in another unstable for small perturbations. They also wanted to know how kinetic energy from a solitary wave dispersed depending on whether it was connected to an unstable or stable zonal current. Concerning vortices, Charney and Rossby wanted to find out how a finite isolated vortex moved "in a mean zonal flow with a prescribed vorticity gradient" and if a change would be produced in the mean zonal circulation. They also wanted to find out how a vortex "street" consisting of alternating vortices moved in a zonal current regardless of the outside circulation and what kinds, if any, of non-linear interactions occurred. Since a finite disturbance in zonal flow could not be completely damped out, Charney and Rossby wanted to see to what type of motion it would tend. Finally, they wanted to

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[573] W. Barkley Fritz to von Neumann, 10 April 1951 (von Neumann papers, B12, F3).
[574] Eaton to von Neumann, 10 May 1951 (von Neumann papers, B12, F3).

know at what point an atmospheric jet changed from one that widens gradually to one that widens rapidly. All of these items were related to problems of the zonal westerlies and the development of blocks.[575] In other words, their ENIAC runs were not simply to test a model in order to see whether it worked or not. Rossby and Charney were investigating theoretical ideas.

The ENIAC run was scheduled for the end of May. Project members were trying to determine what models they would run in their pursuit of theoretical aims. Platzman opined from Chicago that due to the limited machine time, they would need to select the parameters very carefully and have two or even three models ready to go. What the team members decided to run also depended on their objectives for this round. Platzman thought they should be investigating the behavior of certain flow patterns. That meant that they needed to know what flow patterns and behaviors to investigate before they went into the computations.[576]

Back in Princeton, the team members had not made any final decisions about model choices. However, they had decided their overarching aim: to study the non-linear interactions of systems to gain a better understanding of observed

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[575] Von Neumann to M. A. Eaton, 16 May 1951 (von Neumann papers, B12, F3). Nebeker, *Calculating the Weather,* does not mention Rossby's involvement with the examination of the first ENIAC expedition results or planning for the second ENIAC expedition. This is a crucial omission given Rossby's de-facto leadership of the Meteorology Project from Chicago, Stockholm, or wherever he happened to be. *Zonal flow* indicates air that is moving more or less parallel to lines of latitude. Prevailing zonal flow is from the west (westerlies) between 30 and 60 degrees of latitude; from the east (easterlies) between 0 and 30 degrees, and between 60 degrees and the pole. When a *blocking high* forms it interrupts this zonal flow. Since the air cannot just stop moving, it takes on a meridional flow up one side and down the other of the high pressure area. A *vortex street* is a manifestation of two parallel rows of alternately placed vortices along the wake of an obstacle in a fluid. *Glossary,* 824.

[576] Platzman to Charney and Bolin, 2 May 1951 (Charney papers, B14, F451).

atmospheric phenomena. To that end, Charney posed a series of questions about the behavior of perturbations in zonal flow as related to perturbation wavelength. Although the group agreed with Rossby and Kuo that they should try to discover properties of the general circulation that could be explained barotropically, they thought that the models they had been using were too restricted and possibly physically unsound.

The Project's computer access, in any case, would be limited. Given the allotted three week time slot and assuming 90% efficiency, Charney hoped for a minimum of fourteen integrations and a maximum of twenty-four. Therefore, he did not see how they could run an actual situation – and was not sure that it was appropriate for them to do so at all.

Army Ordnance had promised them the ENIAC starting May 28th, but it soon appeared team members could not have it until after "Decoration Day," i.e., Memorial Day. That meant they would be working at Aberdeen until after 15 June. Charney and Bolin were unable to stay that long, so Charney suggested that Platzman, former team member John Freeman, and Platzman's Ph.D. student Norman Phillips (b. 1923) all come down to Aberdeen from Chicago for the runs.[577] Since too many people might lead to confusion, Charney proposed that they rotate in and out of Aberdeen from Princeton. Those left in Princeton would work on analyzing the results or revising the models as required. Norma Gilbarg,

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[577] Phillips joined the Project permanently (except for a swap with Bolin when he went to Sweden) in the fall of 1951. He did extensive work in modeling the general circulation and moved to MIT with Charney at the conclusion of the Project. See Hessam Taba, "Professor Norman A. Phillips," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1997), 329-338.

one of the Project's "computers," would perform hand calculations in Aberdeen. Joseph Smagorinsky and Davies would also be available to work in Aberdeen.[578]

After discussing the matter with Charney, Platzman and Phillips decided on a plan: they would leave Chicago for Princeton to arrive on 21 May for preliminary work, return to Chicago, then go on to Aberdeen to arrive on 4 June. Personnel issues were becoming a problem. Everyone had someplace else to be: Freeman was scheduled to give a paper in Los Angeles, Phillips had to return for his convocation, and Platzman was obligated to attend a wedding. Platzman thought they could manage the last week in Aberdeen with himself plus Joseph Smagorinsky, Davies and Gilbarg.[579] Von Neumann would be in Los Alamos during this second run.[580] But despite these difficulties, the runs did proceed.

In mid-July, Charney (visiting at the University of Chicago) advised von Neumann (still in Los Alamos) of the second expedition results. ENIAC and IBM equipment problems adversely affected their operations during their last week in Aberdeen. During their four-week stay, they computed 70 time steps for four different models. Charney estimated a 41% operational efficiency – much lower than the estimated 90%. Machine failures cost 40% of their time, while another 19% was lost to programming errors.

The model run contained wave perturbations on a zonal current (west to east) having a jet-like structure. The perturbation energy was fed into the mean

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[578] Charney to Platzman, 12 May 1951 (Charney papers, B14, F451).
[579] Platzman to Charney, 17 May 1951 (Charney papers, B14, F451).
[580] Von Neumann to Charney, 2 June 1951 (Charney papers, B16, F517).

current for two of the models and withdrawn from the other two. While in Chicago, Charney was working up a theoretical analysis of what had happened. Smagorinsky was doing part of the analysis in Princeton. Bolin had written up the barotropic tendency calculations that he had put together with Charney. By July 1951, satisfied that they had significant results, Charney and Bolin were looking for an appropriate publication venue.

Rossby, who had considered remaining in the United States after a fall of teaching in Stockholm, had decided to remain in Sweden permanently and establish an International Meteorological Institute. He had envisioned a larger organization, but since Rossby would be setting it up "without an angel," a smaller scale organization was required in the near-term. This meant that at least some members of Rossby's Institute would likely be available to work elsewhere. It also meant that the help that Charney might have expected to come from the Stockholm group might be forthcoming due to a shortage of personnel. As an interim measure, Charney proposed that von Neumann appoint Eliassen part-time to the IAS staff. Then he could shuttle between Princeton and Stockholm while being paid full-time by IAS. Charney needed Eliassen: "Half an Eliassen is better than two non-Eliassens, and far better than no Eliassen." Charney was not sure about Rossby's plans other than he intended to teach in Chicago during spring term 1952. Rossby had been trying for endowment funding from the Munitalp Foundation – a New York City-based organization interested in supporting basic meteorological research.[581] However, that possibility was fading rapidly. If Rossby's group failed to secure Munitalp money, Charney suggested that they might want to apply for the funds themselves on behalf of the Meteorology Project. Rossby, ever on the lookout to secure funds for meteorological research, liked this idea and expressed his willingness to intercede on their behalf.[582]

The Project's ability to evaluate the impact of the jet stream got a huge boost during the summer of 1951. Joseph Smagorinsky had succeeded in finding a simple Green's function[583] for the solution of the partial differential equation governing the influence of continental topography and friction on a jet-like westerly current. Therefore, they would be able to evaluate the effects produced by the displacement and intensity changes of the zonal jet stream. The Princeton team thought the perturbations produced by topography would localize the centers of action in the upper atmosphere. This would provide a way to introduce heat sources and sinks as well as large-scale turbulent stresses responsible for the variations in the mean jet stream structure.

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[581] Munitalp, which is "platinum" spelled backwards, was a small foundation which funded basic research in the meteorology, primarily areas which its directors thought would not be funded by the military. During a meeting of the board of directors and meteorologists Horace Byers and Sverre Petterssen, in addition to Vincent Schaefer of the General Electric Research Laboratory in Schenectady, New York, Foundation leaders decided to place most of their funding with cloud physics (i.e., weather modification) efforts. Prior to this decision, Munitalp had seriously considered funding international meteorological institutes like the one Rossby was establishing in Stockholm. Proceedings of the First Form of Munitalp Foundation, Inc., 14 November 1951 (V. J. Schaefer papers, M. E. Grenander Department of Special Collections and Archives, SUNY Albany, B II.3.1U).

[582] Charney to von Neumann, 13 July 1951 (von Neumann papers, B15, F1).

[583] *Green's function* (or an influence function) is a function that is the known solution of a homogeneous differential equation of a specified region and that may be generalized (if the equation is linear) to satisfy given boundary or initial conditions, or a nonhomogeneous differential equation. It is an alternative to the Fourier or Laplace transforms. *Glossary*, 349.

Meanwhile, Charney, now back in Chicago, had been working on a theoretical investigation of the mechanism which transferred angular momentum and kinetic energy in both barotropic and baroclinic wave systems. He found that both stable and unstable baroclinic perturbations increased zonal kinetic energy. Charney had examined four barotropic cases. In each one, he had found a net increase in momentum in the middle latitudes.

During this period, the British Meteorological Office (BMO) had also been looking at the quasi-geostrophic advective model[584] and had found it wanting – as had the Princeton group. However, when they included the vertical advection of entropy into the calculations, the results agreed with the observations. BMO personnel had shared these observations with Charney during his tour around Europe visiting institutions which had taken up numerical weather prediction research. Charney had heard the same comments from other groups. Therefore, the Princeton group concluded that it would take the integration of the complete three-dimensional quasi-geostrophic model to produce a major improvement in forecasting. They decided to turn their efforts towards programming the three-dimensional model for the Princeton machine. Rossby's Stockholm group would do much of the preliminary testing and synoptic analysis.[585]

Charney was concerned about the "fog of criticism" that had begun to settle around their numerical efforts. His strong opinion was that they needed to lift it

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[584] An *advective model* is based on discrete advection terms only, with less or no emphasis on forcing, dissipation, and physics. They are usually for one level only. *Glossary*, 15.

[585] The Institute for Advanced Study, The Meteorology Group, Progress Report, July 1, 1951 to September 30, 1951, Contract No. N-6-ori-139, Task Order I (Charney papers, B9, F304).

very quickly. He told Rossby that, in his view, the heart of the problem appeared to be due to the Meteorology Project's reluctance to embrace the baroclinic model. Charney thought that if they stayed with the so-called two- or two-and-a-half-dimensional models they would end up with the same inconclusive results they had seen from the barotropic models. Although he admitted that there was still much to learn from those models, what he feared most was a loss of priority to other numerical prediction groups who were just then starting to work on the two- and two-and-a-half-dimensional models. If the Princeton group did not move into three-dimensional modeling soon, they could find themselves overtaken by other groups. At one point, von Neumann had asked him how many different weather parameters they were actually considering as data. Not many, Charney realized, which meant he realized that the computer would be able to handle the three-dimensional problem. At that point, Project members decided to start immediately on programming the problem for the new machine. They soon discovered that potential temperature[586] was a better coordinate than pressure. Charney acknowledged that Rossby had told him this some time before, but he had "pooh-poohed" the idea. He asked Joseph Smagorinsky to find the appropriate equation. They found potential temperature worked better than using pressure or height as the dependent variable both conceptually and computationally. Then they found out that Frederick G. Shuman – a Weather Bureau meteorologist – had done his thesis

[586] *Potential temperature* is the temperature than an unsaturated parcel of dry air would have if brought adiabatically (i.e., without heat exchange with its surroundings) and reversibly from its initial state to a standard pressure (usually 1000 mb or the earth's surface). *Glossary*, 588.

on the quasi-geostrophic equation of motion in terms of *x, y,* (horizontal coordinates) and potential temperature (vertical coordinate). Harry Wexler sent Shuman to visit the Princeton group. Unfortunately, persistent personnel shortages in the Weather Bureau again intervened: Shuman was unable to follow up on his ideas because he had been assigned as a tornado forecaster.[587]

Although their programming difficulties might appear mostly technical, team members did have some remaining physical concerns. One of those dealt with how to treat the tropopause. If it were treated as a discontinuity, it would make the boundary conditions a real problem. It would not be a problem if it were considered isentropic (i.e., of constant potential temperature), but that was not always the case. If it were assumed to be non-isentropic, it would still be difficult to consider it as a material surface. The group decided consider it not to be a discontinuity.

Their biggest problem was having too few people to do too many things. Joseph Smagorinsky spent all his waking hours on his thesis, which left Charney and Norman Phillips (Ph.D., 1951, University of Chicago) doing most of the work. They desperately needed synoptic help. Charney turned to Rossby. The Bureau of Standards had loaned them a mathematician who had been a huge help on some mathematical problems involving the inversion of matrices. He had asked to join the group, but salary was an issue. Charney thought bringing in yet another mathematician was akin to "carrying coals to Newcastle." Eady had proposed

[587] Charney to Rossby, 16 November 1951 (Charney papers, B14, F459).

sending his student Andrew Gilchrist from the U.K. and they had accepted him. Phillips was working out "splendidly" for which Charney was very glad.[588]

Platzman was working off-site at Chicago on barotropic stability issues. Initially, his emphasis had been on the connection between stability and the mean-flow velocity profile. This approach was essentially the same as the other approaches to the problem. However, as his investigation progressed, he discovered that serious misconceptions could arise if one considered the *shape* of the mean-flow velocity profile as the only controlling factor. He changed his approach and set aside the calculations on which he and Phillips had worked so hard.[589]

By the end of year, the group's size was desperately small and almost unable to function. Only two meteorologists (Phillips and J. Smagorinsky), a coder, and Charney worked on the Project – not nearly enough people to tackle the awaiting tasks. They spent most of their time programming the integration of the three-dimensional quasi-geostrophic equation of atmospheric motion for the new computer, which was now (in late 1951) "physically complete." They broke the large task of programming into several parts. Since it turned out to be easier to use potential temperature instead of the vertical distance as the vertical coordinate, they had first decided to choose θ. However, the ground was not a coordinate surface, so they worked on finding another coordinate which had the advantages of potential temperature and for which the ground was a coordinate surface. They determined the lateral boundary conditions by a heuristic method analogous to the one used in

[588] Charney to Rossby, 16 November 1951 (Charney papers, B14, F459).
[589] Platzman to Phillips and Charney, 20 November 1951 (Charney papers, B14, F451).

the barotropic model. They took the upper boundary to be a 400 Kelvin isentropic surface and assumed it was rigid. Thus they could treat the upper boundary and the earth's surface the same way. After estimating truncation errors for nets of different sizes, they decided to use a horizontal grid spacing of 300 km and a vertical interval equivalent to 150 mb of pressure. The entire grid would cover 4000 km on a side and contain about 1200 points. They then investigated a number of relaxation techniques and settled on a modified Liebman method with an empirically determined constant of overrelaxation.[590]

In early January 1952, life was very hectic at the nascent Institute of Meteorology in Stockholm as the staff members prepared their own attack on the numerical weather prediction problem. Writing to Charney, Bolin advised him that Rossby still "loved" him even though he had not answered Charney's last two letters. However, the Institute still had an uncertain financial base, and Rossby needed to attend to administrative concerns. The Stockholm team members, however, had been busy making tendency computations for Europe and the eastern Atlantic. They would publish the results in *Tellus.* Eliassen and William Hubert – a U.S. Navy aerologist studying with Rossby – were applying the barotropic computations to the blocking cases and attempting to find the reasons behind any errors. Karl H. Hinkelmann (b. 1915) (from Germany) was working on the three-dimensional model, but was leaving for Germany in just a few days. Snorre

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[590] The Institute for Advanced Study, The Meteorology Group, Progress Report, October 1, 1951 to December 21, 1951, Contract No. N-6-ori-139, Task Order I (Charney papers, B9, F304). To *overrelax,* the "guess" is made to overshoot the target value and then gradually converge to a solution.

Arnasson (Iceland) was attempting to forecast surface pressure changes with a procedures he had developed. Ernst Kleinschmidt was looking at potential vorticity in a scenario over the United States. Phil Clapp (from the U.S. Weather Bureau) was making barotropic computations for 5-day mean maps. Chester Newton (also from the United States) was performing a detailed analysis of the jet stream. Bolin himself was considering the possibility of using a very large grid to forecast the large-scale flow pattern. He had determined that one had to ascertain a good initial field and then compute mean values in such a way that the few points available were representative. He was also working on a simple three-dimensional model – one with three points in the vertical, but was not yet ready to discuss it.[591]

A week or so later, Bolin wrote again. He was anxious to avail himself of an opportunity to go to Princeton at the end of the spring term (1953) and stay until January 1954. With the computer (BESK) being built in Stockholm, he wanted to bring himself up-to-speed on the problems of programming by working with the Princeton group. He also wanted time to visit Chicago, Woods Hole, and MIT – partly because he wanted his new bride to see something besides Princeton. Since his last visit with Charney, he had been working on two- and several-parameter models and had almost finished a paper on the latter. He closed by saying that Rossby hoped to place Phillips in Bolin's position in Stockholm.[592] The "air mass mixing" of personnel between the United States and Sweden – and between Princeton and Stockholm, in particular – accelerated after Rossby established his

[591] Bolin to Charney, 10 January 1952 (Charney papers, B4, F121).
[592] Bolin to Charney, 19 January 1952 (Charney papers, B4, F121).

new International Meteorological Institute. Up until this time, Rossby's Stockholm group was mostly occupied with conducting small-scale studies for the personnel-deficient Meteorology Project. But by late 1951, the Stockholm group had started work on its own numerical weather prediction efforts focused on eastern Atlantic and western European data. With Rossby arranging the movement of meteorologists across the Atlantic in both direction, the Princeton group's numerical techniques were being aggressively extended and applied outside the United States.

"JOHNNIAC": ALIVE AND ON-LINE

The Project's quarterly progress report released in late March 1952 announced a major turning point for the Meteorology Project: The IAS computer – nicknamed by some "Johnniac" – was finished. With this milestone reached five and a half years after the Project's stumbling beginning, Charney and his team turned their attention to using the machine. Because the three-dimensional model was just too complex, they decided not to use it for the initial test computations. Instead, the team members opted to run the barotropic model first because they already had experience with it during the ENIAC expeditions. This time around they intended to vary the grid size, time intervals, relaxation methods, and numerical storage specifications. The team also planned to use two-dimensional models incorporating essential barotropic features.

The group discovered during the first run that checking for accuracy at the end of the calculation was not as effective as having the machine check itself and then stop when it made an error. They solved that issue by adding automatic checks to the coded instructions. The team also created a code to convert stream functions,[593] which were read from the weather charts as decimal numbers, directly into the initial binary vorticities needed by the computer. In the past, they had used the stream function as a dependent variable, but the team members reduced the round-off errors by using the absolute vorticity[594] as the dependent variable instead. Then they found the stream function from the vorticity field by solving a Poisson equation.[595]

With the selection of the modified Liebman relaxation method en lieu of the Fourier transforms for solving the Poisson equation, the group had to change all their barotropic programs and codes. They made this decision because they had already chosen the modified Liebman for the three-dimensional model. This gave them an opportunity to try it out with the two-dimensional model. The team used the first few integrations to locate machine errors and then to remove errors in the

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[593] A *stream function* is a parameter of two-dimensional, non-divergent flow with a value that is constant along each streamline (i.e., a line with its tangent at any point in a fluid parallel to the instantaneous velocity of the fluid at that point). *Glossary,* 735.
[594] The *absolute vorticity* is the vorticity (spin) of a fluid particle determined with respect to an absolute coordinate system. *Glossary*, 3.
[595] A "Poisson equation" is a differential equation of the form $\nabla^2\Phi = F$, where ∇^2 is the Laplacian operator, Φ is a scalar function of position, and F is a given function of the independent space variable. *Glossary*, 579.

code and program. However, they did not try to draw a conclusion from the resulting forecast because of high error probabilities.[596]

Rossby had been visiting in the States since January, but was unable to visit Princeton before his return to Sweden in April. He nevertheless reported to the Meteorology Project members that Newton had finished analyzing the maps Charney and his group needed. Navy officer Bill Hubert had sent them to Princeton via the U.S. Embassy. Eady was in Stockholm where he had given a very good seminar on the 2 ½ dimensional model. However, because the new BESK computer was not yet ready, they could only test it out with tendency calculations – the same method the Princeton team had used before running their trials on ENIAC. Finnish meteorologist Lauri A. Vuorela was on board for a couple of months studying atmospheric deformation fields. Rossby was trying to find a mathematician who was trainable in meteorology to help them as soon as the computer (BESK) was ready. He had already identified someone who might be able to join his Stockholm team.[597]

Charney let Rossby know that things were rather frantic in Princeton and he did not think he could escape to Europe any time soon. Besides, until he had new products to show from the IAS computer, he did not think it would be worthwhile to make the trip.[598] Rossby understood the staffing problems facing the Princeton group. He suggested that they add both Chester Newton and his meteorologist wife,

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[596] The Institute for Advanced Study, The Meteorology Group, Progress Report, December 22, 1951 to March 31, 1952, Contract No. N.-6-ori-139, Task Order I (Charney papers, B9, F304).
[597] Rossby to Charney, 5 April 1952 (Charney papers, B14, F459).
[598] Charney to Rossby, 9 April 1952 (Charney papers, B14, F459).

Harriet, to the team to provide help on the synoptic side. After a year of dealing with theory in Stockholm, the Newtons could be a considerable addition to the Project. Rossby suggested that they keep Ernest Hovmöller in Princeton until the Newtons returned. Hubert was in Stockholm working with Eliassen and he could be assigned to Princeton upon his return. A Navy officer, he had sufficient "charm and tact" that he could blend in with the Princeton group without bringing a "military atmosphere" with him. As far as theoretical meteorologists went, Rossby was really without anyone to recommend. In his view, the situation in the United States was rather desperate. There was no one at Chicago. He also thought that Starr at MIT was educating his students in a way that did not match what Charney required in Princeton. One possibility was Hinkelmann from Germany (Bad Kissingen). Rossby offered to bring him to Stockholm for "inspection" and then they could determine if he would be a suitable addition or not.[599]

Wexler visited the Princeton group in mid-May for a briefing and demonstration of the new computer, which was one-tenth the size of ENIAC. When working correctly it could compute a 24-hour forecast in three hours. Von Neumann was postponing a dedication ceremony until he was sure all the "bugs" were out of it. Wexler reported that Charney's primary concern was lack of personnel and not funding. They were especially short of synoptic staff to analyze charts. The Weather Bureau had been providing analysis assistance via WBAN for

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[599] Rossby to Charney, 16 April 1952 (Charney papers, B14, F459). Hovmöller, a synoptician, was visiting from the Sveriges Metteorologiska Och Hydrologiska Institute (Swedish Meteorological and Hydrographical Institute – SMHI).

quite a while, but the increased need had stretched them to the limits. When possible, Namias's extended forecasting section was lending a hand, but it did not have many slack periods. Smagorinsky had completed his Ph.D. and wanted to return to the Weather Bureau instead of accepting a position with the GRD. It was his hope to be able to introduce electronic computer techniques to forecasters. In closing, Wexler wrote, "[I] am quite impressed by the progress shown by this group and feel that continuation of this progress will go far to promote the science of forecasting."[600]

Having discussed the Meteorology Project's progress, the Weather Bureau leaders realized that there would be a move to operational use of numerical methods in the not too distant future. Reichelderfer, planning ahead, recognized their budget forecasts needed to include funding for NWP. These funds would be earmarked not only for eventual computing and accompanying ancillary equipment, but for training of personnel well in advance of their introduction so that the Weather Bureau would be "in the forefront in its readiness to adopt improved techniques."[601] Since the Weather Bureau had for many years been the subject of criticism for being behind the times (despite having an insufficient budget to be *with* the times), there is no doubt that with all the publicity and media hype

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[600] Wexler to Reichelderfer, 12 May 1952 (Welxer papers, B5, F1952-2). Emphasis in original. Also in attendance for the von Neumann briefing were Joseph Kaplan, Father James Macelwane, S.J., and Ross Gunn of the Geophysics Panel, Science Advisory Board, U.S. Air Force; Bernhard Haurwitz (NYU), Herbert Riehl, and George Platzman (both of Chicago).
[601] Reichelderfer to Plans and Program Management Office (P&PMO), 13 June 1952 (Wexler papers, B5, F1951-3).

surrounding forecasting by computer that Reichelderfer wanted to be ready to push ahead with the project as soon as it was operationally feasible.

Rossby's Stockholm group held a conference in early May 1952 on numerical weather prediction issues. Bolin reported that interest in numerical techniques was increasing. At this meeting, the German Weather Service (*Deutscher Wetterdienst*) had reported it was going to try computing the three-dimensional field of the tendency using ten grid points in the vertical. Preliminary attempts to relax in three-dimensions had been quite successful and were less time intensive than expected. The Germans anticipated having one field computed during the summer, but it took five people working for several weeks just to compute the Jacobians that were involved.[602] This result intrigued Hinkelmann and Eliassen, who had discussed the necessity of reducing the equations to one partial differential equation before "relaxing" it, and why it would not be just as possible and effective to relax an entire system of equations. Bolin thought that Hinkelmann was going to attempt it. Fjörtoft had introduced a graphical technique whereby one could produce a twenty-four forecast in about three hours. Those attending the conference were very supportive of getting this method into forecast centers as soon as possible. Several methods were proposed for the derivation of the 2 ½ dimensional models and the Stockholm group planned to run tests of them during the summer.

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[602] A *Jacobian* is the determinant formed by the n^x partial derivatives of *n* functions of *n* variables, when the derivatives of each function occupy one row of the determinant. *Glossary,* 425.

In closing, they were all very curious about the status of the Princeton computations, but understood that Princeton team members were busy with computer problems that needed to be solved. The Stockholm group very much wanted to hear the latest as soon as Charney or Smagorinsky had the time to write.[603]

During the first few months that the computer was fully operational, the Meteorology Project's members concentrated on running a series of tests on the barotropic model for the 500 mb level. To adequately test a variety of three-dimensional models, they chose to use the same highly baroclinic scenario for each run: the November 25, 1950 storm over the eastern United States. Because this system had produced an extremely nasty (and unforecasted) snowstorm which had been a large "bust" for the Weather Bureau forecasters, it was extremely well documented in that somewhat wistful way that only missed forecasts come to be remembered. Therefore, it was a logical choice to see how well the computer runs would compare with the actual observed data. It was also an interesting test case, because its rapid development involved conversions of potential to kinetic energy – something that the models would need to be able to handle effectively if numerical weather prediction was going to move into an operational mode. Predicting only run-of-the-mill low pressure systems moving across the weather map was not exactly a big gain over what experienced forecasters could do without computer assistance.

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[603] Bolin to Charney, 27 May 1952 (Charney papers, B4, F121).

To tackle this problem, Project members in spring 1952 decided to run two models: the equivalent barotropic and a simple baroclinic.[604] The barotropic model only predicted flow at the 500 mb level since it did not account for any vertical structure in the atmosphere. And since this model did not make potential energy available for conversion to kinetic energy, it could only predict a redistribution of the initially present kinetic energy. The equivalent barotropic would be the control. When the computer operated at full capacity, the 24-hour forecast required ninety minutes of run time. The group members determined that this was 10,000 times faster than a person doing the same calculations with a desk calculator which would take eight years of forty hour weeks (and not provide much of a forecast). If they increased the program efficiency, the 24-hour forecast could run in ten minutes. They made twelve runs: six 12-hour and six 24-hour barotropic forecasts. As suspected – it was, after all, a barotropic model – the Project members discovered that the model did not fully predict the extremely rapid and intense development of the storm.

The group decided to overcome this problem by creating a simple baroclinic model which would take the three-dimensional character of the atmosphere into account. Consisting of two barotropic layers at 700 mb and 300 mb, they reduced the time step to thirty minutes to avoid computational instability. Total computation time was two and a half hours at full speed for a 24-hour forecast. Since the machine typically ran at half speed, it took twice as long. As hoped and expected,

[604] An *equivalent barotropic model* is an enhanced version of the standard barotropic model such that the variation in the wind with height is averaged in the vertical.

the baroclinic model performed better than the barotropic model. The 24-hour forecast showed less degeneracy from the 12-hour forecast. The group was thus encouraged that the baroclinic model might be able to give reasonably good forecasts out to 36 or even 48 hours. Because the two-layer model included a vertical component, it could also predict cloudiness and precipitation. The team members concluded that the baroclinic model output agreed fairly well with the observed atmospheric state.

This two-layer model was a step on the path to operational numerical weather prediction. But it was not the destination. Because the two-layer model could not account for horizontal variations of the static stability, the team members would need to create a model with at least three layers.[605] This would entail overcoming many theoretical and programming problems. They had to consider the motion to be adiabatic because they did not have enough information about non-adiabatic effects. That required the potential temperature to be a conservative quantity which in turn allowed its use as a vertical coordinate in a semi-Lagrangian coordinate system.[606] Fortunately, this led to a simple form of the equation of motion which was well suited for numerical integration. The team members had completed the programming for the IAS machine, but the coding and computations

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[605] *Static stability* is the ability of a fluid at rest to become turbulent or laminar due to the effects of buoyancy. Also referred to as *hydrostatic stability* or *vertical stability. Glossary*, 723.

[606] A *Lagrangian coordinate system* requires that a fluid parcel be identified for all time by assigning it coordinates which do not vary with time. Therefore, very few meteorological observations are Lagrangian – to be so, one would need to take observations of the exact same air parcel over time. Because the potential temperature was a conservative quantity, it allowed for a modified version of the Lagrangian coordinate system.

had to wait for the additional memory, which would be provided by the new magnetic drum. The team members eventually hoped to include frictional effects at the earth's surface, non-adiabatic effects and large-scale orography.[607] Although it had only taken a few months to advance their model from the barotropic version to a simplified baroclinic version, the dream of including major frictional and thermodynamic effects in atmospheric models would not become a reality until the late twentieth century. Not only was the problem complex, the computer capacity did not exist to support it. With each new generation of computers, the models would advance. But the advance would be a slow one.

How did the model output stack up against the prognoses produced by experienced forecasters? Although Project members had not made detailed comparisons, the model output and the hand-produced prognoses appeared to show comparable accuracy. As far as the group was concerned, subjective forecasts had not improved in the previous twenty (or even fifty) years – an opinion that was widely held in the meteorology community. Even though model output was not superior to the subjective forecasts, at least the models could be incrementally improved over time as theory, mathematical techniques, and computing power advanced. Due to the Meteorology Project's work, numerical weather prediction efforts were increasing around the world as the Swedes, Norwegians, Danes, Japanese, British, and Germans all conducted research in the area. Thus by 1952, proto-typical numerical weather prediction techniques had achieved an aura of

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[607] The Institute for Advanced Study, The Meteorology Project, Summary of Work under contract N-6-ori-139 (10), NR 082-008 during the calendar year 1952 (Charney papers, B9, F304).

respectability. Meteorologists of all backgrounds had finally come to believe that gains in the science could only come from the application of computerized numerical techniques. Without them, potential advances due to increased surface and upper air observation would come to nothing.

Besides short-range forecasting goals, the group had been pursuing general atmospheric circulation issues which would be required for future attempts to make longer forecasts. In support of this goal, they studied the influence of large-scale longitudinal asymmetries in heating on the mean seasonal flow pattern. They had shown that this was the most important effect when explaining the observed normal lower tropospheric patterns and was just as important as orographic influence at higher levels. They also examined the role of unstable baroclinic disturbances in maintaining the energy balance. That study indicated that large-scale baroclinic disturbances converted sufficient potential energy to kinetic energy to balance the frictional loss, and at the same time were capable of transporting enough heat poleward to balance the net radiational loss.[608]

Despite a chronic personnel shortage, the Meteorology Project was still able to make significant gains between the time of the first ENIAC expedition and the period just following the testing of the new Princeton machine. Models were tried, compared against analyzed data provided by either the Stockholm group or the Weather Bureau analysis center, and then modified for the next run. Both barotropic and baroclinic models were checked out, as were models in two, two and

608 The Institute for Advanced Study, The Meteorology Project, Summary of Work under contract N-6-ori-139 (1), NR 082-008 during the calendar year 1952 (Charney papers, B9, F304).

a half, and three dimensions. But as Rossby had long maintained, to sell the meteorological community on the effectiveness of numerical weather prediction, and simultaneously advance model development, the Project members would need to put their models on the line – the operational line.

THOMPSON: ON THE MOVE

While Charney and his group were sorting out ENIAC results at the end of 1950, Thompson was preparing for a trip to Europe in early 1951. The purpose:

> To establish scientific contact with several meteorological institutes in Western Europe, observe research in progress at those institutions, estimate their capacities to expand the scope and scale of their present research programs, and if it appears desirable, to initiate such preliminary negotiations as are necessary to arrange for partial subsidization of research in meteorology and weather forecasting.[609]

And why was this being done? Because Thompson had convinced the Air Weather Service that there was not enough research capacity – either manpower or facilities – in university meteorology departments in the United States to fulfill the Air Force's needs. Navy and Air Force demand alone had eliminated any flexibility for departments to ramp-up to meet increased demand for training during an emergency mobilization. Therefore, it was important to assess the opportunities to expand their research contracts to Western European countries where most of the meteorological research outside of the United States was being conducted.[610]

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[609] P. D. Thompson request for MATS Transportation, 21 November 1950 (Thompson papers, Biographical 1942-1953).
[610] Ibid.

Writing to his contact at USAF Europe, Thompson further explained that he needed to find out what other meteorological institutes were doing so that he could work towards "meshing" the Air Force's U.S. research program in meteorology with those of his Western European counterparts and "[seeing] where a few hard U.S. dollars would do the most good." To that end, he planned to visit the British Meteorological Office and the "Brunt school" (Imperial College, London) in the U.K., the Oslo Institute, the Stockholm Institute (Rossby), and five institutes in Germany (as recommended by German émigrés Heinz Lettau and Joachim Küttner) located in Göttingen, Mainz, Berlin, Hamburg, and Bad Kissingen.[611] To Arnt Eliassen he wrote that the "[main] purpose of this trip and of my visit to you is simply to get a general idea of the sort of problems which preoccupy the European meteorologists and to absorb something of their viewpoint..."[612]

At each stop on this European tour, Thompson analyzed who was on the staff, what their current workload and funding was, who might be available to do research on a contract basis, and how much of their time could conceivably be taken up with such work. Thompson also raised the issue of the legality of using the host country's funds for the work spaces while the research itself was funded by

[611] Thompson to Major C. V. Hendricks, 19 December 1950 (Thompson papers, European trip correspondence 1950-1951).
[612] Thompson to Eliassen, 20 December 1950 (Thompson papers, European trip Correspondence 1950-1951).

the U.S. government. Thompson wanted to make sure that this arrangement was possible before asking for formal proposals from his contacts.[613]

By the spring of 1951, the Air Force, in an attempt to compete with the ONR for the hearts and minds of scientific researchers, had established an Office of Air Research. However, it was not in a position to finance basic research in the same way as had ONR. Even ONR was moving towards funding more applied versus basic research. Therefore, Thompson advised his friend and colleague, Chankey Touart, then a doctoral student at NYU's Institute for Mathematics and Mechanics, that he should not expect the ONR to be able to continue to support mathematical research at the Institute at the same rate that it had. Thompson argued that ONR had pursued basic research so quickly that it had "crashed" and reached an "untenable position." As a result, it was being forced by "Battleship Admirals" to move just as rapidly away from basic research to applied research. In Thompson's opinion, this would have an impact on what meteorological and geophysical projects would be supported. If any organization wanted to pursue a basic research project, their funding potential would improve if they could somehow disguise it as an applied project.[614]

Thompson left his post as the Chief of the Atmospheric Analysis Lab at the Geophysics Research Directorate on 1 August 1951, and spent the rest of the year and into the next spring finishing his Ph.D. at MIT. As Charney and his group were

[613] Thompson, informal notes from European trip, January-February 1951 (Thompson papers, Notebooks 1951 Trip).
[614] Thompson to C. N. Touart, 10 May 1951 (Thompson papers, Touart, Chankey 1947-1951).

preparing to run models on the IAS computer, Thompson received a request from the Air Weather Service to submit his preferences (known in military circles as a "dream sheet") for his next assignment. Thompson, a first-class schemer and manipulator, did not want to find himself relegated to an operational post after spending his entire career in research and development. On the contrary, he intended to take whatever steps were necessary to ensure his continued leadership in, even control of, the Air Weather Service's numerical weather prediction program. Therefore, he made clear in his response that to his knowledge the Research and Development Command had already requested, or would shortly request, his assignment back to GRD. As he pointed out, he had spent virtually his entire career in the Air Force working in R&D and that certainly seemed to be the best use of his interests and abilities.[615] However, Thompson must have been feeling less than totally confident of this assignment. Jumping his chain of command, he sent a query letter to Sverre Petterssen who was Director of Scientific Services for the Headquarters, Air Weather Service. Thompson and Petterssen had recently attended a conference on general atmospheric circulation. He told Petterssen that he had been thinking about how numerical prediction could be exploited and wanted to make sure Petterssen understood his thoughts. (Demonstrating that Thompson wanted very badly to be reassigned to the GRD's meteorological research arm, despite being an Air Force major, Thompson wrote directly to Petterssen – an extremely clear breach of the command structure.

[615] Thompson to Chief, Air Weather Service, 27 February 1952 (Thompson papers, MIT 1950s).

Clearly he had a direct supervisor – no one in any military service is a "free agent" – and it was most certainly not Petterssen.)

Thompson's aim was to persuade Petterssen that current numerical methods produced forecasts that were the equivalent of more traditionally (hand) produced forecasts. (This was patently untrue – numerical methods would not be equivalent to those which could be drawn by experienced forecasters until the 1960s.) Furthermore, because these new numerical forecasts were completely objective, if the forecast failed, at least one would know where to look to find out where it had gone wrong. He anticipated that numerical forecasts would continue to improve with time as errors were removed from the models. Thompson pointed out that his own thesis work was an effort to "weld together the mechanical and thermodynamical aspects of the prediction problem" and therefore he thought the day would come when it would be possible to predict temperature and vertical motion fields. When that time came, then true objective weather forecasting would be at hand. Further, Thompson thought that waiting for something better was "psychologically untenable." He argued that to sustain an operational numerical weather prediction organization one would need to develop the models, build the equipment, find suitable physical facilities, and secure well-trained specialists. Those specialists did not exist, at least not in sufficient numbers. Therefore, they would need to be trained. By Thompson's estimate, if the Air Weather Service started immediately, it would take at least two years before they would be ready with an operational organization. Given that the work to date had been fairly incremental in nature, Thompson thought it was safe to go ahead and pursue what he had in mind as his goal – creating an operational numerical weather prediction center immediately.

There would, of course, be obstacles to overcome. The primary one was a complete lack of trained manpower. The question then became: where could sufficient numbers of people receive training in numerical weather prediction techniques? Thompson argued that it should not be undertaken in Princeton, because it would detract from the Meteorology Project's research mission. Even at the Geophysical Research Directorate in Cambridge, there were only a handful of people who were familiar with numerical techniques and the theory behind it. Unfortunately, he noted, virtually everyone who was active in the field had come from Scandinavia: Eliassen, Fjörtoft, Høiland, Bolin, and "of course Rossby." (So, of course, had Petterssen.) Thompson's suggestion: "move Mahomet to the mountain" – send people to Stockholm for training. Conjecturing that it would take Rossby at least a year to package a course of instruction, Thompson thought this alternative would work just fine because it would coincide with the completion Stockholm's new computer (BESK – a computer modeled on von Neumann's IAS computer, it was built by the Matematikmaskinamnden, i.e., Council for Mathematical Machines).

So what, exactly, did Thompson want? He wanted to go to Stockholm even if it was not good for his career. (In most cases, accepting too many assignments away from the operational forces reduces a military officer's future promotion opportunities. Thompson had never really served in an operational job.) Assuming that Petterssen would be in touch with Rossby soon (in fact he had timed this letter to reach Petterssen the day before he left for Scandinavia), Thompson asked Petterssen to explain his position to Rossby.[616]

The same day, Thompson sent a letter to Charney and enclosed a copy of the letter to Petterssen.. He asked Charney to "show the letter to no one" and to discuss the contents only with Rossby. However, Charney was not supposed to let Rossby know that the contents came from the letter to Petterssen. Thompson continued, "I am sorry to appear so conspiratorial about this, but my position would be extremely awkward if all the gory details were known to the people at Wright Field" (home of Thompson's superiors).[617] "Extremely awkward" was an understatement. Thompson's career could have been in jeopardy if his superiors had found out that he was working behind their backs and outside the chain of command.

Thompson, in the letter to Petterssen, indicated that the time was ripe for pursuing operational numerical weather prediction. In his letter to Charney, a disingenuously irate Thompson, attempted to put Charney on the defensive over a perceived deception: that Project members had not kept him fully advised of how close they were to implementing some sort of operational organization for numerical weather prediction. Further, since the progress towards operational NWP was so far along, there were no longer numerous options. In fact, there appeared to

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616 Thompson to Petterssen, 31 March 1952 (Thompson papers, B2, GRD Correspondence).
617 Thompson to Charney, 31 March 1952 (Thompson papers, B2, GRD Correspondence).

be just one: a joint organization among all the weather services. Thompson had opposed a joint venture. But now he desperately claimed, upon realizing that it might be coming about whether he wanted it or not, that he did not want to be left out. Of course, that is not what Thompson had told Petterssen, and Charney knew it. Thompson was trying to play both sides against each other and claim a spot with the victor. Hedging his bets, he told Charney he wanted to go to Stockholm. Could Charney, he asked, please pass that piece of intelligence on to Rossby?

One main point of Thompson's campaign against a joint operational unit was to argue against a key assumption of the Princeton group: that to make an accurate prediction the numerical modelers would need to deal with the three-dimensional motions of the atmosphere. On the contrary, Thompson thought it was possible to deal with baroclinicity in general by the "mathematical artifice" of vertical integration. If he got to New York after Easter, he might be able to get together with Charney to discuss it further.[618] Based on this comment, it appears that Thompson was not seeing Charney every "two to three weeks" as he claimed in a subsequent interview with historian William Aspray.[619] Indeed, it sounds like he had been, by his own actions, very definitely left out of the loop.

The next day, Thompson sent a letter directly to Rossby with the opening paragraph:

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[618] Thompson to Charney, 31 March 1952 (Thompson papers, B2, GRD Correspondence).
[619] Thompson, "An Interview with Philip Thompson" (conducted by William Aspray on 5 December 1986), Charles Babbage Institute, Minneapolis, MN quoted in Nebeker, *Calculating the Weather,* 154.

> We were in some hopes that you might turn up at the general circulation conference at MIT last week, but learned that you were returning to Sweden on Wednesday, nursing an incipient cold and otherwise worn by winter travel. (How you are able to withstand the pace of commuting between Chicago, Stockholm, and Princeton is a mystery deeper than the confluence theory.) At any rate, since it is unlikely that I shall see you before your next visit I felt that I should write to state my position as clearly as is possible under the present circumstances.[620]

Throwing military protocol to the wind, Thompson pushed on and told Rossby how much he wanted to join his group in Stockholm. Thompson understood that Rossby might be concerned about a military person's injecting "a completely alien outlook," but Thompson wanted him to know that his only interest was in research, not in a series of administrative staff jobs. (Since Navy Lieutenant Bill Hubert was already in Stockholm working with Rossby, and Navy Lieutenant Commander Dan Rex had completed his Ph.D. under Rossby in Stockholm, Thompson had to have realized that his military standing made no difference to Rossby. Therefore this comment is completely disingenuous.) Thompson declared that he was "looking forward to a visit to Sweden as a sort of preview of the meteorologists' Valhalla (which I am sure must be somewhere in Scandinavia) where the ghosts of old heroes still mingle with younger warriors." In fact, however, Thompson was less concerned with ghosts than he was perturbed that the forward movement in operational numerical weather prediction was a *fait accompli* and that nothing he did would affect the outcome. He was determined to go to Stockholm, even if it

[620] Thompson to Rossby, 1 April 1952 (Thompson papers, B2, GRD Correspondence).

remained to be seen exactly how that would play out with the Air Weather Service leadership.[621]

Nothing came immediately from Thompson's exertions. He soon received a response from Petterssen who was glad to know that Thompson agreed with him on "all essential points" – a fascinating statement since Petterssen was the one in charge. Whether Thompson agreed with him or not was completely immaterial. Petterssen was expecting to see Rossby soon and told Thompson he would get back to him later in the spring.[622] Rossby, meanwhile, had gotten Thompson's "flowery letter" and was not quite sure what he wanted. Surmising that Thompson wanted to join his group, Rossby told Charney that it was fine with him, but that he would talk to Petterssen who would be arriving in Sweden shortly.[623]

Thompson's letters strongly suggested that he was very much interested in (1) being a team player with the rest of the people working on NWP issues, (2) that he *really* wanted to go to Stockholm to study with Rossby, and (3) that he was willing to pull any strings possible to get what it wanted. Yet it seems evident that throughout most of his tenure at the Cambridge center, Thompson had been actively seeking to steer Air Force meteorology's research agenda. The trip to Europe by so junior a person to evaluate possible research contract opportunities, the astute assessment of research differences between the Air Force and Navy research arms, the drive to secure an operational run of his models, and the behind-

[621] Ibid.
[622] Petterssen to Thompson, 2 April 1952 (Thompson papers, B2, GRD Correspondence).
[623] Rossby to Charney, 5 April 1952 (Charney papers, B14, F459).

the-scenes, "don't tell anyone about this letter" maneuverings for his next assignment, all point to a scenario that was not exactly what it appeared to be to anyone involved with Thompson.

Over the next few months a very different scenario would be played out – not the team player one, but one that would quickly force the hand of everyone who had been involved in the Meteorology Project. All of this correspondence appears to have been a rather elaborate cover for what Thompson really wanted: a shot at controlling operational NWP.

WHO WILL CONTROL NWP?

While Thompson was making overtures to Rossby and Charney, which amounted to staking his claim as a team player, Chankey Touart – Thompson's relief as Chief, Atmospheric Analysis Laboratory, GRD, Cambridge – was writing to the Commanding General, Air Research and Development Command to propose the establishment of a Numerical Prediction Project (NPP) with Thompson as its leader. Thompson was most certainly behind this initiative.

Touart explained that two groups were working on weather forecasting using electronic computers: the IAS group, which focused on basic research and a thorough, systematic exploration of fundamental problems, and the GRD group – led by Thompson until the fall of 1951 – which focused on applied research and concentrated on the development of mathematical models and their exploitation with a more immediate concern for practical forecasting.

Work on numerical weather prediction had focused on the creation of "idealized models" of the atmosphere solvable by electronic computer. The GRD group had focused on a linearized, two-dimensional, adiabatic, quasi-barotropic model which assumed an average wind in the vertical, no energy input to the atmosphere, and that potential energy is only minimally converted into kinetic energy. Nonetheless, test runs had shown the model could produce a 24-hour forecast of 500 mb level winds commensurate with those produced by an experienced forecaster. The model broke down in cases that were more likely to cause a subjective forecast to bust: when there was a new system developing.

Touart then described the IAS model as being a "non-linear analogue" of GRD's own model. As far as he knew, once the IAS team had checked out their new computer, they planned to make actual forecasts based on a three-dimensional version of their model. Touart did not have much on which to base a comparison of the models, but "[guessed] on physical grounds" that the IAS model would not produce results which were radically different from GRD's except that the IAS model would produce an actual, not an averaged wind.

GRD thus had the following plan: to introduce topographic effects, prepare experimental 48-hour forecasts, and use their current computing machines to produce 24-hour forecasts on a "production" basis (presumably trying to do it real-time). Thompson, just finishing his Ph.D. at MIT, was crucial to the success of this work: his current research was focused on the "strong-development" problem and there appeared to be a practical application possible from this work. Additionally, Touart suspected that Charney was ready to take his project from the experimental forecast to operational implementation of some type.

Therefore, it would be of the greatest advantage to merge the operational and research arms of numerical prediction into one Numerical Prediction Project headed by Thompson whose "background, capabilities and burning aggressiveness" were required for the "immediate success" of this project. Touart requested Thompson's assignment back to GRD to head up the proposed NPP. In the meantime, Thompson would be assisting "unofficially" in getting it established.[624]

Within a few days of this letter, the *GRD Spectrum* (an internal GRD newsletter) published a small article on the "pioneers" in the Atmospheric Analysis Laboratory who would, "for the first time in meteorological history, as far as we know," produce an upper-level wind forecast by machine methods during the forecast period. Air Force civilian meteorologist Lou Berkofsky would direct the running of Thompson's model on real-time data. They hoped to have the forecast out twelve hours after the first PIBAL report came across the wire.[625] If Thompson had been surprised that Charney's group was about ready to pursue an operational venue, he certainly cannot have been in the dark about what the AAL was doing with his model.

Thompson, however, managed to maintain a low profile about his involvement with the proposed Numerical Prediction Project until early July 1952,

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[624] C. N. Touart to Commanding General, Air Research and Development Command, 22 April 1952 ((Thompson papers, Proposed Numerical Prediction Project 1952). Emphasis in original.
[625] *GRD Spectrum,* Vol. 1, No. 21, 25 April 1952 (Thompson papers, B2, GRD Correspondence).

when he started sending out letters looking for staff members for his new venture. Since all the letters were basically the same except for the opening personal paragraph, the inquiry sent to U.S. meteorologist Chester Newton – then studying with Rossby in Stockholm – can be used as an example. Thompson filled him in on the background: several institutions in the U.S. and overseas, including GRD, had groups working on the numerical weather prediction problem. He claimed that unlike the other groups, GRD had been "pursuing a long-range program" which would ultimately move into semi-routine numerical predictions. It was now time to exploit and apply the methods that had been formulated. Thompson added that he believed Rossby "[would] attest to this." (This comment was added for Newton's benefit since it did not appear in the letter to a UCLA staff member. Thompson, wanting to tie himself to Rossby with this comment, was counting on Newton to discuss the contents of the letter with Rossby.)

To speed up the process, the GRD planned to bring the research folks and the operational folks under one roof to facilitate a two-pronged attack on the problem. In case Newton might be wondering why the Air Force would choose to do this on their own instead of as a joint project (as was done with the Meteorology Project in Princeton), Thompson smoothly explained that it would take twelve to eighteen months just to get through all the administrative negotiations and hassles to make it happen. Therefore, it seemed best to set up their own outfit and "invite" people from other weather services. The target date: 1 February 1953. After asking Newton about his interest in accepting an appointment, Thompson then inserted a disclaimer that he had no official status in relation to the project although it was "tacitly assumed" he would be its director. "Meanwhile, I am acting as unofficial head of an almost non-existent project – as an entrepreneur, if you like."[626]

Writing to Charney a week later, Thompson tweaked him for not responding to the accusation leveled in his 31 March letter that Charney was basically involved in some sort of conspiracy against him. Then Thompson explained that things had changed in the meantime, and discussed GRD's plans to establish the new research/operational project in Cambridge. He argued that the development and application of numerical methods would proceed most rapidly if all aspects of the problem could be attacked "simultaneously and under the same roof." The "roof" that Thompson proposed covered the Air Force's Geophysics Research Division in cooperation with the Air Weather Service. He listed four subsidiary aims: develop, test and evaluate new methods of numerical prediction; devise high speed computational and data processing methods and equipment; prepare and send numerical products on a "semi-operational" basis; and, provide experience to personnel who would then move to field units.

Apparently Charney and Thompson had already discussed how a future operational unit might look because Thompson made it clear that he knew Charney did not support such work being done by a single entity. However, if NWP were just handled by the Air Force, there would be less bickering and they could get started that much faster. The new research group would include six people:

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[626] Thompson to C. W. Newton, 3 July 1952 (Thompson papers, B2, GRD Correspondence).

Thompson, a civilian who would relieve Thompson when he transferred, two meteorologists like Bolin or Phillips, and two more junior meteorologists. That being said, Thompson needed to find some research folks to join his group. Did Charney know of anyone that would be available? In particular, he intended to sound out Phillips – already under contract to Charney's group in Princeton – and hoped that Charney would not regard it as "proselytizing." And one last thing – Thompson wanted to know if Charney had any ideas about "inexpensive and efficient computation devices."[627]

Charney, now perceiving Thompson's vast ambitions, was livid. He called Wexler and gave him the gist of Thompson's letter. Charney made sure that Wexler understood that he especially did not like the idea of cutting the Weather Bureau out of the numerical weather prediction picture, given all the help it had provided over the lifetime of the Meteorology Project. Furthermore, Charney considered the Weather Bureau to be *the* federal meteorological service – not the Air Weather Service. He was sure that von Neumann was going to be "greatly disturbed" when he got the news. Obviously Petterssen was involved since GRD and the Air Weather Service were establishing the new unit together. There were also rumors about forming a unit in Washington. That would mean four proposed units: Chicago, Cambridge, Princeton, and Washington.[628]

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[627] Thompson to Charney, 9 July 1952 (Thompson papers, B2, GRD Correspondence).
[628] Record of telephone conversation, Charney and Wexler, 15 July 1952 (Wexler papers, B5, F1952-2).

The next day Wexler went to Cambridge to meet Touart and German-born climatologist Helmut Landsberg, who headed the GRD. Backpedaling, they denied that Thompson's letter had any official status because, after all, he was a student at MIT and not a member of GRD at all. Touart did admit, however, that the letter was written with his knowledge. The descriptive points about the unit? They were from a memo Petterssen had sent to General Senter (Air Weather Service). But the unit was not nearly as pretentious as Thompson had made it out to be. It was just an extension of the small NWP unit that existed within the Atmospheric Analysis Laboratory. They just planned to increase the number of workers to 24 and use punch card machines to find "analytic solutions for a linearized model in 24 hour jumps." At that point in the conversation, Wexler, Touart, and Landsberg called Charney. When asked about the linear models, Charney maintained that those models were "hopelessly out-of-date." Therefore, it would be a complete waste of time, money, and manpower to test them.

As a result of the conversation, Charney decided to call a meeting in Princeton. Thompson's letter had brought the matter to a decision point and representatives from all the weather agencies needed to get together and come up with a plan. For his part, Landsberg thanked Wexler for bringing the matter to his attention.[629] Assuming Landsberg's comment was genuine, Thompson and Touart had together jumped way past the chain of command on an official matter. It had been bad enough when Thompson went directly to Petterssen to request his help

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[629] Wexler to Reichelderfer, 16 July 1952 (Wexler papers, B5, F1952-2).

with getting to Stockholm, when Touart skipped past his immediate superior – Landsberg – and went directly to Air Weather Service with his proposal, the potential for disaster was truly imminent.

Wexler shared his more personal thoughts on this issue with Reichelderfer. In military terms, they were walking through a minefield and would need to be very careful. This problem was exacerbated by "strong ambitious personalities and presumably inter-service rivalries." It would be difficult to select any one model to be the operational one. Each model had its strengths and weaknesses. While the researchers may have been looking for some kind of perfection, the forecasters were looking for a product which would help them out in the near-term. They would need to be careful. Any quick moves into operational NWP could discredit the entire concept even if "obsolete" models were the ones they used.[630]

Charney recognized that there were not enough people to man several numerical weather prediction centers. If they did not all work together, then they risked failing. Having worked together from the beginning it was unconscionable for one group to engage in empire building while shutting out the people who had brought the numerical techniques to the verge of operational status. In a vain attempt to bring these issues to Thompson's attention, Charney's response was extremely blunt. "I saw no reason for answering your earlier letter with its fantastic imputation of ulterior motivations," Charney wrote. "Conspiracies are foreign to my nature, besides they take too much time." Continuing on to the plan itself,

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630 Ibid.

Charney argued that it was unwise to leave the other weather services out of the planning. The Air Force, Weather Bureau, and University of Chicago were already establishing a program in Chicago which would be performing the same kind of work that Thompson was proposing for the GRD site. He did not think that GRD was any better equipped than any other place to become an operational center. The personnel issues were huge. "If you succeed in obtaining good meteorologists who are already trained in numerical forecasting," Charney wanted to know, "will you not be robbing Peter to pay Paul? What is needed now more than anything, I repeat, is to train people."[631]

Furthermore, Charney was "astonished" that Thompson had not consulted with him or with von Neumann about these plans. While they were not "building empires" at IAS, they did have a one to two year lead on everyone else and were glad to share their experience. What was especially irksome was that GRD now provided half of their funds, so that IAS was openly competing with its own contractor. He made clear that Phillips was staying with the Meteorology Project – they had contracted with their funding agencies for four years just so they would have the long term stability they needed to hire people and keep them.[632]

Thompson seemed little dissuaded by Charney's letter. On August 1st, he sent a letter to Bolin in Stockholm asking him to join the GRD group. Bolin

[631] Charney to Thompson, 16 July 1952 (Thompson papers, B2, GRD).
[632] Ibid.

declined.[633] So did Newton, who thought that any operational unit needed to be "joint." His other concern was more fundamental: too rapid a move into operational NWP could leave them with forecasts of lesser quality than were available by subjective, hand-drawn techniques. If that happened, many people could be inclined to kill NWP before it even got off the ground.[634] Morton Wurtele, then at UCLA, was more interested. Besides wanting to know his possible civil service grade, Wurtele was concerned about how he could get his work published if he accepted a position.[635] Thompson clarified for him that the purpose of the group would be to develop the theory on which the NWP methods were based and to apply the theory that Thompson himself had just worked out.[636] Based on that comment, it does not appear that Thompson anticipated using any other group's models in his new Air Force unit.

Later in the month, Charney sent a copy of Thompson's letter (minus the first "conspiracy" paragraph) to von Neumann along with his comments. "This is empire building in its crudest form," Charney explained, clearly appalled that Thompson's proposal had been made without consulting either *the* only experienced numerical weather prediction group or the Weather Bureau. He realized that they were not going to be able to stop this kind of forward movement

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[633] Thompson to Bolin, 1 August 1952; Bolin to Thompson, 22 August 1952 (Thompson papers, B2, GRD Correspondence).
[634] C. Newton to Thompson, 22 July 1952 (Thompson papers, B2, GRD Correspondence).
[635] M. Wurtele to Thompson, 29 July 1952 (Thompson papers, B2, GRD Correspondence).
[636] Thompson to M. Wurtele, 4 September 1952 (Thompson papers, B2, GRD Correspondence).

into pre-operational research and training for numerical weather prediction. The only solution was to counterattack with their own proposals.

Numerical weather prediction, Charney argued, just like other weather-related issues, was universal in nature. That is, no one service should have a lock on creating or providing the service. Just as the joint WBAN analysis and forecast center handled these tasks for all three services, so a similar venue should be created for NWP. They would need trained personnel to launch such a group. But there was nowhere to train anyone except "in-house," because they were learning as they went along. Even with trained personnel, they would still need R&D work done on communications, data handling, and objective data analysis that could not really fit under a pure research umbrella. In fact, Charney noted that such research did not belong to any one group or person.

He proposed that their Princeton group could train a few people, but they really were not equipped to take on that task. They would need to set up an entirely new group – preferably located close by – which could work with the original group. Or, they could set it up with the University of Chicago group. That was already a joint venture under Petterssen's direction. However, Petterssen already had to be backing Thompson's new unit – a group restricted to the Air Force. Thompson had given every indication that he was not going to maintain close contact with Princeton group unlike the Chicago group under Platzman and the Weather Bureau which wanted to continue their close working relationship with the Meteorology Project. Charney reported that Wexler was "equally indignant."

Charney clearly did not trust the GRD or Thompson. He strongly urged von Neumann to join him in taking the initiative to put operational numerical weather prediction in the "proper hands."[637]

In correspondence with Charney, Thompson just as adamantly maintained that the Air Force had the right to go it alone for military reasons. Although now saying that the project was a "stop-gap" measure, in his opinion the necessity to support short-range military objectives meant it was "neither necessary nor desirable" to set up a joint operational group. However, Thompson was willing to let the other weather services take part up to the point that the NPP was not "too seriously affected by the attendant divisive forces." Likening Charney's version of the call to the "parlor game called Telephone," Thompson maintained that Touart had told Charney that they intended to test a number of new models and not just the linearized two-dimensional (Thompson's) model. If so, then both Wexler and Charney misunderstood Touart, for both were convinced that Thompson's new group would only test Thompson's model.

In his letter, Thompson backed away from his comments about training, implying that Charney himself had tried to put distance between the Princeton group and any operational venture. Since he now knew that was not the case, he promised to "belabor and besiege" Charney for assistance. He appreciated his offer of assistance for training even while commenting on Charney's "explosion" which

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[637] Charney to von Neumann, 17 July 1952 (von Neumann papers, B15, F1).

had come as "a surprise to everyone" when the topic had been discussed the previous week.[638] Clearly this letter was not meant to invite cooperation.

Von Neumann and Charney did not wait to consolidate their position. They called a meeting for 5 August in Princeton. Reichelderfer sent letters to the leaders of the Navy and Air Force weather services asking them to attend because there was not much time before the budget hearings. If they were going to plan for operational numerical weather prediction they needed to develop their justification. The letter included a discussion outline prepared by the Princeton group – probably Charney under the circumstances, although that is not explicit in the letter – which set out four stages which needed to be addressed en route to an operational reality. They were: preliminary training, pre-operational research and development (objective analysis, communications analysis and development, forecast evaluation, deductions of weather from numerical forecasts of meteorological variables, derivation of small-scale phenomena from large-scale predictions), direct preparation for the numerical forecast service, and operation of the numerical forecast service.[639] In his account, Aspray maintains that the meeting was called quickly because of concern over the budget hearings.[640] That is highly doubtful. Until Charney, Wexler and von Neumann were confronted with Thompson's

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[638] Thompson to Charney, 29 July 1952 (Thompson papers, B2, GRD Correspondence).

[639] Reichelderfer to R. O. Minter (Navy) and Thomas S. Moorman (Air Force), 29 July 1952 (Wexler papers, B5, 1952-1).

[640] Aspray, *John von Neumann,* 147. Nebeker's (*Calculating the Weather*) account follows Thompson (1983) which eliminates this entire piece of the story. Likewise Charney's 1981 interview with George Platzman makes no mention of the trigger for the meeting or the fallout from it. See Lindzen, et. al., *The Atmosphere – A Challenge.*

brazen attempt to take the "joint" out of numerical weather prediction, there had been no discussion of having a meeting to provide ammunition for a budget hearing. In any case, it would not have included everyone who had ever been concerned with NWP if it were just a Weather Bureau concern. Clearly this was used as a "cover story" to get everyone to Princeton and to force a decision to keep any move into operational numerical weather prediction a multi-agency program.

Wexler represented the Weather Bureau. The Air Force sent representatives from the Air Weather Service, the Air Research and Development Command and the Geophysics Research Division. Thompson, conspicuously, was not one of them, although Touart and Petterssen were present. The Navy sent representatives from ONR and Naval Aerology with Rex representing the latter group. The Princeton staff members rounded out the group.[641]

After reviewing the progress made in numerical weather prediction thus far, von Neumann made "inferences" based on what the Princeton group had learned. He assumed a general baroclinic model providing a forecast of at least 36 hours would be the first practical forecast. He noted that there would be three basic steps to any such forecast: (1) input, (2) actual computing, and (3) output. All of this should take 12 hours to complete. When programmed for speed considerations, he anticipated that it would take four hours to do the actual computations. Most of the other eight hours would be tied up with inputting the data.

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641 Wexler to Reichelderfer, 7 August 1952 (Wexler papers, B5, F1952-2).

Furthermore, von Neumann envisioned two problems that would need to be overcome: education and technology. The first was a problem because there were very few people who had both the synoptic meteorology and mathematics background to supervise and operate the program. With an intense training program, he thought they could get people trained in about three years. Technology problems included having a machine that was in "perfect condition" and in working order at any time. Petterssen noted that the machine would be "idle" most of the day, but that the "idle" time would be required for maintenance. Von Neumann envisioned that one third of the day would be devoted to preventive maintenance, one-third to test runs, and the rest for both operational and research runs.

Petterssen questioned the times for input, computation and output. However, Charney thought that they were being sufficiently conservative for an initial attempt. Eventually they would be able to save time in the input and output sections.

Another discussion point concerned geographic coverage. Von Neumann thought that available machines could cover the United States, but even that would not be optimum for the 36-hour forecast. He argued that it would be best to show that numerical weather prediction was viable before requesting the increased amounts of data that would be required. The machine could only handle so much data due to memory limitations. If they extended the area to range from Japan to Eastern Europe – a four-fold increase – then they would need a much larger machine. A sufficiently big machine might be available in five years.

The Weather Bureau representatives (Wexler and Smagorinsky, also working in Princeton) commented that it took seven hours from data receipt to facsimile transmission of the prognostic chart with current subjective, hand methods. A question that went unstated, but probably thought, was "How does it improve the situation to take an additional five hours to get the product out?" Wexler reported that Weather Bureau forecasters valued numerical weather prediction for time periods beyond 36 hours because they could already produce sufficiently accurate products for the 24 to 36 hour range. However, the general group consensus was that it was "too early" to lead people to think that longer range forecast reliability would be improved. Charney also argued that the barotropic forecasts, as then run, were not as good as a subjective forecast, but preliminary work on baroclinic models showed a promise of improvement.

The other major problem – training of personnel – also had to be addressed, and it had to be addressed quickly. Due to its chronically undermanned state, the Meteorology Project was probably not the best venue for carrying out this training. However, its members did have more experience than anyone else. Therefore, the Project members were willing to take in three people at a time for training. The attendees suggested one person from each service: Navy, Air Force and Weather Bureau. Those persons should possess most of the required qualifications: solid synoptic meteorology background, mathematical and physical expertise, and a limited need for a refresher course in meteorological theory. Others, including synoptic forecasters with some theoretical background or those with little or no meteorological knowledge, but solid in mathematics and/or physics, were thought to be best trained in a university setting. The University of Chicago had a program suitable for synopticians needing theoretical work. According to von Neumann, Chicago needed to attract more people from applied mathematics and physics into the program to ensure its long-term success. Petterssen noted that weather forecasting did not attract "many theoretical minded people," but believed that as numerical forecasting became a stronger element more of those kind of people would be willing to join the discipline.[642]

At this point in the meeting, the GRD's Touart introduced Thompson's personal statement about his plan for a parallel group moving forward with a two-dimensional model. This statement reflected the message sent to Charney, which had been the impetus for the August 5th meeting at Princeton. Thompson claimed that it was "wasteful" to pursue a three-dimensional model of the atmosphere when his two-dimensional model worked just fine. With just a modest investment, they would be able to turn his two-dimensional model into an operational model suitable for routine uses. Thompson maintained that "immediate military needs," which were distinct from the needs of the populace at large or of the research interests of the scientific community, dictated that they work on a model (his) that would be operational in two years or less. Thompson's would be a short-range program to produce the best model within two years for military purposes, while a long-range program pursuing the best possible model could still be undertaken for general

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642 Minutes of the meeting held at the Institute for Advanced Study, 5 August 1952 concerning practical numerical weather forecasting (von Neumann papers, B15, F4).

use.[643] Short of two dismissive comments by Charney and von Neumann, there was absolutely no discussion of Thompson's August 1952 proposal.

In the end, the attendees of the Princeton meeting only agreed on a few issues. Each service representative volunteered to provide a "trainee" to Charney's group (due to other commitments, it appeared that 1 December would be the absolute earliest arrival date for anyone). The Navy representative expressed support for a joint venture along the lines of the WBAN. The Air Force representative declined to comment on the joint venture and indicated the Air Force would continue its own project at GRD while supporting the IAS Project as it had done in the past.[644]

What is most clear, however, is that any attempt by the Air Force to claim NWP for itself was doomed. In his attempt to control NWP, Thompson had overreached, and, temporarily at least, those working in Air Force NWP were no longer trusted by other workers in the field. Despite the claim of Air Force representatives that they would proceed alone, what had started out as a joint project would remain a joint project. With no decision yet on what model to use, model development would continue at the Meteorology Project.

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[643] P. D. Thompson, "Statement to the Conference on Numerical Prediction to be held at Princeton, 5 August 1952 (Charney papers, B4, F135).
[644] Wexler to Reichelderfer, 7 August 1952 (Wexler papers, B5, F1952-2).

CHAPTER 8
A CHANGING ATMOSPHERE: FROM DEVELOPMENTAL TO OPERATIONAL NUMERICAL WEATHER PREDICTION (1952-1955)

"Electronic 'Brain' Planned to Forecast the Weather." Just one week after the 5 August 1952 meeting which set in motion the transition from developmental to operational numerical weather prediction, *The Boston Daily Globe* ran a Science Service article describing how computers would be making Weather Bureau forecasts in "two to three years." Although still experimental, the Weather Bureau planned to use numerical weather prediction operationally by feeding current data into complex formulae and getting out eight charts every twenty-four hours. "These will represent eight horizontal slices of the atmosphere, beginning at sea level and extending up to about 13,000 feet."[645] That statement must have been a huge shock to everyone even remotely involved with the Meteorology Project. They had concentrated on producing a 500 mb chart alone. Moreover, a model that topped out at 13,000 feet (the 500 mb "steering level" is at about 18,000 feet) would not do anyone much good.

And so the race for operational numerical weather prediction was on. This was a race in which the Weather Bureau very much wanted to participate and come out a winner. Reichelderfer immediately took steps to ensure that his agency would be prepared when the models and the computers were ready. The Navy, as

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[645] "Electronic 'Brain' Planned to Forecast the Weather," *The Boston Daily Globe*, August 12, 1952. For an insiders recollection of the move from research-oriented to operational NWP and the subsequent efforts of the Joint Numerical Weather Prediction Unit, see Cressman, "The Origin and Rise of Numerical Weather Prediction."

personified at this stage by Rex, wanted to make sure that the joint plan went forward as had been discussed at the August 1952 meeting. That meant bringing the matter to the attention of the Joint Meteorological Committee (JMC) under the Joint Chiefs of Staff. Within a short period of time, an *ad hoc* committee would be up and running, making recommendations to the JMC that would move the joint project forward. The Air Force, while continuing its investment in Thompson's Cambridge group, kept its hand in too.

Meanwhile, in Princeton, the Meteorology Project redoubled its efforts to make sure that an operational model was ready to go. However, that meant getting enough synopticians on the staff to perform the analyses critical to determining the efficacy of the models as they tested them. It was also becoming apparent that there were other problems that would need to be considered: selecting a computer, handling the data quickly and efficiently, and ensuring that the data sources themselves were adequate for the models. These issues were outside the purview of the Princeton group's members since they were modelers, not data handlers. People external to the Project would need to address those issues and soon. In the meantime, Project members wanted to make sure that the promise of NWP was not oversold to the media.

And these groups in the United States were not the only ones working on NWP. Rossby's Stockholm group was working to bring the NWP project to an operational stage with the goal of running models on the Swedish Air Force's new computer, BESK. They had fewer bureaucratic hoops to jump through – would they be the first ones to get out usable prognostic charts?

This final phase would bring numerical weather prediction from a theoretical vision to an operational reality. It would involve many more people than the small Meteorology Project in Princeton. This transition would very much involve the same government agencies that had been interested from the start: the Weather Bureau and the military weather services. The final part of this story (although just the beginning of the story for numerical weather prediction) addresses themes of governmental control over scientific endeavors, the use of the media to spread an agency's message, and how scientific research can come to take a back seat to more practical issues when science moves from the theoretical to the operational realm.

THEORY TAKES AIM AT OPERATIONS

While the Weather Bureau considered how best to move numerical weather prediction into practice, the Princeton team continued its work on model development. Past modeling efforts had been more about theory development than weather forecasting. But now, with the push to go operational, the Meteorology Project's models were going to have pass the usefulness test. As important as the theoretical work was to the overall development of numerical techniques and to a robust theory on which to base the atmospheric sciences, it was no longer sufficient to see what the models told team members about how the atmosphere worked –

they needed predictions that were realistic. To see if their forecasts matched reality, the team members would need some theoretically savvy synopticians to analyze the data for comparison. Synopticians were not available in the United States, so once again the Project looked toward Scandinavia. Project members also looked to the United Kingdom for additional help from dynamicists. And they prepared to bring weather service representatives – the men who would actually take numerical weather prediction operational – on board for training. Under pressure to produce a computer-generated weather forecast map that could meet or exceed the accuracy of one drawn by hand, the normally hectic way of life for the Meteorology Project was about to become more so.

Throughout 1952, the Meteorology Project had concentrated on short-term forecasting goals as well as on more theoretical work concerning the general circulation of the atmosphere. The latter would be especially critical as Project members attempted to extend numerical forecasting to longer periods. Forecasts also had to become more sophisticated. Not only must models generate an appropriate pressure pattern, they needed to offer guidance for the prediction of cloudiness and precipitation. After all, when the general population wanted a forecast they were interested in precipitation – would it rain, snow, hail, drizzle? For numerical weather forecasting to be successful, the modelers were going to have to have something more than steering flow to show for their efforts. Thus the Project members had spent much time determining atmospheric flow changes for 24 and 48 hours. These changes in the field of motion were a necessary, although not a sufficient, condition for predicting cloudiness and precipitation. Project members were still being guided in their modeling efforts by following a philosophy which started with a simple model and then made it progressively more complex. They analyzed their models for shortcomings, and then attempted to fix them in the next version.

Just as the group developed a simplified baroclinic model in mid-summer 1952, hardware problems struck the Meteorology Project. From mid-August through mid-September 1952, team members only had eleven hours of useful machine time running at eight kilocycles. With daytime hours reserved for machine maintenance, Project members were forced to conduct model runs during the evening. Their memory salvation – in the form of the new magnetic drum – would not be ready for another one or two months. So the team, under Phillips's direction during Charney's absence, decided to continue work on the general model using the minimum number of layers until the hardware situation improved. The team would not be able to advance its models without additional memory and a machine that was operational more hours than not.

Sharing his frustrations with Charney (who was visiting the Astrophysical Institute in Oslo in September 1952), Phillips reported that he had just managed to "eke" out a 12-hour forecast on the barely-operable computer and was sending the resulting charts on to Oslo for perusal. In Phillips's opinion, the finite 12-hour change was "disappointing" with only a very slight improvement over the corresponding barotropic run. However, he thought the initial tendencies looked quite good. Phillips did not yet understand the major error – the weakening of the height fall center – because the rise center, on the contrary, had "behaved well." Possible error sources included a poor physical assumption, e.g., the geostrophic or two-layer assumption, or the mathematics itself. Round-off error would not have been large enough to account for the produced effect. Truncation error could have accounted for the height fall error, but Phillips could not understand why it would be systematic. On this run, the two-layer forecast beat out the barotropic. The correlation coefficient computed for the two-layer forecast was 0.783 (with 1.000 being a perfect correlation); for the barotropic forecast, 0.745. Phillips attributed these results to better height fall center placement in the two-layer forecast.[646] Yet, it was not ideal and much more work needed to be done.

The Meteorology Project would see some personnel changes in the near future. The Navy's Rex had already acted on the Project's invitation to provide an NWP trainee. He established a Navy "billet" (position) and named Lieutenant Albert L. Stickles, a recent graduate of the Naval Postgraduate School's meteorology program, to fill it.[647] Although Stickles would be attached to the Princeton NROTC Unit for administrative and military purposes, his only duty would be as a researcher with the Project. Rex assured Charney that Stickles had a personal interest in numerical weather prediction and was professionally competent

[646] Phillips to Charney, 15 September 1952 (Charney papers, B14, F449).
[647] Rex to Phillips, 7 October 1952 (Charney papers, B14, F465 - attached to Stickles letter of 15 June 1953).

in mathematical methods as they pertained to meteorology.[648] Stickles would be the first of the three weather service members assigned for training with the Meteorology Project and would ultimately move to the Joint Numerical Weather Prediction Unit.

However, the Meteorology Project was still short of people – particularly with Charney overseas and Phillips home alone. It was trying to attract Briton Eric T. Eady, and Norwegians Eliassen and Fjörtoft for another tour of duty. Fjörtoft could be available as early as February, but would certainly be in Princeton before September. Eady was available for six months starting in September. Eliassen would not be able to come until early 1954, but would then be available for two years.[649]

The critical personnel problem was the lack of available synopticians. And all eyes turned toward Sweden. The question is why? Why was the Meteorology Project unable to find synoptic meteorologists within the United States who could do the job? Why did they have to import them from across the Atlantic? The short answer is that synoptic meteorology was not held in high regard in the United States and neither were its practitioners. The lack of interest in synoptic meteorology was not only a problem for the Meteorology Project, which, in the overall scheme of things had only a very small, albeit critical, need. It was a problem for the largest employer of synopticians – the Weather Bureau. In a

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[648] Rex to Charney, 24 November 1952 (Charney papers, Box14, F465 - attached to Stickles letter of 15 June 1953).
[649] Charney to von Neumann, 17 November 1952 (Charney papers, Box16, F517).

memorandum to some of his division heads marked "ADMINISTRATIVELY RESTRICTED," Reichelderfer bemoaned the fact that so "few meteorologists really find their principal interest centered in the daily weather picture." He wanted some answers. Why were people being led away from synoptic meteorology?[650] One Weather Bureau man thought it boiled down to either "you have it or you don't." In other words, there were those who experienced the poetry of weather and found "communion with the infinite." Others saw weather as contours on a piece of paper. The latter did not make synopticians.[651] On a less poetic note, a fiscal reason appeared: synoptic meteorologists within the Bureau were not rewarded with the equivalent pay grades occupied by their more theoretical brethren. The supervisor of the analysis section held a lower pay grade than those who led other sections. The district forecasters could never advance beyond their GS-11 paygrade. They were chronically undermanned, were rarely selected for graduate study, and were allotted no research time. For comparison, their French counterparts who were lead forecasts, i.e., those who led a forecast team of several people, worked one week on, one week off, and then had two weeks to do research. In marked contrast, the Weather Bureau lead forecasters worked virtually around the clock, seven days a week, and got recognition only when they made a very bad forecast, known in the profession as "busting the forecast." Until that situation changed, synoptics would

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[650] Chief of the Bureau (Reichelderfer) to Assistant Chief (O); SR&F and Scientific Services, 31 October 1952 (Wexler papers, B5, F1952-1).
[651] I.R. Tannehill to Reichelderfer, 7 November 1952 (Wexler papers, B5, F1952-1).

hold no allure for meteorologists desiring career advancement.[652] And so the Meteorology Project would need to depend on their Scandinavian contacts to fill their requirement, for it was the synopticians who were needed to move NWP from development to operation.

The grim staffing situation, which had left only Charney, Phillips, and J. Smagorinsky working in Princeton, had gotten some relief during the summer of 1952. Swedish synoptician Ernest Hovmöller, who had worked on jet stream structures and aerological studies of cyclone structure with Rossby's Stockholm group, arrived in July to work on a synoptic investigation connected to the transition from barotropic to baroclinic models. On a leave of absence from the Swedish Meteorological and Hydrological Institute (SMHI), he was obligated to return to Stockholm in January 1953. Von Neumann desperately needed Hovmöller to stay until at least September 1953 to complete this synoptic work. In fall 1952, Von Neumann proposed to Anders Knut Ångström, the Institute's director, that Hovmöller stay in Princeton after a short trip back to Sweden in early 1953. In pressing his case, von Neumann wrote, "His association with us is perhaps the first example of the kind of cooperation that will ultimately have to take place between theoretical and synoptic meteorologists, if and when numerical forecasting is integrated into the governmental weather services."[653]

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[652] T. P. Gleiter to Tannehill, 7 November 1952 (Wexler papers, B5, F1952-1).
[653] Von Neumann to A. Ångström, 14 November 1952 (Charney papers, B16, F517). Ångström spent much of his career studying radiation. In the 1920s, he worked with the Smithsonian's Abbot on measuring the solar constant. For more information, see Hessam Taba, "Dr. Anders K. Ångström," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1988), 75-83.

While von Neumann's argument was a valid one, Ångström rebuffed him, instead proposing that Hovmöller be replaced by fellow synoptic meteorologist Roy Berggren. Von Neumann was willing to take Berggren, but wanted Hovmöller, too, since their abilities would "complement each other admirably." Finances seemed to be a major stumbling block, but von Neumann was sure something mutually agreeable could be worked out. He suggested that Ångström wait until he had had a chance to debrief Hovmöller in Stockholm before making a final decision.[654]

Back in Stockholm, Hovmöller provided a full accounting of the progress in Princeton to Ångström and to Rossby, who was pleased that Project members were producing a significant number of computer forecasts. After Rossby had seen Ångström's latest letter to von Neumann about Hovmöller, Berggren stopped by to discuss the Princeton group with Rossby. Rossby then realized that Berggren had been "somewhat of a pawn in this peculiar chess game in which also Hovmöller is one of the pieces." Rossby told Berggren he could make a better contribution to the Project if he thoroughly reviewed theoretical issues before departing for Princeton. Berggren readily agreed with that plan.[655]

However, von Neumann's needs compelled him to approach Ångström again. Von Neumann was in desperate need of an outstanding synoptic

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Ångström came from a long line of scientists. His grandfather, Anders Jonas Ångström, lent the family name to a non-SI unit of wavelength: 1 ångström (Å) = 10^{-10} meters. SI units (Système International d'unités) are those based on the gram/centimeter/second or kilogram/meter/second units of measurement.

654 Von Neumann to Ångstrom, 2 December 1952 (Charney papers, B16, F517).

655 Rossby to Charney, 12 January 1953 (Charney papers, B14, F460).

meteorologist. In what can be characterized as a whining note, he plaintively wrote, "To obtain such a person we are willing to bring him over from Europe and we agree with you that we should pay the costs, providing of course that he comes not as a student or trainee, but as an employee." Von Neumann was still paving the way for more collaboration with the SMHI so that its personnel could receive training in numerical methods, and, of course, so that Meteorology Project could get its synoptic work done. Ångström was unmoved. Hovmöller remained in Sweden. Berggren joined the team in March 1953 and stayed until the end of the year.[656]

The continuing saga of the synopticians was not the only item on the personnel agenda. Rossby's Stockholm group, awaiting the birth of its computer, needed to prepare by practicing on an operational machine. Thus, Bolin announced in early 1953 that he wanted to get on the new IAS computer as soon as possible. The only potential scheduling problem would occur in the summer. Rossby would be out of town and if Bolin left too, there would be no Swedes at the Meteorological Institute. So if Bolin could join the Princeton group in the spring, he might have to return to Sweden for the summer and then come back to Princeton in the fall.[657]

Life for Meteorology Project members was hectic once again in early 1953. With the computer finally working well, the team was busy running and modifying models. Staffing, for once, was not a major problem. Six full-time meteorologists

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[656] Von Neumann to Ångström, 3 February 1953 (Charney papers, B16, F517).
[657] Bolin to Charney, 29 January 1953 (Charney papers, B4, F121).

were working with the group, including three new faces: Swede Roy Berggren, Briton Andrew Gilchrist and Japanese researcher Kanzaburo Gambo. Gambo, of the Japanese Meteorological Agency (JMA), was in Princeton to learn numerical techniques. (JMA was preparing to pursue its own numerical weather prediction program.) Charney was delighted to hear that Bolin was in-bound and that, in a swap, Phillips would be taking his place in Stockholm. This move would allow for more cross-pollination between the Princeton and Stockholm groups which were attacking the numerical weather prediction using the same method on similar computers, but for different geographic regions. Charney did not seem to care when Bolin showed up, as long as he did so. If he were able to come earlier, Charney suggested that Bolin consider taking his new bride around to other research centers, i.e., Chicago and Cambridge, as he had previously proposed. He hoped Bolin would be able to stay at least until the end of 1953, longer if possible, especially since both Fjörtoft and Eady would be there for collaboration. Charney was counting on Bolin to take Phillips's place as their "man-machine interface" and to do so, he would need to overlap with Phillips.[658] Since Bolin's presence was required in Stockholm for the summer lest some "foreign person" get "stuck" with handling the Institute's affairs, Bolin suggested that he reach Princeton in August to train with Phillips. Phillips could be in Stockholm for the fall when Rossby's group really needed him for its computer work. Bolin told Charney that he planned to stay

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[658] Charney to Bolin, 3 February 1953 (Charney papers, B4, F121).

at least six months.[659] Charney responded swiftly. He and Rossby had jointly decided that Bolin should start his work in Princeton on September first. Bolin could remain in Stockholm through the early part of the summer, and use the late summer to tour the U.S. with his wife.[660]

Not only had the regular staffing situation improved in early 1953, the three weather service trainees were also on board. Work had begun on the baroclinic model and had progressed into the coding stage by the end of March. Team members had also been able to speed up the barotropic model. The 24-hour forecast only took five or six minutes of machine time – a dramatic improvement over the 24 hours it had taken ENIAC just two years before. This particular version of the model used a one-hour time step and a 361 point grid covering ten million square miles. Team members also did another run of the November 1950 storm using the three-level baroclinic model for both 12- and 24-hour forecasts. This model was significantly better than the one- and two-level versions they had run previously. Additionally, the model output did not deteriorate with time and the storm prediction was better. Since baroclinic models required data at several levels, the group now needed to have an objective analysis program, i.e., a computer program to analyze the data instead of relying on hand analyses. Team members tried a number of schemes, but the best one appeared to be a least squares fitting of a second-order polynomial in the height, to the heights and winds within a 600 km

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[659] Bolin to Charney, 14 February 1953 (Charney papers, B4, F121).
[660] Charney to Bolin, 17 February 1953 (Charney papers, B4, F121).

radius of the point for each level in the multi-layer model.[661] By June, they had run more forecasts for the November 1950 storm as they focused on further development of the baroclinic model. Instead of using 200 mb (about 12,000 meters/40,000 feet), 500 mb, and 850 mb (about 1500 meters/5000 feet) data, they used 400, 700, and 900 mb data instead. The 900 mb height output accurately showed the storm's rapid development. Therefore, the group reported, this was the "first successful attempt to forecast cyclogenesis by purely numerical methods." Team members had then started to make model improvements that would include previously neglected non-linear terms. These models would include the first use of iterative processes in the solution of non-linear equations. Therefore, the results would be of interest to mathematicians as well as to meteorologists.

In order to advance the model work, the team members had to find an objective analysis model that worked. The reason was simple. In order to obtain input data, charts for each level in the atmosphere had to be analyzed, gridded, and the data then extrapolated to the grid points. Once these steps had been completed, the data were punched on to cards or on to paper tape, and fed into the computer. With an objective analysis program, all these steps could be accomplished by machine. As a result, there would be less subjective interpretation of the data, and presumably fewer initial errors introduced into the model. As team members continued to work on the analysis scheme, they attempted a new approach by changing the radius surrounding the data so as to always provide a minimum

[661] The Institute for Advanced Study, The Meteorology Project, Quarterly Progress Report, January 1, 1953 to March 31, 1953, Contract No. N-6-ori-139 (1), NR 082-008 (Charney papers, B9, F305).

number of data points within a circle. Those data point values would then be analyzed by the machine to arrive at one value for the given grid point within the circle. From experience, team members knew this new method probably would not work the first time. However, they planned to learn from each attempt and modify the method until they got the desired result. The Project members would not continue to learn much by running their models on the Thanksgiving 1950 storm data. What they needed were more case study options. With that in mind, team members began analyzing maps for two other time periods – hence the critical need for the synopticians. They hoped to be ready to attack these new scenarios in the near future.[662] And that was a good thing, because the Weather Bureau was moving ahead as fast as possible on their efforts to bring operational numerical weather prediction to fruition.

THE WEATHER BUREAU GETS STARTED

Weather Bureau Chief Francis W. Reichelderfer had been a strong proponent of theoretically influenced methods of weather forecasting since his days as a Navy aerologist, studying and promoting the Norwegian method of analysis and forecasting. He had aggressively encouraged von Neumann to put his new computer towards the weather forecasting problem. Throughout the life of the Meteorology Project, Reichelderfer had sent Wexler, his Scientific Services Division head, to Princeton to check on progress. He had also offered the assistance

[662] The Institute for Advanced Study, The Meteorology Project, Quarterly Progress Report, April 1, 1953 to June 31, !953, Contract No. N-6-ori-139 (1), NR 082-008 (Charney papers, B9, F305).

of Bureau personnel in securing and analyzing data for the Project's use. Now, as a member of the Joint Meteorological Committee, Reichelderfer was in an excellent position to influence the direction of a joint operational unit manned and funded by all three weather services. Just as the analysis function had been centralized at the Weather Bureau under the joint manning and sponsorship of all the weather services, Reichelderfer would do everything possible to ensure the Bureau's position at the forefront of operational numerical weather prediction. To guarantee that he was not out-maneuvered by his military counterparts, Reichelderfer started early to create an adequate support structure within the Bureau.[663]

Joseph Smagorinsky had worked periodically for the Meteorology Project while finishing his Ph.D. during his leave from the Weather Bureau. A month after the operational NWP meeting in August 1952, Reichelderfer invited him to return to the Bureau as the head of its new "pre-operational" numerical weather prediction unit. Its purpose was to indoctrinate Bureau personnel in numerical techniques by doing limited hand computations. The unit would also recommend changes to

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[663] In *Thor's Legions*, author John F. Fuller claims it was the Air Force that was pushing for a joint numerical weather prediction unit, that Reichelderfer and Wexler did not think NWP was ready to go operational, and that the position paper presented by Reichelderfer to the JMC was actually written by the Air Weather Service in the spring of 1953. The archival evidence shows otherwise. The Weather Bureau had already started to move into operational NWP within a month of the August 1952 meeting – a meeting at which the Air Force *declined* to go operational in a joint unit. Indeed, the AWS did make overtures to other participants in the spring of 1953, but only because it feared being left out of the program. See Fuller, *Thor's Legions*, 222 (and footnote 35).

observational methods based on anticipated data needs.[664] Smagorinsky was pleased to accept. He would join the new unit in January 1953.[665]

Since Smagorinsky had more experience with NWP than anyone else at the Bureau, he created his own job description. However, Smagorinsky was not without help in this task. Charney provided him with a detailed list of tasks that needed to be accomplished if the Bureau had any hope of being prepared to successfully enter the operational numerical weather forecasting world. The hand calculations mentioned by Reichelderfer would be focused on Fjörtoft's 24-hour barotropic forecasts, and a two-layer model to compute initial tendencies and vertical velocities. Smagorinsky would also need to direct work on determining large-scale weather elements from numerically predicted flow fields, e.g., deducing cloud formation from vertical motion. He would direct research on problems affecting both long- and short-range prediction. The Bureau would need to do much work on data acquisition and handling, including the determination of the minimum amount of required data for successful model runs, communications requirements for collecting data and disseminating forecasts, and methods of electronically checking data and performing objective analyses. Last, and perhaps most importantly, Smagorinsky needed to introduce the "philosophy, physical basis and techniques" of numerical weather forecasting to the Weather Bureau.[666] There was

[664] Chief, Weather Bureau (Reichelderfer) to J. Smagorinsky, 8 September 1952 (Wexler papers, B32, NWP).
[665] J. Smagorinsky to Chief, Weather Bureau (Reichelderfer), 10 September 1952 (Wexler papers, B32, NWP).
[666] J. Smagorinsky to R. N. Culnan, 30 September 1952 (Wexler papers, B32, NWP).

no guarantee that even if numerical weather forecasting did fulfill its promises that it would be an easy product to sell to the Bureau's beleaguered, marginally-paid forecasters who had limited professional training in meteorology. They were accustomed to doing all analysis and forecasting by subjective hand techniques and were not likely to look with favor upon the output from a new-fangled computer. Smagorinsky would not only need to be a masterful organizer of data, communications, and computers, he would also need to be on top of model development and a salesman *par excellence* to push this project forward.

Smagorinsky had some immediate needs for his new Numerical Forecasting Group: people and equipment. He would need at least two full-time mathematics-savvy synopticians, two part-time statistical clerks, a full-time assistant, and a "simple electronic computer." Smagorinsky would work part-time on all of their projects.[667] The requirement for personnel with both synoptic meteorology and mathematics skills was a critical one. Unfortunately there were very few people who possessed those combinations of skills – especially not people who might be willing to work for the Weather Bureau.

The Bureau had other problems besides personnel and data handling to address. In order for numerical weather prediction models to work, they needed to be kept supplied with adequate upper air sounding data – data which were much more expensive to obtain than surface observations due to the cost of the weather balloons, the gas to fill them, the instrument boxes that they took up into the

[667] J. Smagorinsky to H. Wexler, 5 March 1953 (Wexler papers, B32, NWP).

atmosphere, and the tracking equipment to gather the data. Reichelderfer sent a memo to several of his subordinates addressing this issue. If they were going to use numerical weather prediction operationally, they were going to need not only *more* upper air soundings, but *more widely* distributed soundings. During the war, upper air stations had been installed all over the world. Indeed, it was the very availability of these upper air reports that had made numerical weather prediction possible in 1946. Unfortunately, that very expensive upper air equipment had been installed in developing countries which could not afford to operate it. Therefore, the money to keep those stations operational would need to come out of the U.S. foreign aid budget. In some places, each ascent cost one hundred (1953) U.S. dollars. With two ascents per day (one each at 0000 and 1200 Greenwich Mean Time) multiplied by hundreds of sites, that was an enormous cost. The Weather Bureau certainly did not have the money to keep them open. As Reichelderfer put it, "The belief of our military representatives in foreign countries that some way could be found to continue services at this high level of cost only reflects the lack of economic common sense that is all too prevalent." Having operated his own organization on a shoestring for years, Reichelderfer did not have much patience with his military colleagues who did not seem to recognize a money issue when it presented itself. Reichelderfer asked his people to develop ways of getting needed upper air information for considerably less money.[668]

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[668] Chief, Weather Bureau (Reichelderfer) to Assistant Chief, Scientific Services (Wexler) and P&PMO (Tannehill), 24 March 1953 (Wexler papers, B32, NWP).

Later in the spring, Smagorinsky visited with von Neumann and Charney in Princeton. Smagorinsky still had the same two basic worries: personnel training and data handling. The Bureau had to get ahead of the training program and make sure that it could efficiently handle incoming data for the computers or else the operational plans would never work. Thus, Smagorinsky recommended that the Bureau continue with its numerical weather prediction program as it existed, but start preparations for expansion by sending some of its employees to Platzman's ten week summer course in NWP at the University of Chicago. Platzman had been involved with the Princeton group for a number of years and knew the needs of people entering the field. His planned course would cover the logic, physical basis, and techniques of numerical forecasting. Smagorinsky argued that very few people had numerical weather prediction expertise. His goal was to ensure that the Weather Bureau maintained its perceived edge. Smagorinsky recommended that at least three Bureau meteorologists currently working in the numerical field be sent to Chicago for the course despite the tight budget situation. In a marginal note, Wexler agreed.[669]

On the data handling issue, Smagorinsky recommended that the Bureau attack this problem by pursuing the techniques and equipment for automatic data accumulation, handling, and transmission required for numerical weather prediction techniques. This idea delighted von Neumann and Charney because it provided a way to fix the thirty years' worth of "patch-work and improvisation"

[669] J. Smagorinsky to Chief, Weather Bureau (Reichelderfer) via Wexler, 29 May 1953 (Wexler papers, B32, NWP).

that had characterized the Bureau's handling of meteorological data. Von Neumann volunteered to consult on the project. Charney suggested that Julian Bigelow, former chief engineer of the IAS computer, might also make a good consultant. Von Neumann and Charney were also interested in the objective analysis work being undertaken by the Weather Bureau. The Princeton group did not have the time, computer or otherwise, to devote to this part of the numerical weather prediction problem. That the IAS computer was going to be inoperable for the next two to six months did not help the situation.[670]

In July 1953, the Weather Bureau adopted Smagorinsky's suggestion by proposing to study the Automatic Procurement and Processing of Data (APPOD). Sounding the same tone as Smagorinsky's memo – and probably written by Smagorinsky himself – the proposal argued that current data handling was terribly inefficient. When data came in via teletype, it punched a paper tape. Instead of processing the data from the tape, the data were transferred to several other media, and the original tape was thrown out. Several suggestions followed for making the entire system, from collection to analysis, more efficient, more accurate, and less susceptible to human-introduced errors.

The data handling was not a single problem – it combined several problems starting with instrument design and ending with the dissemination of the final forecast. Unless all relevant issues were addressed, the problem would still exist. In that case, a computer could generate a forecast in a few minutes, but only after

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[670] Ibid.

many hours had been expended trying to collect and feed in the data. Since a major selling point of NWP was its speed, failure to fix the data problems would eliminate much of its promise. Under consideration were automatic instruments including those that would take surface readings of wind, temperature, pressure, precipitation, clouds, radiation and visibility. The Army Signal Corps had worked on such instruments, designed to be placed in remote sites, during World War II. One idea for obtaining upper air sounding information from oceanic areas – a critical problem for numerical modelers – was to station automatically operated sites at sea which would serve as microwave communications relay stations transmitting the results of sounding apparatus dropped by rockets. However, given the amount of data needed – two observations per day – launching multiple rockets over an oceanic area for the purpose of dropping "dropsondes" was a very expensive solution, considering that Reichelderfer was not sure he could find enough money to keep overseas stations with land-based launching areas operational.[671] Once collected, if data could be transmitted and received by microwave and then written directly to magnetic instead of paper tape, that would also reduce processing time. As handled at the time, data were checked and evaluated by technicians. If the computer could check and evaluate raw data observations such that spatially and temporally inconsistent data were automatically tossed out, then it could also be used to smooth out small-scale

[671] A *dropsonde* is the same instrument as a *radiosonde* except that the former falls from high altitude to the surface and the latter is sent up from the surface with a balloon. Dropsondes may be launched from aircraft or rockets.

variations and allow for easier analysis and interpolation between grid points. Although this probably seemed like a good idea at the time, sometimes it turned out that the "odd" report *was the correct report*. If thrown out, the results of the run could be badly skewed. At some point a person would still need to be involved as a backup evaluator. However, machine assistance would certainly speed up the process. Once the computer had produced the new chart, forecasters would need efficient ways of getting a hard-copy. Automation needed to extend to this part of the process, perhaps with a mechanical plotting device. And once the plot had been made, the Bureau would need to get the product out to forecasting stations – ashore and afloat. High-speed facsimile broadcasts could be used to get the information to local forecast centers. Local forecast centers could use stored memory devices, e.g., a magnetic drum, for automatic selective broadcasts. The project had an estimated price tag of $55,800; it would fund a staff of eight meteorologists and other technical specialists. Von Neumann, Bigelow, meteorologist Athelstan F. Spilhaus, and engineer J. C. Bellamy of the Cook Research Laboratory in Chicago would be the consultants.[672]

Although the Bureau was not ready for operational NWP, it was on the right path. Smagorinsky had secured help with both training and data handling – issues that had not been on the table for the Princeton team. Now it was just a matter of making it happen both in-house and with the military weather services.

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[672] Proposal for a Study Project on Automatic Procurement and Processing of Data (APPOD), Weather Bureau, 15 July 1953 (Wexler papers, B32, NWP).

JOINTLY SPEAKING

Having a meeting and deciding to "go joint" was one thing – successfully bringing it about was another matter entirely. Such an undertaking had to involve participants who could set aside their own personal agendas and concentrate on successfully melding people, institutional cultures, equipment, spaces, and funding from different sources with a minimum of in-fighting. If the proposed joint numerical weather prediction operational center were to become a reality, those interested in making it happen would need to move early and keep abreast of the situation. The Navy had moved first.

In early October 1952, Commander Daniel F. Rex of the Office of the Chief of Naval Operations, advised Phillips that he had no corrections or objections to the minutes of the 5 August meeting which had addressed operational numerical weather prediction (NWP). Even though some felt the time was not right to pursue operational NWP, the Navy intended to raise the subject through a number of "Washington committee structures." Among them were the Joint Meteorological Committee (JMC) and the Subcommittee on Aviation Meteorology of the Air Coordinating Committee (ACC/MET). The former focused strictly on government issues, while the latter represented civilian concerns as well.[673] A month later, in another letter to the Meteorology Project, Rex reiterated the Navy's intention to get a joint agreement among the Navy, Air Force and Weather Bureau concerning the

[673] Rex to Phillips, 7 October 1952 (Charney papers, B14, F465 - attached to Stickles letter of 15 June 1953).

organization, scope and objective for a "national numerical forecasting (or computing) center."[674]

Then in May 1953 while Smagorinsky was visiting von Neumann and Charney in Princeton, the Air Weather Service's Colonel George F. Taylor dropped by to discuss operational NWP. Recalling that the Air Force had not been an enthusiastic supporter of a joint operation during the August 1952 meeting, Taylor carefully avoided an "official" stance while quietly pressing for a joint operational group. He acknowledged the Bureau's poor fiscal situation; the military services would have to provide most of the funding. Taylor supported forming a committee under the auspices of the JMC or ACC/MET if it would have some real authority to direct action. He most emphatically did not support the establishment of a powerless advisory committee. Smagorinsky subsequently recommended to Reichelderfer that the Bureau work toward forming an operational joint unit with the Navy and Air Force as soon as possible so that they could place an order for an IBM 701 type "high speed calculator."[675]

Within the month, the Weather Bureau acted on Smagorinsky's suggestion. The recommendation: that the JMC create an *ad hoc* committee to draft a plan for a joint operational NWP unit to be established by 1 July 1954. Since the WBAN Analysis Center already existed as a model of a joint meteorological forecasting venture, the Weather Bureau argued that it made sense to establish a similar

[674] Rex to Charney, 24 November 1952 (Charney papers, B14, F465 - attached to Stickles letter of 15 June 1953).
[675] J. Smagorinsky to Chief, Weather Bureau (Reichelderfer) via Wexler, 29 May 1953 (Wexler papers, B32, NWP).

organization for numerical weather prediction. There were so many potential fiscal, technical, equipment, and personnel difficulties, that only by working together were the three weather services likely to see operational NWP in the foreseeable future. Indeed, the Weather Bureau commented, "It has been reported that workers in Sweden, England, and Germany (W.Z.) plan to use those computers available for operational use by January 1954."[676] The possibility of being overshadowed and outperformed by European groups undoubtedly provided some impetus to get the project moving. By the middle of 1953, all the weather services were actively pushing for some kind of joint operational approach to numerical weather prediction. The next step was coming up: the establishment of what would turn out to be a series of *ad hoc* committees.

THE AD HOC COMMITTEES

The JMC was originally established during the early years of World War II to address the provision of meteorological support during a national emergency. All of the military services and the Weather Bureau were represented. Although it could have been a problem having a civilian agency head on a military committee, it was not: Reichelderfer was a retired Navy officer, and his knowledge of military missions and requirements made him a valuable member of the JMC. His position as a JMC member also provided a venue for advocating for the new joint unit. Had Reichelderfer not been a JMC member, it might have taken more time to sell a

[676] Supporting Paper by U.S. Weather Bureau Member (Reichelderfer) on Numerical Weather Prediction for the Joint Meteorological Committee, 10 June 1953 (Wexler papers, B32, NWP).

strictly military committee on the importance of a still operationally unproven method of weather prediction.

Within two weeks of receiving the Weather Bureau's point paper on numerical weather prediction, the JMC created the Ad Hoc Committee on Numerical Weather Prediction. It was composed of representatives from each weather service and chaired by Rex, who had arranged for the original funding for the Meteorology Project while assigned to ONR in 1946. After several preliminary meetings, the Ad Hoc Committee members arranged to hold a conference at the Pentagon (where they worked) on 10 August 1953 with authorities in the numerical weather prediction field. Besides von Neumann, the committee invited meteorologists Charney, Gilchrist, Berggren, and computer engineer Bigelow from IAS.[677]

A very interested Von Neumann was, unfortunately, out of town and unable to attend the meeting. Extremely pleased that plans were moving forward, he requested copies of the minutes so he could provide comments at a later date.[678] Writing to Charney about the upcoming meeting, von Neumann expressed great pleasure in the "'joint' character of the enterprise." He thought it very important for the "enterprise in question" and for the future of the Meteorology Project that Charney participate – which of course he did.[679]

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677 Rex to von Neumann, 22 July 1953 (von Neumann papers, B15, F4).
678 Von Neumann to Rex, 3 August 1953 (von Neumann papers, B15, F4).
679 Von Neumann to Charney, 29 July 1953 (Charney papers, B16, F516).

Less than two months after its creation, the Ad Hoc Committee had developed, with the help of its distinguished panel of consultants, a detailed plan for creating an operational numerical weather prediction unit by the 1 July 1954 deadline.[680] Four goals had to be met before opening the unit: model development, computer acquisition, personnel training, and the finding of a suitable location. The first goal had been met because of the Meteorology Project's work. Numerical output (analysis and prognosis) had shown sufficient skill (based on placement of high and low pressure systems) to make it competitive with the best subjective methods. That was a very optimistic conclusion, but one that had to be made in order to keep the project viable. Had the conclusion been that the models could not compete with subjective methods, the proposal to establish the joint unit would have quickly died. To meet the second goal, the weather services would need to secure a computer. Few computers could handle the meteorology problem in 1954 and in any case they were not available "off-the-shelf." The International Business Machines Corporation (IBM) had produced a Type 701 "electronic computer" – closely modeled after von Neumann's machine – which could be used for meteorological work. IBM could have a leased version of this machine ready by 1 October 1954. The third goal – sufficient trained personnel – had been met because the three weather services had identified enough meteorologists and meteorological analysts to serve with the Unit. The last goal – a location to house the unit –

[680] Minutes of the sixth meeting of the Ad Hoc Committee on Numerical Weather Prediction held 11 August 1953. Members of the committee: Chair: Commander Daniel F. Rex, USN (OPNAV); Majors W. H. Best and T. H. Lewis, USAF, (Air Weather Service); Drs. H. Wexler and J. Smagorinsky (U.S. Weather Bureau).

appeared to have been met based on information provided by the JMC itself. JMC advised the Ad Hoc Committee that space would be available near the WBAN Analysis Center – which had been in the decrepit Weather Bureau headquarters building, but was moving to new spaces in Suitland, Maryland in the spring of 1954.[681]

While everyone concerned agreed that it was best to "go joint," in reality military and civilian organizations operated differently. In particular, military personnel were frequently reassigned. A high turnover rate among personnel would not be advantageous to the Unit's success. Therefore, the military services involved were encouraged to extend the "tours," i.e., assignments, of their personnel for as long as possible.[682] This was a critical issue. Most military assignments were only two years long – some were even shorter. Without the tour extensions, military personnel would be leaving for a new assignment just about the time they became productive members of the team.

With the time ripe to form an operational unit that was without precedent, the Committee wanted to ensure that the unit's organizational structure would be flexible enough to quickly adapt new research results to its operational program. The operational nature of the unit would produce results which would need to be

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[681] Report by the Ad Hoc Committee on Numerical Weather Prediction to the Joint Meteorological Committee on Joint Numerical Weather Prediction (NWP-10-53), 12 August 1953 (Wexler papers, B32, NWP). The technical consultants were from (1) IAS: John von Neumann, Roy Berggren, Julian Bigelow, Jule Charney, and Bruce Gilchrist; (2) IBM: C. C. Hurd, George W. Petrie, and John Sheldon; (3) University of Chicago: Sverre Petterssen and George W. Platzman;(4) Bureau of Standards, Charles B. Thompkins. Major P. D. Thompson, USAF was an "unaffiliated member."
[682] Ibid.

closely and carefully examined. The weather services would then need to follow a course of action that would lead to the ultimate success of numerical weather prediction.

The Committee thus recommended that work on the joint unit move forward. The Weather Bureau would take administrative responsibility. All three weather services would provide funding. Its purpose fulfilled, the Ad Hoc Committee members proposed establishing a new Steering Committee. It would be responsible for the selection of a director for the new joint unit and help him implement the plan.

The mission of the Joint Numerical Weather Prediction Unit was:

> To produce on a current, routine, operational basis, prognostic charts of the 3-dimensional distribution of relevant meteorological elements by using numerical weather prediction (NWP) techniques, in order to improve the meteorological forecasting capabilities of the participating weather services.[683]

Operationally, the Unit would analyze and process data for NWP which could not be, or was not already being, undertaken by the WBAN Analysis Center. It would compute prognostic charts and create products from numerical output which would be most beneficial to field forecasters. Additionally, the Unit would verify the computer generated products – monitoring quality and making suggestions for further improvement. It would develop objective analysis methods and improve data handling techniques, extend models geographically, and adapt models for longer forecast periods. The Unit would liaise with other organizations, particularly

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[683] Enclosure: Plan for Joint Numerical Weather Prediction Unit, 12 August 1953 (Wexler papers, B32, NWP).

those conducting NWP research, and determine the applicability of new research results to operational models. It would also conduct in-house training of personnel to maintain optimal personnel proficiency.

In the summer of 1953, the Ad Hoc Committee concluded that the IBM 701 was the best computing machine available. In fact, Committee members did not even discuss any other options and were perhaps at that stage unaware of any other options. What they did know was that by submitting a letter of intent before 30 September 1953, the computer could be available by 1 October 1954 – a full year later and three months after the JNWPU was to be established. To be co-located with WBAN, the facility needed at least 4500 square feet of floor space with a minimum of 1000 square feet set aside for the computer. After an initial outlay of $94,500, the Ad Hoc Committee estimated the budget for the first year of operations to be $415,000: $193,000 for personnel, $200,000 for the IBM computer, and $22,000 for miscellaneous expenses.[684]

Since the Unit was starting from nothing, the Ad Hoc Committee anticipated a three month "shakedown" period. During that time, the Unit would prepare and distribute one set of (unspecified) prognostic charts daily. Unit members would focus on the development and standardization of an operational routine. By placing the JNWPU next to the WBAN Analysis Center, the services hoped to eliminate duplication of effort – a long-time issue in the U.S. government's provision of meteorological support to the nation. Concerns over so-

684 Ibid.

called duplication of effort in the past had decimated military meteorological organizations at the end of periods of national emergency. The Weather Bureau, always low on the funding scale, would not have wanted its efficiency questioned yet again on this high-visibility, high-cost project with uncertain results. The WBAN could provide plotted maps to the Unit and would provide additional data as needed. Although the Unit might need to perform its own analyses to meet specific NWP requirements, i.e., any analyses needed to provide initial grid point values, WBAN analyses would be considered and used once the Unit had tested them and found them acceptable.[685]

In their pursuit of a routine schedule, Unit personnel would use a simple atmospheric model to develop the first prognoses. These would include the constant-pressure surface at several levels in addition to vertical velocity, average temperature, and perhaps large-scale rates of precipitation in a chart presentation. The charts would be available to WBAN, but the individual services could distribute them within their own systems. The services planned to transmit the numerically produced prognoses via their facsimile channels.[686]

Realizing that the Unit would need a strong verification program in order to effect improvement, the plan called for the evaluation of their prognoses without detailing exactly what methodology would be employed.[687] The Unit would not actually develop models. Instead it would take models developed by R&D sites and

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[685] Ibid.
[686] Loc. cit., Appendix, p. 8., para. 1.b - d.
[687] Loc. cit., Appendix, p. 8, para 2.

adapt them for operational applications. Unit members would be able to extend the geographical area of the models; their goal was hemispheric coverage. The Unit would also be permitted and encouraged to extend the forecast period. Members would consider data processing improvements separately, with much of the emphasis on objective analysis techniques.[688]

Because of problems inherent to joint organizations, there had to be a forum for addressing inter-agency issues. These would be reported to the proposed Steering Committee on Numerical Weather Prediction (SCNWP) under the cognizance of the JMC. The SCNWP, composed of representatives from each service, would hear problems concerning service personnel, requirements being placed on the Unit, and technical matters external to the Unit. For example, each service would have mission requirements which demanded a particular product. If the Unit tried to meet too many of these service demands, it could find itself unable to complete its primary mission. The Steering Committee would act to sort out such conflicting requests. The director of the JNWPU would report to the Weather Bureau Chief on all administrative matters including finance, civilian personnel, and logistical support. Finally, a scientific advisory group composed of subject matter experts, e.g., meteorology, electrical engineering, mathematics, would visit periodically and provide technical advice to the Unit.[689]

Personnel would include the director, an assistant, and a mix of professional and technical workers. The director would have a broad background in both

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[688] Loc. cit., Appendix, p. 9, para 3.
[689] Loc. cit., Appendix, Section III, para.1, p. 9.

synoptic and theoretical meteorology in addition to having previous experience using mathematical and physical techniques in weather prediction. He would also be familiar with basic programming techniques. His assistant, who would help administer the unit, just needed to be "conversant" in the basic NWP concept, i.e., did not need to be a meteorologist. But the person would need "tact" in order to facilitate the mission. The requirement for "tact" was probably a bit of an understatement. Trying to overcome the inherent inter-service rivalry between three competing weather services would not only take tact, but a huge amount of patience.

The remaining personnel would have meteorological and/or mathematics backgrounds. To get the Unit off to a good start, at least some of the incoming personnel would have to come from the ranks of those training with the Princeton or Stockholm groups, or working with the Thompson's Cambridge group. Otherwise, the experience level would be too low. Of great importance was the need for meteorologists conversant with both dynamics and synoptics – a combination that had not been encouraged in earlier years. The six meteorologists collectively, although not individually, would have strong dynamic meteorology and mathematics abilities, and be familiar with machine computations. However, they also needed to have extensive synoptic experience and knowledge of advanced prognostic techniques with "proven ability to carry out independent developmental research." These were definitely not entry level positions. By the very nature of the position description, the Unit needed to bring in people who had already been working on the developmental stages of NWP. The mathematician would need extensive experience in numerical analysis and the programming of complex physical programs. He would be joined by three programmer-coders who would also be strong mathematicians with programming experience. The meteorological analysts would perform synoptic map analysis. They were expected to be skilled synopticians, preferably with training and experience in dynamic meteorology, and have sufficient general knowledge of NWP to be useful team members. A number of lower-level technical positions rounded out the personnel: computer operators, meteorological aids to plot data, plotters to check and plot data, and a secretary to support the Unit's administrative needs.[690]

Securing the computing machine and providing a properly engineered space for it would be two key challenges facing the new Steering Committee. This Committee would be faced with three basic options for obtaining the required computing machine: build it, purchase it off-the-shelf, or lease it. Ordering a custom-built computer was a very expensive option that would limit the Unit's flexibility given the rapid pace of computer development. Buying a commercial computer "off-the-shelf" would also limit the Unit's flexibility to upgrade as newer, more advanced computers came on-line. Thus, leasing the machine was the best approach. The Unit could then make equipment upgrades without the large investment of funds. Perhaps just as important, the providing company would handle the maintenance. Thus, the Unit would save on manpower costs and avoid

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[690]Loc. cit., Appendix, Section III, p. 12-13.

the problems of finding and hiring qualified people (of which there were probably few in 1953) to maintain the computer and associated peripheral equipment. The Committee thought the IBM 701, known within the company as the "Defense Calculator," was the best choice. Designed to meet the demands of the Defense Department and the aerospace industry – which, indeed, used almost all of the nineteen extant machines – the design logic and high speed memory were virtually the same as the IAS computer.[691] With an extremely flexible input/output scheme and a promise from IBM to cooperate on automatic data processing development, the 701 was clearly superior to other options.

The IBM 701 came with its own set of siting requirements which the Unit had to address before accepting delivery. Sufficient heating, lighting and power systems were available in the spaces adjacent to the WBAN. However, the required 30 tons of air conditioning were not. The room needed to be retrofitted with a raised floor to allow for cabling and air conditioning ducts. In addition, the IBM engineers would need an engineering room close by for themselves and their equipment.[692]

Wexler, the Weather Bureau's representative on the Ad Hoc Committee, was determined that nothing should get in the way of this project's forward movement at this late stage. Advising his boss, Reichelderfer, of the financial and personnel burden the Weather Bureau could anticipate sharing, Wexler argued that

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[691] Ceruzzi, *History of Modern Computing*, 34.
[692] Enclosure: Plan for Joint Numerical Weather Prediction Unit, Appendix, Section IV, Para 1 and 3, 12 August 1953 (Wexler papers, B32, NWP).

NWP was "no longer the 'meteorological oddity' of L. F. Richardson's pioneering efforts of 1922." Richardson's ideas for forecasting the weather by numerical means were viewed with some interest in 1922, but quickly abandoned as being completely impractical in a world where all computations were done by hand. Wexler assured Reichelderfer that the current approach to numerical weather prediction, as developed by the Princeton group, was sound. As proof of its "soundness," Wexler pointed to the independent tests being performed in a number of countries and mentioned the operational approaches being undertaken in Sweden (by Rossby's group), England (by the British Meteorological Office), and in the western zone of Germany (by Deutscher Wetterdienst). The foresighted Wexler envisioned NWP as the future nucleus of Weather Bureau's forecasting efforts. He expected forecasts to become available at lower cost as computers became faster and more efficient. Wexler also argued that the Weather Bureau "should not become the 'poor silent relative'" and should make sure everyone knew that it had carried its fair share of the financial burden.[693]

Reichelderfer not only needed allies within the military services to bring the JNWPU to operational reality – he needed allies within the Weather Bureau's umbrella organization: the Department of Commerce. Without support for the Weather Bureau's budget, including any last minute increases to cover the costs of the new Joint Unit, the Bureau would be unable to fulfill its obligations to the other

[693] Wexler to Reichelderfer, 12 August 1953 (final), 31 July 1953 (initial) (Wexler papers, B32, NWP). Between the initial and final report, the Weather Bureau's share of the budget decreased from $144,000 to $139,000. The number of professional meteorologist positions, to be shared among all three services, dropped from sixteen to thirteen.

weather services. Writing to the Honorable Robert B. Murray, Jr., Under Secretary for Transportation, Reichelderfer reiterated the discussions of previous meetings wherein he had set forth the future of forecasting by electronic computers. Acknowledging that the forecasting technique of the moment was the forecaster's personal judgment based on the data as he saw them, Reichelderfer maintained that this new objective technique would eventually, if slowly, lead to more accurate forecasts in the support of aviation, agriculture, and other areas of economic interest. In other words, NWP would ultimately be of benefit to the commercial sector through increased safety of flight, reduced losses of agricultural products both in the ground and en route to market, and improved timing of business decisions dependent upon the weather. Yes, the equipment would be expensive, but future personnel reductions (due to the work being done by the computers) would provide the savings to pay for the new hardware. He reassured the Under Secretary that the Bureau would be able to remain within their fiscal year 1954 budget appropriation. A note at the bottom to "Interested Project Leaders" made clear that the Weather Bureau intended to fully support numerical weather prediction.[694] The unspoken message was, "and I expect that you will support it also."

Another major issue the establishment of the Joint Unit raised was a familiar one in 20th century technology: whether increased mechanization would lead to a reduction in the number of workers. It is not surprising that Reichelderfer made the case for automation as a way to reduce the number of people required to

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[694] Reichelderfer to R. B. Murray, Jr., 14 August 1953 (Wexler papers, B32, NWP).

produce forecasts. Due to considerable fiscal belt-tightening occurring in the Eisenhower administration, less money, not more, would be available in the future. Thus, governmental agencies needed to become more "efficient." Personnel reductions not only saved money in the near-term, they saved money in the long-term by eliminating pension costs.[695] However, this argument in support of possible personnel reductions was in direct opposition to the statement made by Smagorinsky during his presentation on the Radio WGY Science Forum. At that time, he had argued that there would be no reduction in the number of meteorologists at the Weather Bureau because they would still be needed to put out the local forecasts. Numerical weather prediction would just provide them with more reliable information than was currently available from subjectively produced charts.[696] Perhaps Reichelderfer thought that they would need less manpower for the routine work done by technicians in plotting and preparing the raw data for analysis. However, as in many cases where automation was considered the savior of personnel costs, the addition of computers would lead to reassignments, not personnel reductions.

By the end of August 1953, the JMC had still not decided whether the JNWPU would be an independent entity or subsumed within the WBAN Analysis Center. The JMC's Air Force representative had expressed a preference for

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[695] Reichelderfer to Project Leaders, 28 August 1953 (Wexler papers, B32, NWP).
[696] Joseph Smagorinsky, "Numerical Weather Prediction," address on the WGY Science Forum, 20 May 1953 (Wexler papers, B32, NWP).

integrating the Unit within the Analysis Center.[697] However, the Navy was not yet willing to make a binding commitment, and wanted more time to discuss it in-house. The Weather Bureau's representative, Smagorinsky, thought the decision hinged on a possible change to the external structure of supervision for the Unit, i.e., how it would relate to the steering and advisory committees. Or on a third possibility: "horse-trading." The military services were apprehensive about the Weather Bureau funding its full share. Their concern surprised Smagorinsky, who did not view funding as a problem. He was more worried about space issues. After all, the Weather Bureau had to move the WBAN into a new building and determine how the Joint Unit would be co-located with it. A delay in the WBAN move would adversely impact the Unit. Smagorinsky also reported some unanticipated equipment problems. The decision to use the IBM 701 had been called into question. Securing the computer might not be as easy as just signing a letter of intent to lease.[698]

The Eisenhower Administration's emphasis on fiscal conservatism soon threatened to derail the computer acquisition plans. Reichelderfer received a query from Assistant Secretary of Commerce James C. Worthy wanting to know why the JNWPU could not use the Bureau of Census Machine Tabulation Facilities instead of a dedicated computer. If the Weather Bureau did plan to use the Census facilities, Worthy wanted a detailed description of the "nature and scope" of the proposed usage. Reichelderfer needed input from his division heads in order to

[697] Reichelderfer to Project Leaders, 28 August 28 1953 (Wexler papers, B32, NWP).
[698] J. Smagorinsky to H. Wexler, 19 August 1953 (Wexler papers, B32, NWP).

answer these questions.[699] Attempts to save money by sharing computing equipment would be problematic if the Unit were expected to do its runs around other agencies' needs.

Wexler took on the response to Worthy. He forcefully argued that the Census facilities were inappropriate for either the Unit or for the extended forecast division. The Unit needed a machine that matched the capacity and speed of the IBM 701. The Census machine did not meet these basic requirements. Additionally, the Unit would need to use the computer 70 hours per week on a very firm schedule. As the operations become more successful and their numbers increased, Wexler anticipated that the run-time might double after the first year. Even in the early shake-down stages, Unit personnel could not be kept waiting for the Census Bureau staff to finish running their tabulations. The whole idea of behind the creation of an operational unit was to meet operational demands. Weather forecasts could not wait for the kind of non-time-critical statistical calculations of importance to the Census Bureau. Therefore, a shared machine was absolutely out of the question. Wexler's other concern was for the extended forecast section's computer requirements. Due to short lead times, the extended forecast group had to be co-located with its computer. Often the data were ready for processing just a short time before the run. If computer sharing with the Census Bureau became a reality, there would only be two options: move the extended forecasting section to the Census Bureau, or haul the data on punched cards to the

[699] F. W. Reichelderfer to Division Heads, 21 August 1953 (Wexler papers, B32, NWP).

Census Bureau. Either option was untenable. It made no sense to move extended forecast meteorologists away from the rest of the Weather Bureau professional staff. Likewise, driving numerous large decks of cards around the greater Washington, D.C. area during the rush hour, nasty winter weather, or other traffic disasters, would adversely impact the creation of a timely product. Worse yet, what if someone dropped the decks of cards? Huge amounts of time would be wasted. The Weather Bureau required a dedicated computer.[700]

Wexler was very sensitive to Reichelderfer's fiscal worries. Therefore, Wexler presented the argument in favor of leasing as being the most fiscally responsible choice. The computing power required to do meteorological work, both to run realistic atmospheric models and to automatically process data, was so great that existing computers were only marginally able to meet the challenge. The Meteorology Project had already come to that conclusion while running their simple models on von Neumann's machine. Continuous design improvements on computers meant that each new upgrade ran faster and had more memory than any previous models. That being the case, it did not make good economic sense to purchase a machine. It would be outmoded very quickly and if they purchased it, the Weather Bureau would be responsible for the maintenance. In Wexler's view, when the "situation stabilizes," i.e., when computer design slowed so that new, faster models were not continuously being made available, then it would make

[700] Wexler to Reichelderfer, 27 August 1953 (Wexler papers, B6, F1953).

more sense to purchase one.[701] Reichelderfer concurred in Wexler's assessment. However, Reichelderfer argued that government officials, including Congressional authorities, Bureau of the Budget personnel, and high ranking members of the Executive Branch were convinced that too much money was being spent on "machine tabulation equipment." Reichelderfer, and other agency heads, were under pressure to share this equipment whenever possible. Therein lay the suggestions by Worthy that the Weather Bureau share the computers already in place at the Bureau of Standards and the Bureau of the Census. Reichelderfer did not want to appear uncooperative with efforts to economize on computing equipment by insisting that his organization required a dedicated computer. Therefore, he proposed that the Weather Bureau "not give the appearance of obstructing the plan in the beginning by starting off with reasons why we cannot do it." He seemed to think that the reasons would "speak for themselves" once the requirements were reviewed by the Census Bureau and the Department of Commerce.[702] Counting on other agencies to see the wisdom of his thinking was, however, somewhat risky. They already owned the machines – Reichelderfer would be the one coming hat-in-hand. If Census and Standards had thought the Weather Bureau would take over their machines, then he could have counted on their support as well for a dedicated weather computer.

Even as the United States' JNWPU was finalizing its plans to take numerical weather prediction operational, the Swedish group was making rapid

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[701] Wexler to Reichelderfer, 3 September 1953 (Wexler papers, B6, F1953).
[702] Reichelderfer to Wexler, 8 September 1953 (Wexler papers, B6, F1953).

progress as well. Phillips wrote from Stockholm that their computer, BESK, was almost finished. Rossby's team members was just waiting for the completion of the input mechanism so they could start making calculations. Although the new magnetic drum would not be ready for a while, Rossby's group intended to start without it. The BESK had the option of three different electrostatic memory sizes: 256, 512, or 1024 words (40 bit). The Stockholm group was planning on using the 512 word setup with the barotropic code using a simple Liebmann, Jacobian scheme. The code would be in three parts: Liebmann, Jacobian and transformation. Thus, each part of the code was no more than 100 words, allowing for a 20x20 grid. Rossby's on-site mathematician had wanted to use a more complicated formula, but it was less stable than the simple centered-difference formulae. Phillips thought the same was true of the formulae Rossby had been using, but had been unable to convince his colleagues that might be the case.

Elsewhere in Europe, Hinkelmann in West Germany had signed a contract with the U.S. Air Force for numerical prediction "including the building of a machine." This was an outgrowth of Thompson's European visit. The Air Weather Service wanted numerical weather prediction support for its assets in Europe, but was not able to provide them from the United States – computers were not big enough nor fast enough to process all the data. Therefore, the Air Force's solution was to establish numerical weather prediction centers, not unlike the JNWPU, wherever needed. Two members of Hinkelmann's team were in Stockholm, and

Hinkelmann was scheduled to join the Stockholm group in January 1954.[703] So despite the Air Force's desire to participate in the Joint Unit, the Air Weather Service was working to expand its NWP assets by setting up a computer unit in Europe in addition to maintaining Thompson's group in Cambridge.

The JMC ultimately approved most of the plans for the JNWPU. However, they shelved the idea of leasing the IBM 701 without a competitive bid. Dr. J. J. Eachus, a JNWPU project consultant and National Security Agency staffer, had recommended that the new Steering Committee explore the possibility of using Remington-Rand's ERA 1103 instead of the IBM 701. The JMC also decided to authorize the JNWPU to call in consultants as needed instead of creating a permanent scientific advisory group. With the JMC's acceptance of the plan proposed by the Ad Hoc Committee on Numerical Weather Prediction, the latter's work was done. It was dissolved and a new Ad Hoc Group for the Establishment of a Joint Numerical Weather Prediction Unit (en lieu of a steering committee) was created with Rex, once again, as chairman. He was joined by Air Force Major T. H. Lewis representing the Air Weather Service, and Wexler representing the Weather Bureau. Since they had also composed the first *ad hoc* committee, any change was in name only.

The Group's first task was to select a director for the JNWPU.[704] The members unanimously recommended Air Weather Service meteorologist Dr.

[703] Phillips to Charney, undated, ca. Fall 1953 (Charney papers, B14, F449).
[704] Joint Meteorological Committee (JMC-78-53) to Chairman, Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit, 17 September 1953 (Wexler papers, B6, F1953).

George P. Cressman (b. 1919).[705] Cressman, who had earned his Ph.D. at Chicago during Rossby's tenure, was a "well recognized" authority on synoptic meteorology, and had had experience with all three of the weather services composing the JNWPU. The Group approached Cressman informally and he agreed to fill the position if it were formally offered.[706] With the JNWPU expected to be operational on 1 July 1954, the Ad Hoc Group now had less than a year to finalize computer, funding, space, and personnel arrangements.

The computer question would prove to be a difficult one for the Group, particularly given this multi-agency scenario. Two of the three weather services were subsets of the Department of Defense; one fell under the Department of Commerce. Not only did the agencies have to agree on the computer, they had to convince their cabinet-level superiors that it was the right thing to do. This was apparently more of a problem for the Weather Bureau than for the military services which were, at the time, not as budget constrained for computing equipment. Since the new Joint Unit was a government entity, a competitive bid was required. There were very few computer manufacturers. There were even fewer computers that could handle the atmospheric problem. How would the Group make that kind of decision? Before asking the firms for bids, the Group would need to be very clear about what the computing requirements were – not only for the models, but for data

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[705] Cressman would go on to become the Director of the National Weather Service. See Hessam Taba, "George P. Cressman," *The Bulletin Interviews* (Geneva: World Meteorological Organization, 1997), 383-391.
[706] D. F. Rex, Chairman, Ad Hoc Group to Secretary, Joint Meteorological Committee (JNWP-2-53), 22 September 1953 (Wexler papers, B6, F1953).

handling. These areas were still works in progress. There was no guarantee that, in the year it took to build the chosen computer, they would not have a "better" model no longer able to comfortably run on it. While dealing with external inquiries about why they needed a dedicated computer, the Ad Hoc Group asked consultants to help them make a decision that would shape the early success or failure of the JNWPU.

Thus started the quest for a competitive bid in an era when there were few potential bidders. To enable the Group to make an intelligent computer choice, Smagorinsky invited the only two firms with competitive machines – IBM and Remington-Rand – to perform preliminary tests to demonstrate the capabilities of their machines. The Group members invited von Neumann, Charney, Petterssen, Eachus, Bigelow, and Platzman to serve as an informal technical advisory committee and to help them analyze the results[707]

Group members, Cressman, Smagorinsky, and company representatives met in early October 1953. The Group asked both companies to run the three-dimensional quasi-linear model. IBM had a 701 ready to run such a model, but would not encode it for the 701 without compensation. The IBM representatives said they had a 701 available for the test run in the Washington, D.C. area that was already operating two eight-hour shifts per day, five days a week at 75% efficiency. IBM could still deliver a 701 to the JNWPU within a year of receiving a letter of intent. Remington-Rand offered to do the model coding for free, but did not have

[707] JCS/JMC Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit (JNWP-1-53) Minutes of the first meeting held 22 September 1953 (Wexler papers, B6, F1953).

an ERA 1103 available to run it. (Exactly how they expected to run a competitive test without a computer is a mystery. According to Paul Ceruzzi, Remington-Rand, which had acquired both UNIVAC and Engineering Research Associates (ERA), "did not fully understand what it had bought."[708] Consequently, it did not know how to market its computers.) The Remington-Rand representatives did not know when they could deliver an operational computer or how they would handle maintenance issues, but agreed to get back with those answers.

Smagorinsky and Goldstine were designated as "fact finders" – they would determine the suitability of the computers for the meteorological task and report back to the technical advisors.[709] Since Goldstine was already under contract to ONR, Rex asked ONR to make his services available.[710] Within a couple of weeks, Remington-Rand had found machine time and IBM had identified a program that could be run on its 701. The competitive process continued.[711] It appeared that the test runs could start in December and should be completed by the middle of January 1954. [712] The technical advisors would meet at IAS, after the runs were done, to make a decision which was hoped to occur before 10 February.[713]

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[708] Ceruzzi, *History of Modern Computing*, 45.
[709] JCS/JMC Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit (JNWP-11-53) Minutes of the second meeting held 6 October 1953 (Wexler papers, B6, F1953).
[710] Chairman, Ad Hoc Group for the Establishment of a JNWPU to Chief of Naval Research (JNWP-9-53), 8 October 1953 (Wexler papers, B6, F1953).
[711] JCS/JMC Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit (JNWP-11-53) Minutes of the third meeting held 12 October 1953 (Wexler papers, B6, F1953).
[712] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 4th meeting held 28 October 1953 (JNWP-18-53) (Wexler papers, B6, F1953).
[713] JCS/ JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 6th Meeting held on 7 December 1953 (JNWP-27-53); JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 7th Meeting held on 7 January 1954 (JNWP-4-54) (Wexler papers, B32, NWP).

Goldstine and Smagorinsky filed their tentative report at the end of January. Their report was based on the companies' advertising material, personal inquiries, and the test run of the model on each machine. To give the technical advisors enough material with which to make a decision, they outlined modeling, data handling, and other issues which would have an impact on run times.

The computer selection would depend heavily on the kinds of models the JNWPU would run within its first year. There was no point in selecting a machine that could not handle the initial modeling and data handling requirements. It made no sense to choose a machine that could handle the initial models, but would not be able to run models incorporating larger geographical areas, additional variables, or increased forecast periods. Smagorinsky and Goldstine anticipated that the first year's models would include large-scale motions, assume an adiabatic and frictionless atmosphere, and could consider an irregular lower boundary. In mathematical terms, the model represented an initial value problem wherein the geometric boundary conditions were specified at all times. It would have three internal vertical grid points, be quasi-linearized, and have a level lower boundary, i.e., it would not consider topography. The computer would have to solve two-dimensional elliptic Helmholtz equations in successive times. Any inhomogeneous terms would require Jacobian operations to be applied to functions of earlier solutions of Helmholtz equations. The more general model would have 5 to 7 vertical grid points, an irregular lower boundary, and would require the solution of the three-dimensional Poisson equation. The inhomogeneous terms in the Poisson equations would require additional two-dimensional Jacobians. With a horizontal lattice of at least 20x20, Smagorinsky and Goldstine estimated that the run time would be at least five times greater than that required by the quasi-linear, three-level model.

Manual data handling was far too slow and inaccurate for later calculations. Automatic data processing was a non-negotiable requirement. This would require the inversion of about 1000 10x10 symmetric matrices and would require significant amounts of machine time.

Increasing geographic coverage, including moisture distributions for precipitation forecasts and three-dimensional trajectories for condensation computations, would also increase run times. Given these possibilities, Smagorinsky and Goldstine thought it likely that within a few years the time requirements on the computer would be an order of magnitude greater than for the test problem. Since operational predictions would require a faster run time, the JNWPU's ability to function effectively would depend on the availability of newer, faster machines.[714]

On a chilly January day in Princeton, the technical advisors, along with the Ad Hoc Group and Goldstine, Gilchrist, Glen Lewis (all from IAS) and Lieutenant

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[714] Tentative Report to the Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit, The ERA 1103 and the IBM 701, 26 January 1954 (Charney papers, B16, F 522). Although it is not signed, this was the report produced by Smagorinsky and Goldstine.

Commander C. A. Palmer (ONR), met at IAS to choose the computer. [715] Von Neumann chaired the meeting. Rex provided background information. He stressed the importance of leasing the computer for at least a year – an earlier change would disrupt the operation. Then Smagorinsky and Goldstine presented their report. Discussion followed. The computers handled the test problem with virtually the same run time. Remington-Rand's ERA 1103 showed faster internal calculations, but the IBM 701 had faster output so there was no significant end-result difference. As model complexity increased, both machines would reach the limit of their processing capabilities at about the same time. Because of large data input and output requirements, it was important to have rapid printer output. The 1103 did not have an integrated high-speed line printer. Meeting attendees concluded that the 701 would likely have a better maintenance program because IBM, with more of the machines on-line, had had significantly more experience with maintaining the machines. Since both bids were essentially the same, the more reliable IBM machine would be the better choice.[716] Therefore, with the one year lead time rapidly shrinking, the Group recommended the selection of the IBM 701 "Defense Calculator."[717]

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[715] The list of technical advisors had grown and at this point included von Neumann, Charney and Bigelow (all from IAS), Tompkins (Bureau of Standards), Eachus (National Security Agency), mathematician Mina Rees (Hunter College), and C.V. L. Smith (ONR).

[716] Minutes of the First Meeting of the Technical Advisory Group, Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit (JNWP-6-54) (Wexler papers, B6, F1953).

[717] Draft JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 8th Meeting held on 28 January 1954 (JNWP-7-54) (Wexler papers, B32, NWP). Although this information did not appear in the final version of the minutes, the draft minutes listed all the equipment: 1 electrostatic frame, 4 magnetic tape frames, 1 magnetic drum frame, 1 card reader, 1 card punch, 1 high-speed line printer, and 1 power and control unit.

About the time Reichelderfer had convinced Worthy that the Weather Bureau absolutely had to have a dedicated computer, another Commerce Department bureaucrat – Under Secretary for Transportation Murray – weighed in with his off-the-wall question: Why not just use the best parts of the IBM and Remington-Rand computers to form a computer better than either of them were individually? Once again, Wexler was stuck researching the question. He turned to Eachus to find out the feasibility and cost of such an undertaking. At least Reichelderfer could answer Murray's other question: Why not just purchase the machine? Reichelderfer was fully aware that either machine would be obsolete too soon. However, they could not wait two to three years for the next generation of computers to arrive before moving ahead with numerical weather prediction.[718]

Having made its decision, the Group notified IBM of the 701's selection. Smagorinsky and Cressman were scheduled to attend a training seminar at IBM. While there, they would give the IBM staff more information on the history and future of NWP. The Weather Bureau's financial maven – Robert N. Culnan – negotiated the final details and sent the letter of intent.[719] Only four months remained until the JNWPU would open for business.

At the end of May, just when things looked settled for the computer, IBM announced their new, improved computer – the 704. If desired, IBM would

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[718] Reichelderfer to Project Leader (Smagorinsky), 19 February 1954 (Wexler papers, B6, F1954).
[719] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 10th Meeting held on 26 February 1954 (JNWP-12-54) (Wexler papers, B32, NWP); G. W. Petrie, IBM to George Kressman (sic) and Joseph Smagorinsky, 4 March 1954 (Wexler papers, B32, NWP); JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 11th Meeting held on 11 March 1954 (JNWP-16-54) (Wexler papers, B32, NWP).

substitute the 704 for the 701 ordered by the Weather Bureau. The Ad Hoc Group members tossed the idea around, and unanimously agreed to stay with the 701. They had two reasons: a change to the 704 would delay delivery by several more months (and they were already behind schedule due to the late selection of the 701), and the 704, besides being more expensive, did not have a proven operational track record. That the 704 was supposedly faster and more flexible than the 701 did not outweigh its negative points.[720] The official delivery date for the 701 was now 1 March 1955. JNWPU members had at most ten months to test and refine their initial model.[721]

As the opening day drew closer, personnel issues were being settled by inter-agency horse-trading of people and money. Since the Unit would have both military and civilian personnel under civilian leadership, the potential for inter-agency conflict was almost a given. However, by working closely together from the beginning, the Ad Hoc Group was trying to minimize those problems and get the JNWPU off to a good start with capable, enthusiastic personnel. Since some of the services could provide more people than others, the Group decided to trade people for cash. Weather services providing fewer people than previously agreed would make up the difference by transferring more funds to the JNWPU. The Weather Bureau was able to free up positions to cover their obligation, but the Navy could

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720 JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 14th Meeting held on 28 May 1954 (JNWP-19-54) (Wexler papers, B32, NWP).

721 Final Report of the Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit, 30 June 1954 (Enclosure (1) to the Minutes of the 15th meeting held 1 July 1954) (Wexler papers, B32, NWP).

only provide two officers: a meteorologist and a programmer-coder. This was a significantly smaller personnel contribution than the other services. Cressman still had not found a suitable programmer-mathematician, so the Group sought recommendations for possible candidates from authorities in technical fields.[722] As applications came in, they were forwarded to Cressman. He, in turn, kept the Group advised on personnel issues.[723] The JNWPU had two strikes against it from the beginning: it was an entirely untested organization creating untested meteorological products, and the personnel it needed were, likewise, entering an entirely new field which they were learning on the job. Therefore, it is truly amazing that the proposed internal structure for the JNWPU remained in place as hiring continued. Staff recruitment went smoothly, but Cressman decided not to fill sub-professional positions until a permanent home had been found for the JNWPU.[724]

By the time the Ad Hoc Group made its final report and the JNWPU became a reality in July 1954, all but three professional positions had been filled. Of the professional core, seven each were from the Weather Bureau and Air Force, while three were from the Navy. The Unit was still short one meteorologist and two operators, but interviews were in progress. The Air Force provided three sub-

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[722] Ibid.
[723] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 5th Meeting held on 20 November 1953 (JNWP-19-53) (Wexler papers, B6, F1953).
[724] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 6th Meeting held on 7 December 1953 (JNWP-27-53) (Wexler papers, B32, NWP). Final version JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 8th Meeting held on 28 January 1954 (JNWP-7-54) (Wexler papers, B32, NWP).

professional staff members and the Weather Bureau one. The remaining ten positions would be assigned out of Weather Bureau assets as needed.[725]

Just as the JNWPU was officially coming to life, the British Meteorological Office (BMO) arranged to exchange a meteorologist with the Weather Bureau. The BMO wanted their meteorologist to work with the JNWPU in order to come up-to-speed on developments in NWP. The BMO had its own proposed operational NWP group and wanted their man to get some hands-on training and experience. Thus the Unit would get one more person – and as Reichelderfer noted, it would be somebody very good.[726]

Since the Weather Bureau had administrative authority over the JNWPU, Wexler coordinated the appointment of a financial representative from within the Bureau.[727] Culnan thus became the financial coordinator and established contacts with the Air Force and Navy representatives. However, there would be no fund transfers until space modification expenses had been ascertained, and that depended on the exact location of the JNWPU. The Group anticipated a decision by early 1954.[728]

However, by January 1954 there was still no decision. That was creating problems. Without a firm location, the Weather Bureau could not develop a final

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[725] Final Report of the Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit dated 30 June 1954 (Enclosure (1) to the Minutes of the 15th meeting held 1 July 1954) (Wexler Papers, B 32, NWP).
[726] Reichelderfer (writing from Geneva) to Scientific Services Division , 28 August 1954 (Wexler papers, B6, F1954).
[727] JCS/JMC Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit (JNWP-1-53) Minutes of the 1st meeting held 22 September 1953 (Wexler papers, B6, F1953).
[728] JCS/JMC Ad Hoc Group for Establishment of a JNWPU, Minutes of the 4th Meeting held 28 October 1953 (JNWP-18-53) (Wexler papers, B6, F1953).

budget. Despite that, the Group decided that each service should transfer funds – about $31,500 – to cover one-third of the proposed start-up budget to the Weather Bureau, and that Cressman should be authorized to expend those funds.[729] The JMC approved and directed both actions to take place.[730]

On 17 February 1954, the JMC formally designated the Weather Bureau responsible for administering the JNWPU. Reichelderfer wrote to Wexler, "This is a major responsibility. Be sure that we set up arrangements to do the job well." Since Wexler had been intimately involved with the early planning of the Unit, this statement seems superfluous. Reichelderfer was clearly concerned that the Weather Bureau might receive the brunt of the criticism if the Unit did not prove to be successful.[731]

By mid-summer, space had been allocated in Federal Office Building No. 4 in Suitland, Maryland – not next to the WBAN Analysis Center (also in Suitland) as had been previously planned. However, this new location was adjacent to that which would be occupied by the National Weather Analysis Center – the WBAN's successor. In the meantime, the JNWPU would occupy space made available by the Weather Bureau. In financial matters, of the $94,500 start-up funds, approximately $82,000 would be used to modify spaces, power and electrical installations, and engineering services. The remaining money would be used for miscellaneous equipment and furniture. For fiscal year 1955, the estimated expenditure was

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[729] Draft JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 8th Meeting held on 28 January 1954 (JNWP-7-54) (Wexler papers, B32, NWP).
[730] JCS/JMC to the Ad Hoc Group (JMC-26-54), 23 February 1954 (Wexler papers, B6, F1954).
[731] Reichelderfer to Wexler, 24 February 1954 (Wexler papers, B6, F1954).

$311,000, or $103,700 per service. Since the Navy was providing fewer people, its cash contribution was almost twice as high as that from the other two services. By providing more than one-third of the personnel, the Air Force actually reduced its expected cash contribution.[732]

Unfortunately, an unexpected complication appeared. The Air Force member, Major Lewis, reported that another JMC *ad hoc* group had recommended that the WBAN Analysis Center adopt a 1:20,000,000 map for future use. However, such a scale was not useful for NWP work. In fact, IAS, the Weather Bureau and the Air Force had shown that if the map scale were smaller than 1:12,500,000 it could not be used in numerical weather prediction. To make matters worse, the JNWPU budget proposal had counted on the availability of the 1:12,500,000 scale maps. If the map scale changed, the Unit would need to make other arrangements to obtain the correctly scaled maps.[733] The issue came up again two months later. JMC members told the Ad Hoc Group that the WBAN would be able to provide the maps to the required scale *if* the WBAN Center had "suitable transforming or enlargement facilities."[734] However, the JMC members gave no

[732] Final Report of the Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit, 30 June 1954 (Enclosure (1) to the Minutes of the 15th meeting held 1 July 1954) (Wexler papers, B32, NWP). The estimated personnel expenses obtained by using mid-grade Civil Service salaries came to $191,525. The IBM 701 (and peripherals) lease would be approximately $97,000 for the four months from March through June 1955. Additional cost account items included travel, phones, utilities, printing, and office supplies for a total of approximately $22,000.
[733] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 5th Meeting held on 20 November 1953 (JNWP-19-53) (Wexler papers, B6, F1953). The *ad hoc* group in question was the JMC Ad Hoc Group for Development of Plans for Consolidation of Analysis Functions in the Washington Area.
[734] JCS/JMC (JMC-4-54) to Ad Hoc Group for Establishment of a Joint Numerical Weather Prediction Unit, 11 January 1954 (Wexler papers, B6, F1954).

indication that such facilities were actually available. Even if they were, the re-scaling of maps would delay data flowing to the JNWPU.

While computer, space, funding, and personnel issues were all being addressed by the service representatives in Washington, D.C., the Princeton group continued to work on the models. The Meteorology Project members had two basic missions: to clean up the models that would be run operationally, and to gradually extend the forecast lengths for those and other models for future use. The personnel situation had improved dramatically with the help of an infusion of foreign meteorological blood. Joining Charney were Scandinavians Berggren, Bolin and Fjörtoft, and Britons Eady and Gilchrist. Visiting "consultants" – who visited IAS for a few days each – were all from outside the United States. Four representatives from the weather services were in Princeton for training before transferring to the JNWPU. Phillips was in Stockholm with Rossby. And so the international nature of the Meteorology Project continued.[735]

The Meteorology Group had gradually shifted its attention to longer range forecasts since the quasi-geostrophic models and their ability to predict short-range events had become rather routine. However, there was still some cleaning up to do before the model went operational, so team members had not abandoned short-term forecast work entirely.

Team members, busy working on case studies and investigations of additional atmospheric influences on the general circulation, continued to make

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[735] The Institute for Advanced Study, Meteorology Project, Progress Report July 1, 1953 to March 31, 1954, Contract No. N-6-ori-139 (1), NR 082-008 (Charney papers, B9, F305).

important advances during the period of preparation for operational numerical weather prediction. They made forecasts with the three-level model for two more cyclogenetical periods in the eastern United States. In both cases, the model successfully predicted cyclogenesis. Those successes indicated that large-scale middle latitude storms were predictable, quasi-geostrophic and quasi-isentropic – good news for operational applications.

Project members also investigated the effects of horizontal-vertical vorticity conversion, vertical advection of vorticity, influence of mountain ranges, and vertical propagation of energy – none of which had been included in simpler versions of the two- and three-level models. To handle these effects, they integrated the general quasi-geostrophic equations using potential temperature as a vertical coordinate. During the check-out phase of the coding, team members determined that they needed to make fundamental changes in the treatment of the lower boundary potential vorticities before making computations. Since they had had limited experience in the integration of multi-level model equations in the vertical coordinate, team members decided to make additional investigations using pressure as the vertical coordinate before modifying the equations further. To that end, they programmed two five-level models using pressure in the vertical (one model was run on the IAS computer and the other on an IBM computer in New York City). The IAS model had to integrate a highly non-linear partial differential equation, and the potential vorticities carried the history of motion. In the IBM model, the contour heights of the isobaric surfaces carried the history of the motion. Neither model included topography, and the IAS model did not include the vertical advection of potential vorticity. Both used 1000, 800, 600, 400, and 200 mb pressure levels. By March 1954, the Princeton team members had run both models out to 5 hours and were planning to extend them to 24 hours. Cyclogenesis began at the ground and worked its way up into the atmosphere in both models. In preparation for longer period predictions, the group members wanted to explore how far into the future they could successfully extend the predictive period if they took into account energy sources and sinks, and the non-homogeneity of the earth's surface with respect to heat, water vapor, and momentum transfer. The models had to describe the essential processes governing the life-cycle of a single large-scale atmospheric system and account for the "general circulation" of the atmosphere. From previous work, it appeared that it was necessary to have at least three levels in order to predict cyclogenesis, i.e., the development of the large-scale system. However, team members worried that they had not conclusively ruled out the efficacy of the two-level model. Team members had gotten good results from some two-level models, and the results from the three-level models appeared to depend on the chosen levels. In the latter case, the model using 900, 700 and 400 mb data gave a better result than the model that used 850, 500 and 200 mb data. Therefore, they planned additional investigations.

Truncation and round-off errors had become a problem when the forecast was extended out for long periods. To determine the source of the error, the team used a barotropic model with idealized initial data and carried the calculations out for up to 14 days. An analysis of the computations showed that round-off error was not a problem; truncation error was. Team members then considered a variety of smoothing techniques to reduce this error.

Because the geostrophic model was unable to adequately explain the birth of fronts and jets, the team members continued their investigations into the properties of the general equations of motion. Aided by von Neumann's fix of a boundary condition problem, they started by integrating the equations for a one-layer atmosphere with a free surface. Programmed and coded, this method was awaiting check-out. Team members also devised a method of minimizing the effects of gravitational wave energy. On work related to existing models, Charney created a similarity theory which reduced the speed of long-gravity waves in both baroclinic and barotropic atmospheres, thus reducing computation time.

Work on an objective analysis method continued by taking wind and height values on an isobaric surface and interpolating height values at the grid points using a least squares method. The code allowed team members to instruct the computer to draw contours. By interpolating forecast data into data sparse regions, i.e., large unpopulated areas of the United States or oceanic areas, the team could ensure continuity between time periods.

Charney, Eady, and Fjörtoft also pursued a variety of theoretical investigations. Charney completed a hydrodynamical-thermodynamical study of the factors which determined the broad features of the spectral distribution of the atmosphere's energy at the large-scale end which explained the quasi-geostrophic character of atmospheric motions. He also worked on a study of troposphere-stratosphere energy propagation by calculating the "optics" of refraction and reflection of long atmosphere waves. In his work on the geostrophic approximation, Charney found that it was better to consider the horizontal wind as approximately non-divergent in the potential vorticity equation. Eady investigated criteria for the stability of a baroclinic zonal current. With the exception of very long wave lengths, Eady's criteria agreed with Charney's as long as the variation of the Coriolis parameter and an infinitely vertical atmosphere were included. Very long wave lengths, however, became unstable. Eady also studied the stability of barotropic shearing flow and baroclinic flows with a combined horizontal and vertical shear. Fjörtoft showed that repeated space smoothings could be successfully applied to the solutions of general elliptic equations. He also was continuing studies on improving the geostrophic assumption.[736]

As the time approached for the JNWPU to come on-line, the Princeton group was rapidly debugging the operational models. However, its work was not complete. There was still much to be discovered about atmospheric circulation, and there was much work left to be done before longer range forecasts would be viable. As NWP became operational, the Meteorology Project would just shift its focus back to more theoretical issues.

The move to operational NWP was not restricted to the United States. During the year preceding the opening of the JNWPU, other centers of activity

[736] The Institute for Advanced Study, Meteorology Project, Progress Report July 1, 1953 to March 31, 1954, Contract No. N-6-ori-139 (1), NR 082-008 (Charney papers, B9, F305).

were gaining ground in Europe and were pushing towards their own operational forecasting units. By late 1953, the Stockholm group was very busy on BESK. According to Phillips, it had been running quite well. With the exception of output (i.e., the actual printed result), BESK was faster than the IAS machine. Rossby's team members had made three 24-hour barotropic forecasts using a 20x20 grid – the maximum size possible due to the 512 word memory. They were awaiting the installation of the magnetic drum which would allow them to increase the grid size to 31x55, 31x32 and 31x22 for the one-, two- and three-layer models respectively. The Stockholm group planned to concentrate on longer period forecasts in the future.[737]

In late October/early November 1953, Thompson made another European tour to assess the progress of numerical weather prediction in Sweden, West Germany, and the United Kingdom. After returning to the United States, he reported that the Europeans were about six months behind in basic theory and one to two years behind in operational application due to personnel shortages, lack of training, and non-availability of specially dedicated computers for numerical weather prediction. Deutscher Wetterdienst was working on putting NWP into operation, but it did not appear that they could do so before early 1956. The British Meteorological Office intended to fold numerical techniques into their forecasting practice, but without a computer would be limited in what it could do. Rossby's group "professed to have no definite plans for operational applications, but have the

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[737] Phillips to Charney, 19 December 1953 (Charney papers, B14, F449).

capabilities for putting numerical methods into practice by early 1955." Since the Swedish team in fact began producing operational forecasts in 1954, it appears that Thompson was somewhat led astray by what he heard in Sweden. Thompson was authorized to offer Rossby the possibility of an Air Force contract for research. Rossby was glad to take it but reminded Thompson that, because Sweden was a neutral country, the funds would need to be "decontaminated" via a civilian institution, e.g., Woods Hole Oceanographic Institution.[738]

In early spring 1954, Smagorinsky went to Europe and then reported that the British and the Swedes anticipated making daily operational predictions within six months.[739] It happened sooner than that. In mid-June, Rossby informed Charney that the Stockholm team had made 23 barotropic forecasts for the eastern Atlantic and northern Europe, including two operational ones, on BESK. Having gotten good results, they were preparing to make operational 48-hour forecasts.[740]

In contrast, the JNWPU's computer would not be available for six more months.

NWP, THE PRESS, AND PUBLIC IDEAS OF WEATHER PREDICTION

From the earliest days of von Neumann's Computer Project at IAS, there had been outlandish descriptions of what the computer could do for meteorology and weather

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[738] Thompson to Chief, Atmospheric Analysis Laboratory, ca. November 1953 (Thompson papers, Correspondence 1953-1954).
[739] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 11th Meeting held on 11 March 1954 (JNWP-16-54) (Wexler papers, B32, NWP).
[740] Rossby to Charney, 16 June 1954 (Charney papers, B14, F460).

forecasting. As discussed in Chapter 5, early press coverage in *The New York Times* included the comments about numerical weather prediction being the first step toward weather control. Comments linking weather prediction and control were largely missing by the early to mid-fifties. By then, weather control had become a subject in its own right due to the cloud seeding efforts of Nobel Laureate Irving Langmuir and his assistant Vincent Schaefer of General Electric's Schenectady Laboratory. However, press coverage still tended to exaggerate the capabilities of the new computer forecasts. Those press reports made von Neumann, Charney, and others very uneasy. They thought the entire project was being oversold. Indeed, the Project members were having enough problems persuading some members of the meteorological community to take their work seriously without reading that computer weather prediction for long periods of time could take just a few minutes. One way to counteract fantastic press reports would be to meet the press. And they did. The resulting spin depended on who was doing the talking or writing.

In May 1954, Joseph Smagorinsky, Project Leader for the Weather Bureau's efforts to be ready for NWP, addressed the WGY "Science Forum" about numerical weather prediction. (WGY was the General Electric Company's radio station in Schenectady, New York.) After reviewing why everyone wanted to know the weather in advance, Smagorinsky explained that the physical understanding of the atmosphere had increased over the previous 50 years. He described how physical laws could be described by differential equations – equations which related small spatial variations with small intervals of time. Smagorinsky explained that those equations were basically unsolvable until the advent of the electronic computer. Therefore, during the previous 50 years meteorologists had analyzed available data by hand. Computers would make it possible to obtain more accurate forecasts by doing the actual integrations of the equations. He went on, "The vision of Professor John von Neumann of the Institute for Advanced Study at Princeton, New Jersey made it possible to apply high speed computer methods to the weather forecast problem – which has come to be known as numerical weather prediction." Smagorinsky also credited Rossby with being instrumental to the study of earlier meteorological work which allowed current researchers to avoid the "pitfalls" of the past. And he credited Charney with simplifying the meteorological equations in a "rational" manner that bypassed "years of experimentation and research."[741]

Smagorinsky further explained that to obtain forecasts by numerical methods meteorologists needed wind, pressure, temperature, and humidity data over large geographical areas, up to an altitude of 70,000 feet above sea level, and not more than 200 miles apart. A 24-hour forecast for New York City would require data in a 600 mile radius around the city. For a longer forecast period or a larger forecast area, meteorologists needed a larger radius. Thus to forecast for the United States, data were needed from the Pacific to the Atlantic and from the Canadian Arctic down to Mexico. Once the data were available, the forecast would take just a matter of minutes.

[741] "Numerical Weather Prediction," address on WGY Science Forum by Joseph Smagorinsky, U.S. Weather Bureau, Washington, D.C., 20 May 1953 (Wexler papers, B32, NWP).

The general public's concern about automation eliminating jobs must have been on Smagorinsky's mind while preparing his presentation. This radio talk was primarily about the methodology of numerical weather prediction, and yet Smagorinsky went on to explain that numerical weather prediction would not result in the mass unemployment of meteorologists. Rather, meteorologists would just have more time to make sense of the numerical products and thereby provide better, smaller-scale forecasts than were presently available. Smagorinsky also admitted that there were many factors which impacted the weather that were not yet clear to meteorologists, e.g., the sun's radiation, atmospheric turbulence and conditions for precipitation. Until those issues were addressed, longer term forecasts of up to a year in advance could not be attempted. However, Smagorinsky assured his radio listeners that the Weather Bureau was addressing all these concerns so that they might continue to provide the very best weather information to the general public.[742]

Smagorinsky was definitely looking for "positive press" that would enhance the reputation of the Weather Bureau. According to Smagorinsky, his talk received a good review from the *Schenectady Gazette.* He pointed out that it "once again indicates that publicity originating from the Weather Bureau can help us to have *sympathetic relations with the press and the public*." (Emphasis mine.) This was in contrast to the article "Tomorrow's Weather" which appeared in the May 1953 edition of *Fortune* and had become a public relations disaster. The article, which

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[742] Ibid.

considered both weather modification and extended weather prediction, indicated that the Weather Bureau was so conservative that it refused to use ground-based seeding methods perfected by commercial seeders in its own trials. Although not overly critical of the Weather Bureau, it did put those working in the private sector in a better light. Smagorinsky continued, "It would seem that the Weather Bureau should seize upon every opportunity to educate the public (in a dignified manner, of course) on our efforts toward carrying out our primary mission." In the margin was "Do we not?!!" - probably written by Harry Wexler.[743] Apparently, the Weather Bureau was feeling beleaguered by adverse press coverage.

As the JNWPU project continued to gain ground, Ann Ewing, staff writer for the Science Service, attended a meeting of the Ad Hoc Group to get help with an article she had written about NWP. Ewing's article described how a "giant electronic 'brain'" (computers in this period were almost always "electronic brains") would be making daily wind predictions which would then be used for local weather forecasts on an *experimental* basis within a year's time. (Emphasis mine.) Billing the undertaking as an experimental program run by the three services, the article went on to say that this "revolutionary method" was first developed at the IAS. In fact the field was so new, Ewing wrote, that there were few experts on the subject in the entire world. After describing what data would be

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[743] J. Smagorinsky to Reichelderfer via H. Wexler, 29 May 1953 (Wexler papers, B32, NWP). "Tomorrow's Weather," *Fortune* 47 (May 1953): 144-149+. The *Fortune* article pitted the Weather Bureau's extended forecasting section against the private forecasting firm of "war forecaster" Irving P. Krick, who was viewed with disfavor by Reichelderfer. It also addressed the weather modification work of both Krick and Langmuir.

input and what would be produced, she continued that the "brains" would "eventually eliminate most of the forecaster's personal *opinions* from his predictions." (Emphasis mine.) Noting that the formulas had not been entirely worked out, Ewing wrote that eventually meteorologists hoped to include a variety of energy sources, e.g., radiation from the sun and heat from condensation, and energy sinks such as those due to evaporation, in their models. These improvements would allow forecasts for five, thirty or even more days. Long range predictions were not expected soon.[744] It is unclear whether Ewing considered five days to be "long range" or not. Certainly long range predictions were not going to appear any time soon. The idea that forecaster's "opinions" would ultimately be eliminated from weather prediction was another common theme. Statements like these led the public to think that the computers would spit out the forecasts they heard on the radio or read in the newspaper.

A month later, ONR, with the approval of the Department of Defense's Office of Public Information, issued its own press release on NWP. Entitled "Electronic Weather Forecasting," the Navy was quick to take credit for numerical weather prediction saying that the newly forming Joint Unit was an outgrowth of research "initiated in 1946 when the Office of Naval Research contracted with the Institute of Advanced Study in Princeton to study numerical prediction technique…" (So far, so good.) It went on to say that the project itself was set up by ONR and that von Neumann and Charney were "given the problem of

[744] Article by Ann Ewing, Science Service Staff Writer on NWP, October 1953 (Wexler papers, B32, NWP). Published as "Weather by Giant 'Brain,'" *Science News Letter* 64 (1953): 309.

developing the technique." This statement included a little license, since Charney did not come on the scene until after the Project had already been in existence for almost two years. After briefly describing the roles of von Neumann, Goldstine and Bigelow in the design and development of the Princeton computer, the release continued by saying, "In January 1949 the Office of Naval Research *invited* the Geophysical Research Division of the Air Force to participate in the electronic forecasting technique." (Emphasis mine.) As a result, the Project moved forward even more rapidly than before. Finally, there was a lengthy quote from "Dr." (vice Commander) Daniel F. Rex of the Office of Naval Aerology which compared the revolution in numerical techniques to the one spawned by the "Norwegian Wave Cyclone Theory." In his opinion, numerical methods would enable local forecasters to spend more time on the details of their local area weather since they would not need to draw their own prognostic charts.[745] Of course, Rex was a Ph.D. meteorologist who received his degree in Stockholm while studying with Rossby. By referring to him as "Dr." instead of as an active duty officer, it appears that the Navy was trying to attach more credibility to his statement. There was another problem: no available archival evidence indicates that the Navy asked the Air Force to join in sponsoring this project.

The press releases and articles did not escape the notice of the Ad Hoc Group (of which, of course, Rex was a member). The members decided that whenever a significant development occurred, or when new information was

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[745] Office of Naval Research Press Release "Electronic Weather Forecasting," 23 November 1953 (von Neumann Papers, B15, F2).

available, a joint statement would be prepared and sent to von Neumann, Reichelderfer, Charney, Thompson (GRD), and Palmer (ONR) for their own use or for further distribution within their organizations.[746]

Later in the month when Wexler visited von Neumann and Charney in Princeton, they discussed some of the recent press accounts. Von Neumann was upset that some of the articles had oversold NWP and tended to place the greatest emphasis on the machinery, as opposed to the intellectual achievement which allowed the modeling to take place. Wexler reported to Reichelderfer, "We shall have to be even more careful in the future in cautioning reporters to avoid some of the objectionable features."[747]

Wexler could play the press game too. In February 1954, he presented a proposed press release about the computer simulation of the "busted" east coast snow storm. In the article, the data were run through one of Princeton's models, which successfully identified the storm, and steered it in the correct direction. The implication was that given numerical methods, better forecasts would result. In the discussion that followed, the Group expressed concern that the press in general was taking the results of the competitive tests between the IBM and Remington-Rand computers as a test of actual NWP techniques. Therefore, the success of those runs would be equal to the success of operational NWP in the reader's mind. The Group wanted to make sure the press did not leave the public with the impression that

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[746] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 6th Meeting held on 7 December 1953 (JNWP-27-53) (Wexler papers, B32, NWP).
[747] Wexler to Reichelderfer, 9 December 1953 (Wexler papers, B32, NWP).

NWP was a done deal.[748] But there is no doubt that the word was out. Even the Boy Scouts of America's Editorial Service sent a letter to von Neumann asking about the work of the Princeton teams.[749]

Numerical weather prediction needed the help of the media – radio, newspapers, magazines – to tell their story. They desperately wanted the public to get these messages: NWP was worth the investment of time and money, it would lead to more accurate forecasts, it was the future of modern meteorology. Each organization involved wanted to be shown, of course, in the best light. But while the participants in the project wanted both the public and the scientific community to be sold on NWP, they did not want them to be *oversold.* And while articles about "giant brains" forecasting the weather certainly attracted attention, there was always a nagging concern, particularly at the Weather Bureau, that if the result did not live up to the hype, what little credibility the meteorological community had would be significantly reduced.

THE JOINT UNIT COMES TO LIFE

After over a year of planning and negotiations, the Joint Numerical Weather Prediction Unit became a non-operational reality on 1 July 1954. It was non-operational because it had no computer and would not have one for at least six more months. However, its personnel still had plenty of work to accomplish.

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[748] JCS/JMC Ad Hoc Group for Establishment of a JNWPU: Minutes of the 9th Meeting held on 5 February 1954 (JNWP-12-54) (Wexler papers, B32, NWP).
[749] Von Neumann to Mr. Carol Spica, 5 March 1954 (Charney papers, B16, F517).

Cressman laid down four primary tasks for his Unit: evaluating which model would be best for initial operational use, preparing a program library, training personnel to program the computer, and training the analysts.

The JNWPU worked closely with both GRD (the Numerical Prediction Project under Thompson) and IAS to evaluate models. Both IAS and GRD ran their three most promising models from the same initial data and compared the output. JNWPU members planned to obtain time on IBM's New York-based 701 to run some of the programs. They anticipated running the three models based on thirty different starting maps by 1 February 1955. After studying approximately sixty baroclinic forecasts made by the GRD, the JNWPU's Development Section discovered that half of the systematic errors could be attributed to neglecting terrain-induced vertical motions. It was also analyzing the effects of ignoring some of the terms in the vorticity equation. Another study dealt with erroneous boundary assumptions and how they affected model output. However, sixteen of the sixty 500 mb height forecasts were found to be significantly more accurate than the subjective maps obtained from the USAF Weather Central for the same verifying times. Based on these findings, the Development Section members had revised models and they were being tested by both hand and machine computation at IAS. The Computing Section was working on a number of different programs including barotropic, three-parameter baroclinic with terrain, objective analysis, three-parameter baroclinic for comparison testing, two-parameter baroclinic programs, and a program which would give a baroclinic forecast with boundary conditions given by a barotropic forecast covering a larger area. Unit members who reported in July attended an IBM-provided programming course. IBM would provide a similar course in the fall for those arriving later.[750]

With the JNWPU officially open for business, the work of the Ad Hoc Group was done. However, an oversight committee still needed to be formed to provide assistance and work out problems between the three contributing weather services. Therefore, on 4 November 1954, the JMC formally dissolved the Ad Hoc Group and established yet another *ad hoc* committee: The Ad Hoc Committee on Numerical Weather Prediction (JMC/NWP).[751] Under the "Terms of Reference," i.e., the description of its tasks and responsibilities, each weather service was authorized to appoint one member to the Committee although others would be allowed to attend meetings in an advisory capacity. The JMC/NWP would stay cognizant of the workings of the JNWPU, assist and advise its Director on requirements, external technical matters, fiscal issues, service personnel issues, and off-time usage of equipment. The JMC/NWP would keep the JMC informed of NWP matters and bring any major policy issues to it for resolution. However, it was not within the purview of the Committee to solve any highly technical problems. For those, the Cressman could seek the advice of scientific consultants after receiving the concurrence of the Committee members. This quasi-supervisory role of the Ad Hoc Committee did not give it license to be a micro-manager. Since

[750] Activities of the Joint Numerical Weather Prediction Unit, 1 July to 1 October 1954 (Wexler papers, B32, NWP).
[751] The original members JMC/NWP were Captain W. E Oberholtzer, Jr., USN, Lt. Colonel H. H. Bedke, USAF, and Dr. H. Wexler, U.S. Weather Bureau.

the Unit was a new entity in a new field, the Director was to have wide latitude in determining what should be done.[752]

One of the first issues, not surprisingly, dealt with personnel. The Navy representative (Captain Oberholtzer) made clear that all Navy personnel assigned to the Unit must be trained in each of its primary functions, i.e., modeling, programming, and analysis. Cressman indicated that personnel would be cross-trained to the extent that there was a fit between their background and their desires, but that some personnel did not want to perform some of the functions of the Unit. This likely sent Oberholtzer over the edge, as it pointed to a tremendous gulf between the culture of civilian meteorologists and the military services: in the Navy, one's individual "desires" had nothing to do with one's assignment to a task. In response to a question by Wexler, Cressman stated that all of the analysts were taking the [machine] coding course and that everyone would be involved in discussions of all aspects of the program. Service representatives would share information about the qualifications of incoming personnel directly with Cressman. Cressman would handle unsuccessful assignments with the appropriate service representative.

Cressman had already made the necessary contacts to secure technical consultants before the terms of reference were issued. Consultants from outside government included Charney, Gilchrist, and Bigelow from IAS, Platzman from

[752] Terms of Reference, Joint Meteorological Committee, Ad Hoc Committee on Numerical Weather Prediction (JMC/NWP), Enclosure to Memorandum for the Members, Joint Meteorological Committee (JMC-130-54) of 10 November 1954 (Weather Bureau papers, RG 27, JCS/JMC).

the University of Chicago, and Rossby from the University of Stockholm, Sweden. Thus, after over nine years of helping to coordinate the development of numerical weather prediction, Rossby finally had an official role. Consultants from within the government service included computer specialists Lawrence Gates (GRD), von Neumann (AEC), and Franz Alt (National Bureau of Standards).

Any agency desiring to place new requirements on the JNWPU had to coordinate them through JMC/NWP. Without this provision, there would have been chaos almost immediately. The Air Force, Navy, and Weather Bureau each had different mission requirements. Each would be seeking different products from the Joint Unit. Without a clearing house for their specialized mission requirements, the Unit would be overwhelmed with requests. As far as requirements being levied *by* the Unit, Cressman reported that the WBAN Analysis Center would be plotting and analyzing two 400 mb charts per day starting in January 1955. Because the analysis section of the JNWPU had been kept small on purpose, he was counting on WBAN to fill its needs. Due to the coordination required between JNWPU and WBAN, the Ad Hoc Committee determined that the Joint Unit would need to be able to deal directly with the Coordinating Committee of the National Weather Analysis Center (WNAC – the replacement for the WBAN Analysis Center) if and when such a committee was established under the JMC.

Another important issue was the policy for "outside use" of the IBM 701. The machine had not yet arrived, but outside agencies were already seeking computer time. Under the terms of the proposed policy, the machine could be used by either governmental meteorological services or cooperating NWP research groups subject to the Director's approval of the problem to be run on the computer. Any use of the computer had to be at the convenience of the Unit, and the Unit would provide no manpower assistance with the exception of the machine operator. Any non-governmental groups using the machine would be expected to pay for all machine time unless there was a reciprocal arrangement on another machine. When Wexler questioned why the JNWPU needed to be reimbursed, when the machine time was already paid for, Cressman commented that they wanted to discourage non-meteorological organizations from using the machine.[753] Discussion also revolved around who would be allowed to submit programs to run on the machine. The Air Force representative thought the first priority should go to whatever group had the most to contribute to NWP regardless of whether they were a governmental agency or an NWP research group. As far as reciprocal computer time, Cressman noted that both the GRD and IAS had run programs for the JNWPU, and therefore the Joint Unit should run programs for them if asked. The other issue was machine time outside of the time already contracted for with IBM. Once those hours were exceeded, then the cost increased. Therefore it was decided that as much as possible, any requests for time would have to fit into the time for which IBM had already been paid.[754]

[753] JMC/NWP Minutes of 1st Meeting held 29 November 1954 (Weather Bureau papers, RG 27, JCS/JMC).
[754] JMC/NWP Minutes of 2nd Meeting of 13 December 1954 (Weather Bureau papers, RG 27, JCS/JMC).

Since the beginning of fiscal year 1956 was only six months away (it would start 1 July 1955), the Committee considered its budget needs. Cressman anticipated no further staff increases after fiscal year 1956. He thought he might even be able to reduce staffing by one plotter. By the five year point, it might be possible to reduce the programming staff. Apparently Cressman thought that once they had the models programmed they were home free and would do very little programming work. His casual comment shows a consequence of a complete lack of experience in the field – no fault of Cressman, everyone was new to the field – and yet it defies common sense. The purpose of this Unit was to take upgraded models and put them to operational use. The programming would always need to be done in-house. Therefore, the number of programmers would not decrease with time unless no improvements were made to the models. The whole idea behind making the transition from a research to an operational organization was to insure that model improvements took place faster. Decreasing the numbers of programmers would probably cause modeling to stagnate instead. The anticipated contribution of each service for fiscal year 1956 was approximately $205,000. The Air Force representative advised that his service would need an estimated fiscal year 1957 budget not later than January 1955 (fiscal year 1957 would have started on 1 July 1956).[755] It is somewhat surprising that the military members were not pushing for budget estimates for years even further out. (Generally budgets were

[755] JMC/NWP Minutes of 1st Meeting held 29 November 1954 (Wexler papers, B32, NWP).

set up in five year cycles and then readjusted each year as the operational climate changed.)

JMC members addressed the coordination problem with the new National Weather Analysis Center (NWAC) in late 1954. Whereas the WBAN Analysis Center had fallen under the supervision of the ACC/MET, the new analysis center would be without JMC supervision if a new *ad hoc* committee were not established to fill that role. The JMC expected that analysis center would be operational in January 1955. If no action were taken, JMC would no longer have a supervisory role. On the other hand, the Joint Unit fell under the cognizance of the JMC via the *ad hoc* group. That meant the Unit could not directly approach the analysis center for assistance – it had to follow a cumbersome, circuitous chain through advisory committees *up* to JMC and then *down* to the analysis center. This was clearly a problem. Since JMC had discussed merging the Joint Unit and the analysis center, members suggested that the JNWPU be placed under ACC/MET and the *ad hoc* group dissolved.[756] This issue was discussed again a few weeks later. The requirement for the analysis center to produce 400 mb charts for the Joint Unit was sent by the JMC to ACC/MET. Then JMC members addressed the issue of weather service coordination. The question: who should have supervisory authority over the Joint Unit and the analysis center? Even though the Weather Bureau argued that there was no reason for the JNWPU to be under JMC supervision, all agreed that

[756] JMC 60/15.5; Joint Meteorological Committee, Coordination Between the Joint Numerical Weather Prediction Unit and the National Weather Analysis Center, 16 December 1954 (Weather Bureau papers, RG 27, JCS/JMC).

the best scenario was for both units to be supervised by ACC/MET once they were operational. Since neither was operational, it was not yet an issue. In response to the Weather Bureau's comment about JMC supervision, the Air Force pointed out that it received part of its budgetary support for the Joint Unit by virtue of its association with the JMC. The Weather Bureau argued that unless it was absolutely necessary, no committees should supervise either unit because as Reichelderfer put it, "committee operation of a unit is never good." Undoubtedly part of Reichelderfer's motivation was due to the fact that both of these units resided in Weather Bureau spaces and were under the Weather Bureau's administrative control despite being jointly funded and staffed. The military units were probably concerned with losing any kind of control within a civilian organization without the JMC related supervision.

The JMC discussed the use of computer time by outside agencies and concurred in the policy as proposed by the *ad hoc* group. The JMC also brought up the fiscal year 1956 budget, but both military representatives asked for a deferral until they could study it. All agreed that they strongly supported the Joint Unit and did not anticipate a problem with their share of the budget.[757] However, by the middle of January, funding and manpower problems were beginning to appear. The Air Force could not meet the manpower requirements, but could substitute funds for manpower even though it was not sure it would have its full share to offer. Although Weather Bureau leaders wholeheartedly supported the NWP effort, high

[757] Minutes of the Joint Meteorological Committee 340th Meeting held 21 December 1954 (Weather Bureau Archives, RG 27, JCS/JMC).

authority had eliminated the money the Bureau had set aside for the Unit. However, the Bureau continued to seek funding for its full share. The Navy reported that only part of its share had been included in its budget. That was partly due to the sharp increase in the Unit's budget between fiscal years 1955 and 1956, as it moved from the pre-operational to the operational stage. The Navy needed to wait until the entire military budget had been adopted before knowing if there would be additional funds. The JMC then approved the proposed fiscal year 1956 budget with the stipulation that it would await the outcome of the total budgets of the Departments of Defense and Commerce.[758]

Cressman briefed the JMC on the status of the Joint Unit on 3 May 1955. The IBM 701 had been checked out and accepted from IBM two months earlier. In mid-April, Unit members had run the first experimental forecasts. The results had been better than anticipated. Since the computer had arrived later than expected, Unit personnel would not complete the shakedown phase (Phase I) until 6 May 1955. At that time, Unit members anticipated the beginning of Phase II operations. During Phase II, they would extend the objective analysis for North America approximately 1500 miles into the North Pacific and North Atlantic. This analysis would be for internal JNWPU use only. Unit members would also produce a baroclinic three-level prognostic chart for the United States. Problems with the introduction of terrain effects had led to some programming difficulties, but Unit members expected to overcome those within a couple of weeks. They would also

[758] Minutes of the Joint Meteorological Committee 341st Meeting held 18 January 1955 (Weather Bureau papers, RG 27, JCS/JMC).

be producing vertical motion products between the 900 mb (1000 meters/3300 feet) and 700 mb (2700 meters/9000 feet) layers and the 700 mb and 500 mb (5500 meters/18,000 feet) layers at 12-hour intervals. The baroclinic and vertical velocity products would be available for users starting on 6 May. Work continued on a barotropic 500 mb prognostic chart covering all of the Northern Hemisphere.

The relative accuracy of the computer generated charts generated a happy surprise for the Joint Unit. To "verify" the weather maps, Unit members checked the 24-hour computer produced prognoses at the three levels (400, 700, and 900 mb) by comparing the distance between the forecast and observed low height center positions. The 400 and 700 mb levels showed a difference of two degrees each of latitude and longitude, while the 900 mb level showed a two degree latitude and five degree longitude difference. (Each degree is approximately 65 miles.) These results were better, Cressman argued, than the best subjective efforts and should be considered to be the worst that could come out of the Unit. After all, this was an initial, experimental effort.

The handling of the incoming data for the objective analysis continued to be a major problem. The data came in via teletype and fifteen man-hours later Unit members had finished manually punching the data onto cards and feeding them into the machine. The Unit had obtained a machine that would read the teletype paper tape and convert it automatically to punched cards. This new procedure would reduce the number of sub-professionals from five to one. The computer could then be programmed to sort through the observations and reject reports, which were either not needed for the objective analysis or which were garbled, before running the program. Cressman noted that automated data handling was just beginning and it would be a number of months before this would become a routine operation.

The teletype system, sufficient for subjective methods, was also a problem. It took nine hours just to collect all the data needed for a single chart. If the observation and transmission schedules were changed, it would only take thirty minutes. Considering the amount of time that it took to tear teletype tape and punch cards plus the time to run the programs, it was apparent that the nine hours being absorbed by data collection would need to be dramatically reduced if the numerical prediction runs were to work.

As an example of the military influence on the Unit's work, prognoses were being created for the 500 mb level. This was an obvious level to try out first since it represented the "steering level" for surface systems and thus was highly valuable for forecasters. It was also used because that was the level flown by Air Force Weather Reconnaissance Aircraft. However, the Air Force was thinking of moving those flights to a higher level (400 mb) which could potentially impact the desirability of creating the 500 mb charts. The aircraft reports were used as input and verification tools. Changing the level could influence the operation of the Unit.

Cressman also wanted more data from over the Pacific. As a trial, the Weather Bureau had put an upper air team aboard USNS *General Hugh J. Gaffey* (a Military Sealift transport ship) while underway in the Pacific, and had obtained excellent results. The regular availability of such soundings from ocean areas would help to anchor the forecast. Another possibility: use dropsondes launched from aircraft transiting the area.[759] However, both of these were very expensive options, and if the Weather Bureau leaders were worried about having enough money to keep the Unit operational, they probably did not have enough money to send upper air teams out to ride ships-of-opportunity, i.e., ships transiting the area that were willing and able to take on men and material, or to send dropsondes out with military planes flying across the ocean.

AT LONG LAST – DEDICATION

The shakedown period of Phase I was over for the JNWPU. Unit members had checked out the computer, the personnel were on board, the model was running, and communications circuits were in place. The time had come for numerical weather prediction to leap beyond the experimental and into operation.

The dedication ceremony took place on 6 May 1955 – almost nine years to the day of the time IAS had sent its proposal for a Meteorology Project to ONR. In the Weather Bureau's remarks prepared for that day, tribute was paid to the pioneers of hardware development, upper air investigations, dynamic meteorology, and, of course, to L. F. Richardson who in 1922 published the disastrous results of his attempt at numerical weather prediction. And credit was given to von Neumann and Charney for their leadership in the Computer and Meteorology projects and for bringing to fruition two of the three legs on which numerical weather prediction

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[759] Minutes of the 344th meeting of the JMC held 3 May 1955 (Weather Bureau Records, RG 27, JCS/JMC).

stood: the electronic computer and meteorological theory of large-scale atmospheric motions. The third leg – a sufficient density of upper air observations – was in place as a result of World War II. Credit was also given to Air Force, Navy and Weather Bureau personnel who had been critical to the development and planning of this "unprecedented venture." Absent was any mention of the tag-team of European meteorologists – primarily Scandinavians and Britons – who had bailed the Meteorology Project out of numerous manpower holes. These meteorologists, who had stayed in the United States for several months to a year at a time, had been crucial to creating the dynamic-synoptic meteorology interface required for the successful creation of numerical weather prediction models.

The speaker emphasized that the "new era in meteorology" that provided these computer products was not an excuse to "sit back and take it easy." On the contrary, forecasters would now have more time to devote to their local forecasts, with the computer taking care of the large-scale forecast. Modeling results had revealed that topographic, coastal, and diurnal effects were more subtle than previously thought. This discovery would allow meteorologists to concentrate their efforts on other elements that might ultimately be more important to solving the forecasting problem. The computer "under intelligent human direction" would be the forecaster's assistant – not the controlling factor in making forecasts.[760]

And so it was. Or was it? Despite the comments that the results were very good from their shake-down runs, how acceptable would these computer products

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[760] "Suggested Remarks for Mr. Little at the Joint Numerical Weather Prediction Unit Opening Ceremony May 6, 1955," 4 May 1955 (Reichelderfer papers, B2, F10).

be to the men at the forecasting desk? How would they be viewed by the different weather services? What was the future of the resulting man-machine interface? How long would it take before this operational unit was truly operational? How long could a melding of personnel from different operational backgrounds, serving very different customer bases, function before the infighting led to its disintegration?

CHAPTER 9
EPILOGUE – A NEW ATMOSPHERE

The opening of the Joint Numerical Weather Prediction Unit was both a beginning and an end. It marked the conclusion of the initial research phase required to put NWP on a firm theoretical footing. But it was nonetheless only a shaky start: while billed as an operational entity, it was operational only in the most loosely defined terms. Computer-produced products were only used in-house, decisions about appropriate models remained, and model development continued – both within and outside the unit itself. Moreover, lots of "nuts and bolts" kinds of work still needed to be accomplished: hardware usage, data handling, data coding and transmission schedules.

As long as all these elements remained, it still seemed realistic to think that a joint organization combining the talents of the Air Force, Navy, and Weather Bureau meteorologists, mathematicians, programmers, and sub-professional assistants would lead to the quickest results. Indeed, until meteorologists were forced to get their new numerical forecasts out on some kind of schedule, i.e., before the forecast period started and not weeks or months later, there would be no impetus to clean up models to make them run faster or to deal with the long-term problem of obtaining and handling meteorological observations. Once some of these basic issues were under control, however, the climate started to change for the three weather services involved. That point was the beginning of the end for joint operational numerical weather prediction.[761]

Despite attempts by various government watchdogs over the course of the early 20th century to root out duplication in the provision of weather support, an indisputable fact was that the Army (later the Air Force), Navy, and Weather Bureau forecast the weather for very different audiences. Furthermore, each of these organizations possessed a very distinct culture that affected their way of providing services to their customers. These reasons had doomed anti-duplication efforts of forced jointness in the past. By 1960, they doomed this one as well.

The problems which foreshadowed the JNWPU's demise in 1960 were already starting to make their appearance within six months of the Unit's opening. There were four basic issues: the models to be used, their coverage (geographic and spatial), who (or what) would determine when computer products would replace hand-drawn products, and who (or what) would determine what products went out over the facsimile broadcast. Each service looked at these basic issues and had its own vision.

The Weather Bureau, which had administrative control, was focused on its customers in the continental United States, the majority of whom were on the ground. They did support aviation interests, but they were not supporting high-

[761] For short discussions which focus on model development within the Joint Unit and after, written by participants in the effort see Cressman, "The Origin and Rise of Numerical Weather Prediction"; Frederick G. Shuman, "History of Numerical Weather Prediction at the National Meteorological Center," *Weather and Forecasting* 4 (1989): 286-296; Thompson, "The Maturing of the Science" and "A History of Numerical Weather Prediction." See also Nebeker, *Calculating the Weather*, 160-161.

performance, high-flying aircraft. The Air Force was. They did support marine interests, but they were not supporting ships at sea. The Navy was. To leaders of the Weather Bureau, if they were going to provide the nation with numerical weather prediction products, those products had to be superior to anything currently available by subjective methods. *By law* they were responsible for providing the nation's weather service, and therefore would provide the best possible analyses and prognostic products. If that meant using NWP products as guidance, fine. If that meant substituting them for subjective products, fine too, as long as they were the very best that they had to offer.[762] This pragmatic, civilian-dominated mindset influenced every decision the Weather Bureau made.

The Navy had customers too, but they were at sea. To serve those customers, the Navy needed more coverage of the Atlantic. That meant that ocean-based data sources needed to be assessed and included into the models. In addition, the geographic extent of the charts needed to be expanded. Until that happened, the Navy argued that the JNWPU prognoses were of "academic interest only" and were of "little or no operational value." When the facsimile broadcast carrying charts of the United States went out to Navy ships, those on the receiving end were not getting what they needed to conduct their operations safely. The Joint Unit could not accommodate them without increasing their manpower by thirty percent. They

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[762] Statement by Mr. Vernon, Minutes of the 19th meeting of the SC/NAWAC held 26 March 1957 (Weather Bureau papers, JCS/JMC).

were already twenty-five percent undermanned as it was. The Navy was not going to get their products.[763]

The Air Force had ground forces to worry about as well, but their primary forecasting problem was airborne and it was *way* up there. Therefore, it needed numerical products which provided forecast information for high-altitude flight. Air Weather Service meteorologists wanted to see a multi-level baroclinic model in place, the sooner the better. Even more, the Air Force wanted to see the results from research centers brought in to the Joint Unit as soon as they were available. By December 1955, Air Force meteorologists were demanding closer ties with the GRD in Cambridge.[764]

But while the multi-level baroclinic models were acceptable to the Weather Bureau, they were not acceptable to the Navy. The barotropic models worked best over the Eastern Atlantic where the Navy was operating. A shift to a baroclinic model as *the* operational model threatened to leave the Navy meteorologists in a bind. They were already in one because of the lack of Pacific data. If they could not use the Atlantic charts, then NWP was doing nothing for them. Already getting testy in the spring of 1956, Navy leaders wanted outside reviewers brought in. Any model selections had to be made "without bias." The data to support these models – very-high-altitude reports for the Air Force and more oceanic reports for the Navy

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763 Minutes of the 5th meeting JMC/NWP held 28 September 1955. Minutes of the 6th JMC/NWP held 27 December 1955 (Weather Bureau papers, JCS/JMC).
764 Ibid.

– were also being debated for the same reasons. There was only so much money. Who was going to get the data important to them?[765]

As these skirmishes came and went, technological problems plagued the Joint Unit as well. The IBM 701, installed with great fanfare in 1955, was quickly overwhelmed by the models. Within a year of going on line, its capacity was almost exceeded. It would take at least a year to get a new IBM 704, and Unit members already realized the models would take over its full capacity shortly after delivery. The cost would double. If all the weather services did not hang together as a group, the Weather Bureau was not sure they could all move forward. As Cressman argued, efforts to expand meteorological computing should be done "as part of one United States system and such expansion should receive careful joint study and action."[766] Cressman voiced his concerns in June 1956, when none of the services had openly discussed pulling its resources out of the Joint Unit.

Part of the reason the Unit disintegrated by the end of the 1950s can be placed on different styles and approaches to modeling: the Weather Bureau had a more theoretical approach than the "let's-just-make-it-work" approach of the Navy. Yet the reason that was most widely publicized involved the content and control of the facsimile broadcast. The facsimile broadcast was the communications method used to send completed weather maps out over the airwaves. Those charts went out on a schedule – the same type chart was transmitted at the same time each day.

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[765] Comments on the 352nd meeting of the JMC held 20 March 1956 (Weather Bureau papers, JCS/JMC).
[766] Minutes of the 355th Meeting of the JMC held 18 June 1956 (Weather Bureau papers, JCS/JMC).

Generally, the broadcast schedule was full. Therefore, if a new map were to be placed on the schedule, another map had to be removed. The Weather Bureau controlled the broadcast. Therefore, Weather Bureau leaders thought that any attempt to force their hand in either putting new weather maps on, substituting a computer produced weather map for a hand-drawn map, or pulling a weather map off entirely, was meddling in their internal affairs. Leaders in the Navy and the Air Force did not. If they agreed with a Weather Bureau decision after being consulted about it, that was fine. If they did not agree, they wanted the final decision to rest with the Joint Meteorological Group (formerly Committee) of the Joint Chiefs of Staff. After all, the Navy and Air Force were providing a considerable share of the funding, and these maps were going out to their activities. For their part, the Weather Bureau thought that when it came to the NWP problem, the Air Force personnel were "unfamiliar," "not well briefed," and "suspicious and overly-cautious." Perhaps, but the Navy agreed with its Air Force colleagues. So did the JMG. Weather Bureau meteorologists such as Cressman were not happy. In their eyes, the military weather services were moving into areas, operationally and in research, that were not unique to them. The Weather Bureau had recognized that within the context of the Cold War, there was more support for military funding than for civilian meteorological funding. The Bureau had gamely marched on, but its patience had worn thin. Reichelderfer thought the responsibilities of each service needed to be clearly delineated so funding could be adjusted accordingly.[767]

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[767] Cressman to J. Eberly, 21 October 1958. Minutes of the 371st meeting of the JMG held 13

By the time the fiscal year 1961 budget discussions came up, the Navy suggested that the Weather Bureau fund the entire operation (which had reached $1.7 million in 1960). The Air Force agreed, while indicating that they would not pull out immediately.[768] The Navy argued that military budgets were uncertain and they did not want to make a commitment. That was probably true: uncertainties were historically the norm. Yet they were rarely so uncertain that the cost of weather support could not be covered. The Navy was happy to participate in the funding when the JNWPU was basically a research organization. Now that it was operational, the Navy argued that the Weather Bureau needed to fund NWP themselves. Although this round of budget disputes was overruled by the Bureau of the Budget, the JNWPU had few remaining days.[769]

The end came in the waning days of the Eisenhower Administration. In January 1961, the Navy announced that it was opening its own Fleet Numerical Weather Facility on the grounds of the Naval Postgraduate School in Monterey, California. Its announced mission was to provide operational numerical weather products peculiar to the needs of the U.S. Navy, including the development and testing of numerical techniques in both meteorology and oceanography. The Navy packed up all its personnel and moved west.[770]

January 1959. Weather Bureau comments on the 371st meeting. Minutes of the 372nd meeting of the JMG held 24 February 1959. Weather comments on the 372nd meeting. (Weather Bureau papers, JCS/JMG).

768 Minutes of the 373rd meeting of the JMG held 7 April 1959 (Weather Bureau papers, JCS/JMG).

769 Minutes of the 381st JMG meeting held 16 February 1960 (Weather Bureau papers, JCS/JMG).

770 Minutes of the 147th Panel/WP meeting held 17 January 1961 (Weather Bureau papers, JCS/JMG).

Already by that time, the Air Force had set up its own computer facility at Offutt Air Force Base, Nebraska, and was processing its classified products separately. By the end of the summer 1961, the JNWPU was subsumed under the Weather Bureau's National Meteorological Center. After fifteen years of more-or-less joint cooperation on numerical weather prediction since the end of World War II, the three weather services were all poised to go it alone.[771]

In subsequent decades, model development and operational products would burgeon. The Air Force, Navy and Weather Bureau all would develop their own distinct models to best serve their particular customers. The academic community concentrated on theoretical modeling efforts at such places the Geophysical Fluid Dynamics Laboratory of Princeton University, the National Center for Atmospheric Research in Boulder, Colorado, and in university atmospheric sciences departments all over the United States. Modeling efforts expanded overseas too – first in efforts confined to individual countries and then to joint efforts like the European Center for Mid-Range Weather Forecasting. With each new generation of computers, models could include more variables, different approximations, time-steps, and geographic areas. Meshes got finer and time periods extended into centuries with

[771] Enclosure to Item 2, Briefing by the Director, NMC to the JMG on Numerical Weather Unit Matters dated 8 August 1961 (Weather Bureau papers, JCS/JMG).

climate modeling. Today the search continues for answers to the very large and variable puzzle that is the atmosphere.[772]

[772] For a discussion of weather and climate modeling throughout the world see John Houghton, "The Bakerian Lecture, 1991: The Predictability of Weather and Climate," *Philosophical Transactions: Physical Sciences and Engineering* 337 (1991): 521-572. For a historical discussion of early work in Sweden and Germany, and later work at ECMWF, see Aksel Wiin-Nielsen, "Numerical Weather Prediction. The Early Development with Emphasis on Europe," 29-50; Heinz Reiser, "The Development of Numerical Weather Prediction in the Deutscher Wetterdienst," 51-80; and Lennart Bengtsson, "The Development of Medium Range Forecasts," 119-138; all in *50th Anniversary of Numerical Weather Prediction Commemorative Symposium, 9-10 March 2000, Book of Lectures*, ed. Arne Spekat (Berlin: Deutsche Meterologische Gesellschaft e.V., [2000?]). Other articles in this collection address the advances in numerical weather prediction, the future of NWP, and the gradual transition into climate prediction.

CHAPTER 10
CONCLUSION

At the beginning of the twentieth century, meteorology and weather forecasting were one and the same: in the minds of the public, in the minds of scientists, and, for the most part, in the minds of meteorologists in the United States. This perception is not surprising. Virtually all American meteorologists worked for the Weather Bureau. Its mission was to provide forecasts. It had no research mission.

While European nations also had national weather services, meteorological research *was* part of their mission. Funded at a much higher level than their American colleagues, the Europeans were in a better position to develop and mix theory with practice. The almost impenetrable divide between theoreticians and applied meteorologists that existed in the United States was, therefore, ameliorated in Europe. Theoretical advances such as the air-mass analysis, and later polar front theory, of the Bjerknes's Bergen School were put into daily practice. Such cross-pollination of theory and practice would have been impossible in the United States. As a consequence, the advance of meteorology as a science suffered in the United States.

Europeans also viewed meteorology as a science on par with astronomy and other physical sciences. Indeed, the concept of geophysics – the methods of physics applied to the earth sciences – was already well established in Europe.[773] In the

[773] Oreskes and Doel, "Geophysics and the Earth Sciences."

United States, meteorology lurked on the margins of both geography and physics. Not being fully welcomed into either discipline, it always seemed on the verge of being squeezed out. And finally it was. The geographers were intent on building up climatology which better fit their disciplinary focus. The mathematics and physics upon which meteorology would depend for its advancement had no place in the geography curriculum.[774] On the other hand, meteorology was not typically seen as rigorous enough for the physicists: there were too many immeasurable variables and unsolvable equations. Moreover, the problem of making daily forecasts made it a "guessing" science. Meteorology was a science without a home, and yet not strong enough to stand on its own.

As often is the case in the history of science, war made the difference. World War I military aviation assets, extraordinarily flimsy by the standards of the twenty-first century, had to be protected from the vagaries of the weather if they were to carry out their missions effectively. Those same aviation assets brought information from higher altitudes – information that had been routinely unobtainable until the 1920s. Studying the atmosphere from the earth's surface had never been terribly successful. With fixed-wing airplanes and rigid airships plying the skies, aeronautics and meteorology became inextricably tied. They needed each other. Funding that had not been forthcoming for "surface" weather services was forthcoming for "aviation" weather services.

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[774] Livingstone, *The Geographical Tradition.*

Military weather services, operating with virtually no trained personnel, had a desperate need for advanced training. The Navy's weather service, under the leadership of Francis Reichelderfer, arranged for the first graduate courses in meteorology at MIT. The guiding force behind that program, Carl-Gustav Rossby, emerges as the most influential meteorologist of the middle twentieth century. This step, in both operational and academic meteorology, was critical to the introduction of Bergen School methods to meteorologists in the United States. There were only a few students at first, but the numbers grew. Bergen School leaders and acolytes spread news of their techniques on both coasts. A core of theoretically-minded meteorologists began to take form, and academic programs took root at New York University and Caltech. With the founding of programs at the University of Chicago and UCLA, the "Big Five" schools were able to provide training to thousands of military men and women during the Second World War. Between 1900 and 1945, the meteorology community had been transformed from a community composed of a few hundred in-house trained Weather Bureau weather forecasters to a community composed of thousands of university-educated, mathematics- and physics-savvy meteorologists – the very meteorologists needed to advance meteorological theory and create numerical weather prediction.

The standard numerical weather prediction story, as told by historian of science Frederik Nebeker and historian of technology William Aspray, and indeed by meteorologists themselves, has been the John von Neumann story. Von Neumann was, indisputably, a brilliant mathematician and computer creator. He had pursued hydrodynamical problems during World War II. There probably was nothing that he could not figure out. Although von Neumann's name was on the contract with the Office of Naval Research, the driving forces behind the Meteorology Project were *meteorologists.* Meteorologists provided meteorological theory and meteorological applications. Without meteorologists there would have been no numerical weather prediction.

Von Neumann's contribution to numerical weather prediction was a large one: he created the computer, he provided the numerical analysis techniques, and he solved the mathematical and programming problems inherent in initial condition and boundary layer problems. That he is credited with the success of the *entire* project is a case study in itself of Robert K. Merton's "Matthew Effect."[775] The Matthew effect results in the recognition of scientific contributions going to the individual in the project with the greatest reputation, whether or not that person actually made the contribution.

In the case of the Meteorology Project, all of the meteorologists involved in the project were unknown outside the meteorology community. Some were even unknown *within* the community. Indeed, most were very young – having only entered the field during the war. The most distinguished meteorologist of the day – Rossby – was completely unknown to the members of the Institute for Advanced Study. The archival evidence is clear: Rossby was the *de facto* leader of the

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[775] Robert K. Merton, "The Matthew Effect in Science" in *The Sociology of Science: Theoretical and Empirical Investigations*, Norman W. Scorer, ed. (Chicago and London: The University of Chicago Press, 1973).

Meteorology Project. He provided ideas, encouragement, personnel, and publication venues. Jule Charney was the on-site leader. He provided the equations, based on early work with Rossby, and looked to Rossby as an advisor and intellectual sparring partner. Charney's desire: to turn meteorology into a theoretically-based science. Francis W. Reichelderfer, Chief of the Weather Bureau, with no funds of his own, had drawn the people and agencies together so that financial support would flow to the Project. He provided analysts, he provided encouragement, and through his assistant, Harry Wexler, he maintained regular contact between the operational Weather Bureau and the researchers of the Project. Having seen first-hand the effects of a poorly-trained, bunker-mentality ridden Weather Bureau, Reichelderfer was determined to advance the standing of his agency through operational numerical weather prediction. Philip D. Thompson, brilliant and ambitious, one of the new breed of mathematical meteorologists, wanted the Air Weather Service to be similarly advanced. Leaving the Meteorology Project, he effectively established a competing project at the Air Force's Geophysics Research Directorate in Cambridge, Massachusetts. The Navy's Daniel F. Rex, representing the Office of Naval Research, saw numerical weather prediction as the path to better meteorological support for Navy assets. These men – all but anonymous, bit-players in the history of the Meteorology Project – are the ones most responsible for its success.

Equally important was Rossby's role as the leader of a research school – one of the most significant research schools of the mid-twentieth century, and one of the least-studied.[776] Virtually every meteorologist involved in this Project was tied in some way to Rossby. The provision of the "Scandinavian Tag-Team" members, those meteorologists with the synoptic and dynamic meteorology backgrounds necessary for data analysis and theory development, was directly attributable to Rossby's intervention and behind-the-scenes leadership of the Meteorology Project. Meteorologists with that combination of talents were not available in the United States. Without the contributions of the reality-grounded, tag-team meteorologists, the mathematically-sterile models feared by Charney would have become a reality. Instead of numerical weather prediction, meteorologists would have been left with numerical weather theory.

Six years into the Project, after two "expeditions" with the ENIAC computer and then model runs on von Neumann's own computer, it became obvious to Charney and Rossby that only computer production of operational weather charts would advance the Project. While it was fine to spend a day producing one weather map to test model validity, operational requirements would be much more demanding. The computer had to be reliable, it had to have enough memory, and the models had to be stable.

But while Charney envisioned that the operational offshoot of the Meteorology Project would, like the Project itself, be a joint academic-Navy-Air

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[776] One of the defining characteristics of the research school that Rossby created was its determined international character, despite persistent Cold War pressures, an issue that is not explored in the essays comprising the "Research Schools" volume of *Osiris* [Gerald L. Geison and Frederic L. Holmes, ed., *Osiris* Second Series 8 (1993)]. As noted in Chapter 6, Rossby's research school, which deeply influenced meteorological practice in the United States and Europe, merits further attention.

Force-Weather Bureau effort, Thompson did not. The accounts to date – even those of the participants themselves – would leave us to believe that this harmonious working group just continued on with a shift to Weather Bureau administration. It did not. The archival evidence clearly indicates Thompson attempted to co-opt operational numerical weather prediction for the Air Weather Service for the purpose of using his own models and shutting out the Navy and Weather Bureau. Not only did he anticipate establishing numerical weather prediction centers in the United States to fulfill Air Force needs, he was looking to Europe to establish centers to support Air Force assets there. This attempt was the act that precipitated the August 1952 meeting that led to the decision to form a joint operational unit. That meeting was led by Charney and von Neumann, and it was arranged by the Weather Bureau under the ruse of needing information for a budget hearing. As a result, the Air Force was essentially precluded from having a role in the early stages of operational numerical weather prediction. The Navy's Rex and the Weather Bureau's Reichelderfer took the first steps with the Joint Meteorological Committee to get an *ad hoc* team together to explore the establishment of the Joint Numerical Weather Prediction Unit.[777] The Weather Bureau's Smagorinsky and the IAS's Goldstine spearheaded the computer selection. The Weather Bureau handled

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[777] While funding and support of scientific research *within* the military services is a critical issue, we alas know very little about these developments, as most historical studies in the post-1945 period have focused on university and other largely civilian scientific efforts (important exceptions include DeVorkin, *Science With a Vengeance* and Weir, *An Ocean in Common*. Much more historical research, based on a wide range of archival materials beyond the limiting confines of individual service records, is needed to comprehend the contributions military scientists made in the second half of the twentieth century.

the logistics. Reichelderfer did not need to have numerical weather prediction forced upon him – he led the operational charge from the very beginning of the Meteorology Project. His friend Rossby led the theoretical charge.

The Meteorology Project was led, nurtured, and succeeded because of the efforts of an international meteorology community and the nascent *professional* meteorology community in the United States. It grew in less than a century from a tiny, dispersed, under-funded research field to a community at the forefront of major scientific discoveries and policy issues. In contrast to natural history, perhaps the only other discipline at the start of the twentieth century that also employed large numbers of amateurs in its ranks, meteorology experienced a spectacular rise in stature and disciplinary authority.[778] This extraordinary professional and disciplinary transformation can be laid at the feet of numerical weather prediction.

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[778] On the fortunes of natural history in the nineteenth and twentieth centuries, see Paul Lawrence Farber, *Finding Order in Nature: The Naturalist Tradition from Linnaeus to E. O. Wilson* (Baltimore, Maryland: Johns Hopkins University Press, 2000).

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APPENDIX: GLOSSARY

GLOSSARY

Absolute vorticity: the vorticity (spin) of a fluid particle determined with respect to an absolute coordinate system.

Acceleration: any change, either in speed or direction, of an air parcel.

Advection: the movement of a physical characteristic of the atmosphere by the wind flow. For example, warm thermal advection would occur if air blowing over a warm land mass increased its temperature and then flowed over a cooler land mass, thus increasing the temperature of the air over the land.

Advective model: a model that is based on discrete advection terms, i.e., atmospheric properties transported by mass motion, with less or no emphasis on forcing, dissipation, and physics.

Baroclinic model: includes the horizontal movement (advection) of the initial circulation field and the advection of the temperature fields, an explicit representation of the thermodynamic energy equation, and at least two vertical data levels. The advection of thermal characteristics is required for the prediction of the development of new systems.

Barotropic model: single-parameter, single-level model based only on the horizontal movement (advection) of the initial circulation field. To use this model, one must assume that pressure and temperature surfaces are coincident. Hence, it is unable to predict the development of new weather systems.

Blocking high: a large area of high pressure that interferes with a zonal flow patterns and is characterized by dry, sinking air.

Col: the intersection of a trough and ridge on a surface pressure map.

Coriolis force: the force which causes the apparent deflection of a particle due to the rotation of the earth underneath it.

Cyclone: large-scale regions of low pressure which turn counter-clockwise in the northern hemisphere (clockwise in the southern hemisphere). In meteorological usage, a cyclone is not a tornado or similar small-scale disturbance.

Divergence: the spreading out (if positive) or coming together (if negative) of the vector field representing motion of air parcels in the atmosphere.

Dropsonde: a version of the *radiosonde* which is dropped from an aircraft and falls through the atmosphere.

Dynamic meteorology: the branch of meteorology which deals with the solution of hydrodynamical and thermodynamical equations as related to the full range of atmospheric motion.

Equivalent baroclinic model: Philip D. Thompson's model which included horizontal space coordinates and time as independent variables, and temperature and contour height of a constant pressure (isobaric) surface at an identifiable 'level of equivalence.' Although the equations were linear, in two-dimensions they could be solved without the use of high-speed digital computers.

Equivalent barotropic model: an enhanced version of the standard barotropic model such that the variation in the wind with height is averaged in the vertical.

Front: the discontinuity between two different air masses.

Frontogenesis: the process whereby a front is "born."

Green's function: a function that is the known solution of a homogeneous differential equation of a specified region and that may be generalized (if the equation is linear) to satisfy given boundary or initial conditions, or a nonhomogeneous differential equation. It is an alternative to the Fourier or Laplace transforms.

Gravity wave: a wave disturbance in which buoyancy is the restoring force on parcels displaced from hydrostatic equilibrium.

Internal wave: a wave in fluid motion having its maximum amplitude within the fluid or at an internal boundary.

Isentropic chart: a synoptic chart with plotted meteorological elements (pressure, wind, temperature moisture) on a surface of constant potential temperature.

Jacobian: the determinant formed by the n^x partial derivatives of n functions of n variables, when the derivatives of each function occupy one row of the determinant.

Kinematic boundary condition: the condition that the fluid velocity directed perpendicular to a solid boundary must vanish on the boundary itself.

Lagrangian coordinate system: one that requires that a fluid parcel be identified for all time by assigning it coordinates which do not vary with time. Therefore, very few meteorological observations are Lagrangian – to be so one would need to take observations of the exact same air parcel over time.

Linear differential equations: contain dependent variables which are raised to the first power (first-degree algebraic terms). There are many mathematical techniques for solving such equations.

Long (or planetary) waves: atmospheric disturbances having wavelengths on the order of the earth's radius.

Meridional flow: air movement is said to be "meridional" if it moves roughly parallel to lines of longitude.

Non-adiabatic process: one which involves an exchange of heat between an air parcel and its surroundings or environment. Same as a *diabatic process*.
Non-linear differential equations: contain dependent variables which are other than first-degree algebraic terms. They may be solved by numerical analysis techniques or by making approximations that render them 'linearized.'

Orography: the branch of physical geography which deals with mountains.

Overrelax: a relaxation technique whereby the "guess" is made to overshoot the target value and then gradually converge to a solution.

Poisson equation: a differential equation of the form $\nabla^2\Phi = F$, where ∇^2 is the Laplacian operator, Φ is a scalar function of position, and F is a given function of the independent space variable.

Potential temperature: the temperature an unsaturated parcel of dry air would have if brought adiabatically (i.e., without heat transfer from or to its environment) from its initial state to a standard pressure of 1000 millibars (or 100 kilo Pascals).

Primitive-equation model: the seven equations with seven unknowns governing atmospheric motions, i.e., the equations forming Newton's laws of motion, the principle of mass conservation, the first law of thermodynamics, and the Boyle-Charles law, were complete and recognized as relevant to atmospheric prediction by Vilhelm Bjerknes. Taken together they form the hydrodynamical equations.

Quasi-geostrophic: a fluid is quasi-geostrophic if the time scale on which a system evolves is slow compared to the rotation period of the earth and the length scale is larger than the distance cold pools of air can spread under the influence of the Coriolis force. Horizontal motions are geostrophically balanced and vertical motion is limited.

Polar front: the semi-permanent front separating tropical and polar origin air masses.

Relaxation method: a method whereby successive approximations are used starting with an initial guess. The error of the guess is reduced by an improved guess until the error falls below some preassigned value. A *Liebman (or sequential) relaxation* converges to a solution more rapidly because each new guess is used immediately in computing the new guess of an adjacent point in the grid.

Radiosonde: a meteorological instrument used to measure temperature, relative humidity, pressure, wind speed and direction with height. The battery powered sensors and transmitters are packed into a box and launched with a balloon inflated with helium or hydrogen. As it rises, the transmitter sends the information down to a ground receiving station.

Ridge: on a weather chart, an elongated area of high pressure.

Signal velocity: the propagation speed of a hydrodynamic influence.

Stable system: a system is "stable" if small disturbances have only small effects, i.e., it will return to its equilibrium state; it is "unstable" if a small disturbance generates or leads to a large effect. An unstable system does not return to an equilibrium state.

Static stability: the ability of a fluid at rest to resist becoming turbulent or wavy due to the effects of buoyancy. Also referred to as *hydrostatic stability* or *vertical stability*.

Stratosphere: the second "layer" in the atmosphere, it extends from the top of the troposphere (approximately 10-17 km above the earth's surface) to the bottom of the mesosphere (approximately 50 km above the earth's surface.

Stream function: a parameter of two-dimensional, non-divergent flow with a value that is constant along each streamline, i.e., a line with its tangent at any point in a fluid parallel to the instantaneous velocity of the fluid at that point.

Synoptic meteorology: the branch of meteorology which studies and analyzes surface weather observations made at periodic times (usually in three- or six-hourly intervals as dictated by the World Meteorological Organization). Examples of observed elements are: temperature, wind velocity, atmospheric pressure, and sky cover, i.e., type and extent of clouds.

Three-dimensional: adds the vertical space coordinate to the two-dimensional space.

Troposphere: the lowest layer of the atmosphere, it occupies the space between the earth's surface and the bottom of the stratosphere (approximately 10-17 km above the surface).

Trough: on a weather chart, an elongated area of low pressure.

Two-dimensional: horizontal space coordinates and time as independent variables.

Vortex: an area characterized by vorticity or spin.

Vorticity: a measure of local rotation of a fluid flow; spin.

Zonal flow: air movement is said to be "zonal" if it moves parallel to lines of latitude. The prevailing zonal flow is from the west (westerlies) in the mid-latitudes (30° to 60°), and from the east (easterlies) in the tropics (0° to 30°) and the polar region (60° to the pole).