Climatology as a Profession

Through the first half of the 20th century, climatology was a nearly stagnant field. The prevailing view saw climate as a static average condition, pinned down by tedious statistics. The study of climate change *(what to many climatologists seemed a contradiction in terms) was only an occasional interest of individuals who worked in divergent ways, and scarcely knew of one another's existence. The Second World War and Cold War promoted a rapid growth of meteorology and other fields of geophysics. But the dozens of scientific specialties that might have something to say about climate remained mostly isolated from one another. In the 1960s, the rise of interdisciplinary institutions and large-scale international projects, combined with concerns about climate change, began to bring the diverse fields into contact. People interested in climate change kept their identification with their individual disciplines rather than forming a distinct community of their own, while communicating through various means that cut across disciplinary boundaries. Around the start of the 21st century, the International Panel on Climate Change institutionalized an uprecedented process of workshops and exchanges, building a community of experts who forged a consensus on what could be reliably said about global warming (see the essay on International Cooperation.)*

"We cannot hope to understand the causes of climatic stability or change by restricting ourselves to any one field of earth science. Nature is ignorant of how our universities are organized..." — *Peter Weyl*[1]

At the middle of the 20th century the study of climate was a scientific backwater. People who called themselves "climatologists" were mostly drudges who compiled statistics about weather conditions in regions of interest—the average temperatures, extremes of rainfall, and so forth. That could have offered a broad global perspective, but most climatologists set the planet as a whole aside and attended to regional problems. The people who needed climate information were farmers planning their crops and engineers designing dams or bridges.[2] This did not mean climatologists overlooked unusual weather, for it was precisely the decade-long drought or unprecedented flood that most worried the farmer or civil engineer. But people saw such catastrophes as just part of the normal situation, transient excursions within an overall state that looked permanent on the timescale of human society. The job of the climatologist was to remove uncertainties with statistics, fixing the probable size of a "hundred-year flood" and so forth.

Typical was the situation at the U.S. Weather Bureau, where an advisory group reported in 1953 that climatology was "exclusively a data collection and tabulation business."[3] Not much money

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[1] Weyl (1968), p. 60.

[2] For civil engineers demanding more data, see Genuth (1987), p. 244.

[3] United States (1953), p. 24.

or administrative attention was committed to such work, nor were the intellectual prospects enticing. To the extent workers had research plans, their aim was just to find better ways to synthesize piles of data. A climatologist was somebody who *described* climate––mainly at ground level, where the crops and structures were found. These climatologists' products were highly appreciated by their customers (such studies continue to this day). And their tedious, painstaking style of scientific work would turn out to be indispensable for studies of climate change. Still, scientists regarded the field (as one practitioner complained) as "the dullest branch of meteorology."[1] Another expert remarked that in the study of climates, "the scientific principles involved are barely mentioned... Whether they are right or wrong does not seem to be of any moment, because they are never used to *calculate* anything."[2]

When climatologists did try to go beyond statistics to explanations, they would explain the temperature and precipitation of a region in geographical terms—the sunlight at that particular latitude, the prevailing winds as modified by mountain ranges or ocean currents, and the like. The explanations were chiefly qualitative, with more hand-waving than equations. This was close to the field called physical geography, a matter of classifying climate zones, with less interest in their causes than their consequences. If in the first half of the 20th century you looked in a university for a "climatologist," you would probably find one in the geography department, not in a department of atmospheric sciences or geophysics (hardly any of the latter departments existed anyway). The geographical way of explaining regional climates was an essentially static exercise loosely based on elementary physics. The physics itself was useless for telling farmers what they needed to know. Attempts to make physical models of the simplest regular features of the planet's atmosphere (for example, the trade winds) failed to produce any plausible explanation for how the winds circulated, let alone for variations in the circulation.

This failure was hardly surprising, since meteorologists did not have an accurate picture of what they were trying to explain. Few measurements existed of the winds and moisture and temperature above ground level, and even ground-level data were scanty for vast portions of the globe. Most textbooks of climatology accordingly stuck to listing descriptions of the "normal" climate in each geographical zone, compiled by authors who, as one scientist complained, "know little, and care less, about mathematics and physical science."[3]

Climatology could hardly be scientific when meteorology itself was more art than science. If the general circulation of the atmosphere was a mystery, still less could anyone calculate the course of storm systems. People had a variety of techniques for making crude weather forecasts. For example, while climatologists tried to predict a season by looking at the record of previous years, meteorologists similarly tried to predict the next day's weather by comparing the current weather map with an atlas of similar weather maps from the past. More often a forecaster just looked at

[1] H. Lamb quoted in Alexander (1974), p. 90.

[2] For 19th-century origins of climatology, see Nebeker (1995), pp. 24-25; on the empiricism of the field, Eady (1957), quote p. 113, emphasis in original.

[3] Cressman (1996), pp. 382-85; "know little:" Stringer (1972), p. xii.

the current situation and drew on his experience with a combination of simple calculations, rules of thumb, and personal intuition.

This craft had little to do with scientific advances. As one expert remarked ruefully in 1957, the accuracy of 24-hour weather forecasts had scarcely improved over the preceding 30 or 40 years. A canny amateur with no academic credentials could predict rain at least as successfully as a Pd.D. meteorologist, and indeed most of the “professionals” in the U.S. Weather Bureau lacked a college degree. Aside from a handful of professors in a few pioneering universities (mostly in European geophysical institutes), meteorology was scarcely seen as a field of science at all, let along a science firmly based on physics. Meteorology, one academic practitioner complained to another in 1950, “is still suffering from the trade-school blues.”[1]

Some hoped that climate, averaging over the daily vagaries of weather, might be more amenable to scientific investigation. They tried to understand changes on a timescale of decades or centuries, and searched for regular climate cycles. While a few looked for possible physical causes, it was more common for a climatologist to avoid such speculation and carry out grinding numerical studies in hopes of pinning down recurrences and perhaps predicting them. Analysis of large sets of data turned up various plausible cycles, correlated perhaps with variations in the number of sunspots. These correlations invariably turned out to be spurious, further lowering the poor reputation of climate change studies.

The stagnant condition of climatology mirrored a deep belief that climate itself was basically changeless. The careers of climatologists—their usefulness to society—rested on the conviction that statistics of the past could reliably describe future conditions. Their textbooks *defined* “climate” as the long-term average of weather over time, an intrinsically static concept. As one practitioner later complained, “authoritative works on the climates of various regions were written without allusion to the possibility of change, sometimes without mention of the period to which the quoted observations referred.”[2] In part this approach reflected a simple absence of data. There were hardly any accurate records of daily temperatures, seasonal rainfall, and the like that went back more than half a century or so, even for the most civilized regions. The records were scantier still for less-developed countries, and mere fragments for the three-quarters of the world covered by water and ice. It seemed reasonable to assume that the existing records did reflect the average weather over at least the past few thousand years. After all, historical records back to ancient times showed much the same mixtures of frosts and rains, and the crops that went with them, in a given region. In fact, history gave only the crudest indications. Climatologists scarcely recognized their ignorance, relying explicitly or implicitly on old assumptions about the stability of nature. In other sciences like geology, experts found good reason to maintain that natural

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[1] Accuracy not improved: Mason (1957), p. 175, see p. 183; Koelsch (1996); College degrees: Byers (1976), p.1343. “Blues”: George Platzman to Jule Charney, 18 June 1950, Box 14:451, Charney Papers, Massachusetts Institute of Technology Archives, Cambridge, MA. On the low professional status of US meteorology see Harper (2006).

[2] Lamb (1959), p. 299.

processes operated in a gradual and uniform fashion. Ordinary people too mostly believed that the natural world was self-regulating. If anything perturbed the atmosphere, natural forces would automatically compensate and restore a self-sustaining "balance."

To be sure, at least one immense climate change was known and cried out for investigation—the ice ages. The stupendous advances and retreats of continental ice sheets were worth study, not because scientists thought it was relevant to modern civilization, but because they hoped to snatch the brass ring of prestige by solving this notorious puzzle. Both professionals and amateurs advanced a variety of simple explanations. Most of these amounted to no more than vague but plausible-sounding arguments presented in a few paragraphs. Each expert defended a personal theory, different from anyone else's. The few scientists who attempted to write down equations and calculate actual numbers for the effects managed to prove little, except at best that their ideas were not wildly astray by orders of magnitude.

The most acceptable explanations for the ice ages invoked geological upheavals to block ocean currents or raise a mountain range against prevailing winds. This was necessarily an interdisciplinary sort of theory. "It is impossible to separate the geological from the meteorological," as one meteorologist remarked, "as the two are expressions of the results of the same forces."[1] But the many pages that scientists wrote amounted only to elaborate hand-waving, unsatisfactory within either field. "I, for one," said the respected climatologist Hubert H. Lamb, "must confess to having been bewildered and left quite pessimistic by some discussions of climatic variation."[2] The very concept of "theory" became suspect in climate studies.

Theoretical models, whether of climate stability or ice-age changes, were usually pursued as a minor sideline when they were not just ignored. To study "the climate" of the planet as a whole was far less useful and promising than to study "climates" region by region. There was little point in attempting global calculations when all the premises were uncertain and key data were lacking. Given the enormous obstacles to reaching reliable results, and the prevailing view that the global climate could not possibly change on a timescale that would matter except to far future generations, what ambitious scientist could want to devote years to the topic?

Yet it is the nature of scientists never to cease trying to explain things. A few people worked to lift meteorology and climatology above the traditional statistical approach. Helmut Landsberg's 1941 textbook *Physical Climatology* and a 1944 *Climatology* textbook written by two other meteorologists demonstrated how familiar physical principles underlay the general features of global climate, and provided a rallying-point for those who wanted to make the field truly scientific. Many saw such studies as an exercise in pure mathematics, deliberately remote from the fluctuations of actual weather. As one scientist recalled, in the 1940s, "academic

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[1] Napier Shaw in discussion of Harmer (1925), p. 258.

[2] Lamb (1959), p. 4.

meteorologists would sometimes go out of their way to disclaim any connection with forecasting—an activity of dubious scientific standing."[1]

These textbooks came into use during the Second World War as meteorology professors trained thousands of meteorologists for the armed services. The training gave a big boost to the few universities where scientific meteorology already existed, and led to further expansion after the war. One example was the young geology student Reid Bryson, who was picked up by the Air Force and trained in weather forecasting. After the war he got a Ph.D. in meteorology and, finding himself unwelcome in the geography department at the University of Wisconsin, founded a one-man meteorology department there. In 1962, the National Science Foundation gave him funds to establish an important climate research center.[2] Another example was Edward Lorenz, who had intended to be a mathematician but was diverted into meteorology during the war, when the Army Air Corps put him to work as a weather forecaster. Bryson and Lorenz were among "a new breed of young Turks" who broke away from the tradition of climatology as a mere handmaiden to forecasting. (At any rate, that was how they saw themselves in retrospect.)[3]

Leading the movement was a group at the University of Chicago, where in 1942 Carl-Gustav Rossby had created a department of meteorology. Rossby was a Swede who had learned mathematical physics in Stockholm and spent two years at Vilhelm Bjerknes's institute in Bergen, Norway. It was in Bergen that some of the key concepts of meteorology had been discovered, notably the weather "front" (first recognized during the First World War and named in accord with the concerns of the time).[4] Rossby had come to the United States in 1925 to work in the Weather Bureau. Outstanding not only as a theorist but also as an entrepreneur and organizer, Rossby soon left the somnolent Bureau in disgust. In 1928 he created the nation's first professional meteorology program at the Massachusetts Institute of Technology. He did still more in Chicago, thanks to the ample wartime support for training military meteorologists. The department trained some 1,700 in one-year courses. Rossby also helped coordinate new programs for graduate training of meteorologists at several other American universities.[5]

Support continued after the war, as the Cold War and the expanding economy—especially the rapid growth of civil aviation—raised the demand for meteorologists.The Chicago group flourished. It was the first group anywhere to systematically develop physical models of climate, sending out numerous students to carry on the approach elsewhere. Meteorology began to gain a reputation in the United States (as it already held in Europe) as a true scientific discipline, and climatology followed. As Rossby remarked a few years later, basic questions of climate change,

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[1] Landsberg (1941, rev. ed. 1947, 1960), see the preface, p. iii; On the book's importance: Taba (1991), p. 97; Haurwitz and Austin's *Climatology* (and also Rossby) are noted by Smagorinsky (1991), p. 30; "academic... dubious": Sutcliffe (1963), p. 277.

[2] Bryson (1967), and personal communication, 2002.

[3] Koelsch (1996); "Turks": Smagorinsky (1991), p. 31; Nebeker (1995), ch. 9.

[4] Friedman (1989).

[5] Here and below I am grateful for the use of Doel (2001).

such as storage of heat in the oceans or the level of carbon dioxide gas in the atmosphere, "mean a completely new class of questions... In these investigations one is hardly interested in geographical distributions." Unlike the traditional regional climatologists, his group looked at the entire planet as a physical system.[1]

War-trained young meteorologists also moved into the U.S. Weather Bureau, where they found "the stuffiest outfit you've ever seen," as a member of the research-oriented new generation later recalled—"deadly, deadly dull... a backward outfit." An official report complained that "the Bureau has displayed an arbitrary and sometimes negative attitude toward new developments in meteorology originating outside the Bureau." As for climatology at the Bureau, in 1957 another report described it as more than ever a mere passive "subsidiary to the task of forecasting."[2]

Stagnation was unacceptable to those who recalled the invaluable contributions of meteorology to military operations during the war. The armed forces thought it no less important for their postwar global operations, even if the Cold War stayed cold. And if nuclear bombs exploded, meteorology would be especially vital for tracking the deadly fallout. The Navy and Air Force in particular continued to employ many hundreds of meteorologists. Besides, in keeping with the new respect for science that they had learned during the war, they supported a variety of academic researchers whose studies might ultimately make forecasting better. As for climate, some of these researchers held out the fascinating prospect of changing it deliberately. The advances that meteorology was making toward solid scientific understanding, combined with the lavish Cold War funding for all science, made for a rapid expansion and professionalization of climatology.

It helped that the entire area of geophysics, which included most of the fields relevant to climatology, was becoming stronger and better organized. Since early in the century there had been a few institutions, notably university institutes in Germany, that embraced a wide enough range of studies to take the name "geophysical." Already in 1919 an International Union of Geodesy and Geophysics was founded, with separate sections for the different fields such as terrestrial magnetism and oceanography. An American Geophysical Union was also created in 1919 as an affiliate of the U.S. National Academy of Sciences (although it would not become an independent corporation with an international membership until 1972). There followed a few other national societies and journals such as the *Zeitschrift für Geophysik*. Several German universities created formal programs teaching "*Geophysik*."

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[1] Byers (1959); Rossby (1959), quote p. 16. Reputation: Harper (2006).

[2] "Stuffiest": Athelstan Spilhaus, interview by Ron Doel, November 1989, AIP; see also Joseph Smagorinsky, "Climate's scientific maturity," in Baer et al. (1991), pp. 29-35, 31; "arbitrary:" United States (1953), p. 36, see pp. 3-4; "passive:" National Academy of Sciences (1957).

As a founder of the International Union remarked, it was not so much a union as a confederation.[1] The other early professional organizations likewise brought little cohesion. Through the 1920s and 1930s, very few institutions of any kind addressed geophysics in a broad sense. Most individuals who might be called geophysicists did their work within the confines of one or another single field such as geology or meteorology. In the scientific investigation of climate change, when I look over the more significant publications—or at any rate the ones I have used as references in the present study, found in the bibliography—a great variety of books and journals turn up. The only ones that stood out from the crowd in this period were the *Quarterly Journal* and *Memoirs* of Britain's Royal Meteorological Society, which together published 18% of the pre-1940 journal articles I have cited. The runner-up was the *Journal of Geology*, with 9%.

Beginning in the late 1940s, a more significant number of inclusive institutions appeared. Institutes of geophysics were created at American universities and under the Soviet Academy of Sciences, along with funding organs like the Geophysics Research Directorate of the U.S. Air Force. Another big boost came in 1957-58 when the International Geophysical Year pulled together thousands of scientists from many nations. They interacted with one another in committees that planned, and sometimes conducted, interdisciplinary research projects involving a dozen different "geophysics" fields.[2] Most of these fields were relevant to climatology.

The annual meetings of the American Geophysical Union became a rendezvous for divers fields, and for the same purpose the Union began publishing a *Journal of Geophysical Research* (expanded from the older and narrower *Terrestrial Magnetism*). However, for the scattered scientists engaged with climate change, the best meeting-place was *Tellus*, a "Quarterly Journal of Geophysics" that the Swedish Geophysical Society created in 1949. The journal's importance is evident in the list of papers that found their way into the bibliography that I compiled in my research for this study. During the decades 1940-1960, *Tellus* published some 20% of these papers, more than any other journal. (The runners-up were the American interdisciplinary journal *Science*, with 15%, the *Journal of Meteorology,* with 10%, and the *Quarterly Journal of the Royal Meteorological Society*, with 5%. The *Journal of Geophysical Research* accounted for only 3%, about equal to the *American Journal of Science* and the *Journal of Geology*.)

Some two-thirds of these papers were written in the United States—a much higher fraction than for earlier years.[3] This was partly because the rest of the civilized world spent the 1950s recovering from the war's devastation. It was still more because generous U.S. government support for geophysical research, based on Second World War successes, did not falter even when memories of the war faded. For the global military and economic concerns of the Cold War put geophysics near the head of the line for research funds.

[1] L.A. Bauer quoted in Good (2000), p. 286, q.v. for this topic in general.

[2] Doel (1997); Doel (1998).

[3] My rough count from a sample..

In geophysics as in all the sciences of the 20th century, expansion raised a risk of further fragmentation. Early in the century, so little had been known about anything in geophysics that the best scientists had broad knowledge of many aspects of the subject. For example, between the world wars Harald Sverdrup published research on the circulation of the atmosphere, the circulation of the oceans, glaciers, geomagnetism, and the tides, not to mention the ethnology of Siberian tribes. A few decades later, when knowledge had grown deeper and techniques had become more esoteric, hardly anyone could do significant work in more than one or two fields.

It was getting ever tougher for a scientist to become expert in a second field of knowledge. Few now attempted it, for the diversion of energy risked your career. "Entering a new field with a degree in another is not unlike Lewis and Clark walking into the camp of the [Native American] Mandans," remarked Jack Eddy, a solar physicist who took up climate studies in the 1970s. "You are not one of them... Your degree means nothing and your name is not recognized. You have to learn it all from scratch, earn their respect, and learn a lot on your own."[1] Some of the most important discoveries came from people like Eddy, who did spend years in a foreign camp. Another example was Nick Shackleton, who after studying physics (essential for laboratory work measuring isotopes) and mathematics (necessary for analysis of time series) became part of a research group that analyzed pollen in a university botany department.[2] Such combinations, however valuable, were uncommon.

The problem was particularly severe for climate studies. There had never been a community of people working on climate change. There were only individuals with one or another interest who might turn their attention to some aspect of the question, usually just for a year or so before returning to other topics. An astrophysicist studying changes in solar energy, a geochemist studying the movements of radioactive carbon, and a meteorologist studying the global circulation of winds, had little knowledge and expertise in common. Even within each of these fields, specialization often separated people who might have had something to teach one another. They were unlikely to meet at a scientific conference, read the same journals, or even know of one another's existence. Nor did theorists interact regularly with people who worked out in the field. As one climate expert remarked, "lack of interest has all too often characterized the attitude of physical scientists to the masses of information produced by botanists examining pollen deposits and the data turned out by geologists, glaciologists, entomologists, and others. These types of literature have never been part of their regular diet."[3]

To make communication still harder, different fields attracted different kinds of people. If you went into the office of a statistical climatologist, you could expect to find ranges of well-organized shelves and drawers stacked with papers bearing neat columns of figures. In later years the stacks would hold computer printouts, the fruit of countless hours spent coding programs. The climatologist was probably the kind of person who, as a boy, had set up his own home

[1] J.A. Eddy, interview by Weart, April 1999, AIP, p. 4.

[2] Shackleton (2003).

[3] Lamb (1997), p. 200.

weather station and meticulously recorded daily wind speed and precipitation, year after year. Go into the office of an oceanographer, and you were more likely to find a jumble of curiosities from the shores of the seven seas. You could hear adventure stories, like Maurice Ewing's tale of how he was washed overboard and escaped drowning by a hair. Oceanographers tended to be salty types, accustomed to long voyages far from the comforts of home, outspoken and sometimes self-centered.[1]

These differences went along with divergence in matters as fundamental as the sorts of data people acquired and used. The economic importance of weather forecasting meant that climatologists could draw on a century-old and world-wide network of weather stations. "Meteorologists use mainly standard observations made by technicians," as an oceanographer recently remarked, "while the much smaller number of oceanographers usually make their own measurements from a small number of research ships," often with instruments they had built for themselves.[2] The climatologist's weather, constructed from a million numbers, was something entirely different from the oceanographer's weather—a horizontal blast of sleet or a warm relentless trade wind.

On top of social and perceptual gaps were technical divergences. As one expert remarked in 1961, "The fact that there are so many disciplines involved, as for instance meteorology, oceanography, geography, hydrology, geology and glaciology, plant ecology and vegetation history—to mention only some—has made it impossible to work... with common and well established definitions and methods." Scientists in different fields might use standards so different, he said, "that comparisons between the results have been hardly possible."[3]

Meteorology itself had always been divided. The climatologists who gathered weather statistics and analyzed them were intellectually remote from the theorists, who worked up mathematical models based on physical principles rather than observations. Both often looked down on practical forecasters, who in turn had little faith in the professors' abstractions. Among all three types of meteorologist, very few worked on questions of long-term climate change.[4]

This fragmentation was becoming intolerable by the 1960s. More than half a century of reliable temperature measurements were now in hand from around the world, and they showed that global temperatures had risen. Meanwhile observations of the climbing level of carbon dioxide in the atmosphere brought a threat of serious future changes. Besides, scholarly studies that extended the climate record far into the historical past were revealing large climate shifts. Most

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[1] For strains on deep-ocean oceanographers, see Mukerji (1989), pp. 66-73; Ewing: Wertenbaker (1974), pp. 130-33.

[2] Indeed "meteorology and oceanography are practiced in a very different manner and by two largely non-overlapping groups of people... there are still relatively few meteorologists who have more than a superficial knowledge of the ocean, and vice versa." Charnock (1998), p. 623.

[3] Wallén (1963), p. 467.

[4] Nebeker (1995), pp. 1-2.

notable was evidence of a century or so of exceptional warmth in parts of medieval Europe and the North Atlantic (this was when the Vikings settled Greenland). There had followed winters so harsh that early modern times could be called a “Little Ice Age”—at least in some countries. Records were spotty at best for the world outside the North Atlantic region, but there too, evidence was emerging of anomalies such as centuries of prolonged drought. Apparently there was no such thing as a “normal” climate.

Painful experience drove the point home. One notorious case was the experience of firms that contracted to build dams in central Africa in the 1950s, and consulted with climatologists about the largest floods that could be expected according to past statistics. The firms then began construction, only to suffer “fifty-year floods” in each of the next three years.[1] Such experiences pulled the props out from the traditional climatology. The laboriously compiled tables of past statistics were plainly not reliable guides to the future.[2]

This unhappy fact was not easily accepted. As late as 1968, a textbook on *Climatology and the World’s Climate* said baldly, “The subject of climatic change is not given specific treatment in this book.”[3] Applied climatologists continued to base their projections of the future on their hoards of old statistics, simply for lack of anything better. Their work was in fact becoming increasingly useful. As the data base grew and methods of analysis expanded, climatological studies brought a better understanding of how warm spells affected crops, what factors contributed to floods, and so forth.[4] Nevertheless, during the 1960s more and more scientists realized that climate predictions could not rely only on past observations, but must use physical models and calculations. Traditionally “climate” had been defined as the weather in a region averaged over a period. For example, in 1935 the International Meteorological Organization had adopted the years 1901-1930 as the “climatic normal period.” Increasingly experts saw this was misleading. That thirty-year span had turned out to have weather far from what was “normal” in later decades, and indeed there might be no such thing as a set of decades that could define “normal” weather. Climate was something that changed continually under the impact of physical forces.[5]

The new thinking was displayed in full at a 1965 symposium held in Boulder, Colorado on “Causes of Climate Change.” While the meeting made little special impression at the time, in retrospect it was a landmark. For it deliberately brought together scientists from a fantastic

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[1] Floods: Lamb (1997), p.178, see passim for historical work.

[2] Lamb (1959); Lamb (1966a), p. ix.

[3] Rumney (1968), p. vii; in another widely used text, a chapter on climate change first appeared, with a new author, in 1980 (the fifth edition): Trewartha and Horn (1980).

[4] Cressman (1996).

[5] I have seen only one explicit statement about this at the time ("we are faced with an initial difficulty of definition which has far-reaching consequences..."), Robinson (1971), p. 12; similarly but implicitly, Barrett and Landsberg (1975), p. 18; for the history, see Lamb (1995), pp. 10-11.

variety of fields, experts in everything from volcanoes to sunspots. Presiding over the meeting was an oceanographer, Roger Revelle. Lectures and roundtable discussions were full of spirited debate as rival theories clashed, and Revelle needed all his exceptional leadership skills to keep the meeting on track.[1] Convened mainly to discuss explanations of the ice ages, the conference featured a burst of new ideas about physical mechanisms that could bring surprisingly rapid climate shifts. In his formal summary of the discussions, the respected climatologist Murray Mitchell reported that our “comparatively amicable interlude” of warmth might give way to another ice age, and sooner than had been supposed. That foreboding possibility required scientists to understand the causes of climate change, he said, and to suggest how we might use technology to intervene.[2]

This sort of thinking spread widely in the early 1970s. A spate of devastating droughts and other weather disasters showed that climate was grossly unreliable. With the alarming news came warnings that the near future might see still worse—whether global warming or drastic cooling—thanks to pollution of the atmosphere following the explosive growth of human population and industry. This was an active and even aggressive view of climate in relation to humanity. It called for aggressive research. “The old descriptive climatology,” an authority remarked in 1975, “concerned mainly with statistics and verbal interpretation of them, is evolving into a new mathematical, or dynamic, climatology with predictive capability based on physical-mathematical processes rather than extrapolation of statistical measures.”[3]

That required a new kind of research community, more closely linked to other fields and other kinds of science. This was happening in all the Earth sciences. The traditional observational geologist, out in the field with his high-laced engineer’s boots and rock hammer, had to make room for the investigator who saw rocks mainly in her laboratory, or perhaps only in pages of equations and calculations. Old-school geologists grumbled that the move to laboratory and theoretical geophysics took people away from a personal confrontation with nature in all its complexity and grandeur. The same filtering of experience was spreading in climate studies. Most scientists with something to contribute focused on technical problems peculiar to their own specialty. How do aerosols make clouds? How can you get a computer model to show the annual cycle of the seasons? What was the pattern of ancient glacial cycles? Those who did attack broader questions head-on seemed out of date. Some continued to propose simple hand-waving models with physical explanations for climate change (especially the ice ages). But the different explanations were patently speculative, infected by special pleading and mostly incompatible with one another.

Scientists were becoming skeptical of the traditional approach, in which each expert championed a favorite hypothesis about some particular cause for climate change—blaming every shift on

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[1] Mitchell (1968), p. iii-iv.

[2] Mitchell, "Concluding Remarks," drawing on the remarks by Roger Revelle, in Mitchell (1968), p. 157.

[3] Barrett and Landsberg (1975), p. 76; see Lamb (1995), pp. 12-14.

variations in, say, dust from volcanoes or the amount of sunlight. It seemed likely that many factors contributed together. Meanwhile the factors were interacting with one another. And on top of these external influences, it appeared that some part of climate change was self-sustaining, through feedbacks involving the atmosphere, ice sheets, and ocean circulation. "It is now generally accepted," wrote one authority in 1969, "that most climatic changes... are to be attributed to a complex of causes."[1]

The shortcomings of the old single-cause approach were especially visible to those who tried to craft computer models of climate change. A plausible model could not be constructed, let alone be checked against real-world data, without information about a great many different kinds of things. It became painfully clear that scientists in the various fields needed one another. Specialists began to interact more closely, drawing on one another's findings or, equally valuable, challenging them.

These changes in geophysics were typical of a movement in all the sciences. For more than a century many fields of science had narrowed their perspective to simplified cases, pursuing solutions as compact and elegant as Newton's equations. Subjects as far afield as sociology were swayed by what some began to call "physics envy." Only a few scientists insisted on looking instead at whole systems with all their complexities. That approach began to spread in various fields during the postwar years, and a growth spurt in the 1970s brought into prominence what was coming to be called "holistic" investigation. In biology, for example, different disciplines were talking to one another within the increasingly popular field of ecology. This was timely, for scientists were increasingly concerned that biological communities were yet another feature that interacted intimately with the planet's climate. Some specialists had long been aware of such interactions—most notably in oceanography, which was explicitly a union of physical oceanography and biological oceanography (if only because the researchers had to bunk alongside one another on their voyages). Now all of geophysics was coming to be seen as part of a larger field, the "Earth sciences."

In the fields relating to climate, as in other sciences, textbooks and review articles in ever growing numbers summarized the recent findings of this or that specialty for the benefit of outsiders. More and more conferences were held with the aim of bringing together anywhere from a dozen to several hundred people from different but relevant fields. Most scientists, however, continued to call themselves oceanographers or computer scientists or paleobotanists or whatever. Not many would identify themselves as primarily a.... a what? A "climate change scientist?" There was not even an accepted term to describe the non-discipline. The typical landmarks for the creation of a discipline, such as departments at universities or a scientific society named for the subject, never came. The key elements for any profession—socialization and employment, which for scientists usually meant training as a graduate student and employment as a professor—remained firmly fixed within traditional disciplines like meteorology or oceanography. Research on problems directly related to climate change usually

[1] Lamb (1969), p. 178.

began only at the postdoctoral stage or later, and was often done in some sort of interdisciplinary institute or project rather than within an academic department.

In 1977, one landmark for the recognition and coalescence of a scientific discipline did come with the foundation of a dedicated journal, *Climatic Change*. But unlike many new journals, this one did not in fact launch itself as the flagship of a new discipline. Its explicit policy was to publish papers that were mainly interdisciplinary, such as explorations of the consequences that global warming might have on ecosystems.[1] Most scientific papers on climate change itself continued to be published in journals dedicated to a specific field, like the meteorologists' *Journal of the Atmospheric Sciences* or the paleontologists' *Quaternary Research*. But key papers were also welcomed by the two great interdisciplinary scientific journals, *Science* and *Nature*, where specialists in every field would see them. (In my bibliography for 1960-1980, *JAS* published 10% of all papers and *Quat. Res.* 7.5%. *Science* published 23%, if one includes a few news articles, and *Nature* 10%. *Tellus* was down to 5%, equal to the *J. Geophysical Research,* followed by the *Journal of Applied Meteorology* at 4%. The *Quart. J. Royal Met. Soc.* fell to 2.5%.)

On the whole, climate science remained "a scientific backwater," as one of its leading figures recalled decades later. "There is little question," he claimed, "that the best science students traditionally went into physics, math and, more recently, computer science."[2] The study of climate was not a field where you could win a Nobel Prize or a million-dollar patent. You were not likely to win great public fame, nor great respect from scientists in fields where discoveries were more fundamental and more certain. In the mid 1970s, it would have been hard to find a hundred scientists with high ability and consistent dedication to solving the puzzles of climate change. Now as before, many of the most important new findings on climate came from people whose main work lay in other fields, from air pollution to space science, as temporary detours from their main concerns.

Coordination and communication nevertheless improved as climate science was swept along by changes in the sciences as a whole. During the 1960s and 1970s, governments doubled and redoubled the budgets for every field of research, and geophysics got its share. Scientists concerned about climate change worked to get governmental and international agencies to organize their diverse research efforts through a central office or committee. It took decades of failures and false starts, but by the end of the 1970s, they managed to put together a number of ambitious climate programs. While still lacking central coordination, each of the programs

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[1] Edited by Stephen Schneider. Further, in 1983 the *Journal of Applied Meteorology* became the *Journal of Climate and Applied Meteorology,* absorbed in 1988 into the *Journal of Climate*, which had begun in 1986. Policy: Schneider (1991).

[2] The claim was made to warn policy-makers about the unreliability of climate predictions, but it is plausible. Richard Lindzen, Testimony before the Senate Environment and Public Works Committee, May 2, 2001, available as appendix to United States Congress (107:1) (2001).

embraced a variety of fields. In particular, the United States established a National Oceanic and Atmospheric Administration that united oceanography with meteorology in a formal institutional sense, even if the usual bureaucratic barriers remained between divisions. Meanwhile within NASA, where designing satellites to observe the Earth from space gave a push to broader views, some worked deliberately to break down disciplinary boundaries and create an "Earth System Science."[1] Specialists in diverse fields with an interest in climate change found themselves meeting in the various committees and panels that reviewed and directed such programs. The process was officially capped in the mid 1980s by the creation of an "International Geosphere-Biosphere Program," which coordinated work across so many disciplinary boundaries that some began to worry that there were now too many cooks in the kitchen.

The researchers in such programs no longer spoke of studying "climates" in the old sense of regional weather patterns, but of "the climate system" of the whole planet, involving everything from minerals to microbes. This was a fundamentally novel approach. We could call it a new "paradigm," in the word's basic sense of a pattern (like the *amo, amas, amat* of Latin grammar texts) that scientists used to structure their thinking as they attacked their research problems.[2] Many things contributed to the new approach, but nothing so much as the computer studies that began producing plausible climate models during the 1970s. The models spoke eloquently of a global system in their basic concepts, and showed it memorably in their computed maps of weather patterns.[3]

For studying a system with features dispersed among many specialties, the solution was collaboration. This trend was strong in all the sciences, as research problems spanned ever more complexities. Scientists with different types of expertise exchanged ideas and data, or worked directly together for months if not years. Universities and other institutions, braced by ample funding, increasingly encouraged coalitions of research groups in a variety of fields. Specialists in the ionosphere, the Earth's interior, ocean currents, even biology, found themselves sharing the same funding agencies, institutions, and perhaps buildings. Sessions bringing together different specialists on one or another climate topic multiplied at meetings of the interdisciplinary American Geophysical Union and similar organizations. It became increasingly common to hold entire workshops, meetings or conferences devoted to a particular interdisciplinary topic.

Perhaps most important, every scientist read *Science* and *Nature*, which competed with one another for outstanding papers in all fields, including those connected with climate change. Both these weekly journal-magazines also published expert reviews and commentaries, and *Science* published staff-written news articles, keeping everyone up-to-date on selected developments outside their own field. (Of the papers in my bibliography for 1981-2000, *Nature* and *Science* tied with 25% each, including commentary and news articles, followed by the *J. Geophysical*

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[1] Conway (2008).

[2] The term has multiple meanings in the classic Kuhn (1962); for this particular usage, see Weart (1983).

[3] Edwards (2001).

Research with 15% and *Climatic Change* with 7%. *Tellus* fell below 1%. The journal *EOS: Transactions of the American Geophysical Union*, publishing a mixture of short scientific reviews and news articles, came in at 4%. A variety of new review journals titled *Advances in...* and *Reviews of...* collectively contributed another 4%.)

An especially powerful mechanism for cooperation was the formation of projects to address particular interdisciplinary topics. To take one of many examples, specialists in computer modeling got together with paleontologists to test whether the models were robust enough to simulate a climate different from the present one. The Cooperative Holocene Mapping Project (COHMAP) was conceived in the late 1970s at the University of Wisconsin, where Reid Bryson had established a group that reconstructed past climates from fossil pollen and the like. The project expanded through the 1980s, recruiting a variety of domestic and foreign collaborators. Some of them would devote most of their research careers to the project. Typical of such projects, all the collaborators would convene from time to time in large assemblies, but a few leaders would also gather in smaller meetings, "often hosted in home settings where conversations were unhurried and brainstorming was lively." Computer models confronted paleontological data in a continual dialogue, each discrepancy forcing one side or the other, or both, to go back and do better. (Ultimately the modelers produced a good simulation of the climate maps that the paleontologists developed for a warm period 8,000 years ago.) *For other international projects see the essay on International Cooperation*.[1]

Nearly all the papers written before 1940 in my bibliography were published under a single name. Only a few were the work of two authors. But of papers written in the 1980s, less than half had one author. Many of the rest had more than two, and a paper listing, say, seven authors was no longer extraordinary. Large projects were represented by, for example, a 1989 paper with 20 authors from 13 different institutions in seven countries. The trend continued through the 1990s, as single-author papers became increasingly rare.[2]

None of this entirely solved the problem of fragmentation. Into the 21st century there remained entire communities of experts, for example water resource managers, who still treated climate as something that fluctuated only within the unchanging boundaries described by historical statistics. And the more the research enterprise grew, the more scientists would need to specialize. And the imperatives of administration would always maintain boundaries between academic disciplines, and between the government agencies and organizations that supported them. However, by now everyone was keenly aware of the dangers of fragmentation and strove for better coordination. For many kinds of research, climatologists, geochemists, meteorologists, botanists, and so forth added to their disciplinary category a second form of identification—an

[1] Webb (2007), quote p. 107; see also comments in this volume by Kutzbach, p. 224, and Sandy P. Harrison, p. 168.

[2] To be precise, 92 percent were single-authored pre-1940, and 58 percent during 1980-1988. Seven authors: e.g., Hansen et al. (1981); Dansgaard et al. (1982); 20 authors: Cess et al. (1989); multiple authorship in the 1990s is plain from scanning the references in IPCC (2001).

all-embracing name reflecting a new social orientation and holistic approach—"environmental scientist." They were borrowing the luster of a word that had come to stand for a widely admired attitude, with concerns embracing the Earth as a whole.[1]

Meanwhile, some scientists altered even their primary professional identification. By the end of the century the issue of climate change had become important and prestigious enough to stand on its own. Certain scientists who once might have called themselves, say, meteorologists or oceanographers, were now designated "climate scientists."[2] There was still no specific professional organization or other institutional framework to support "climate science" as an independent discipline, but that did not much matter in the new order of holistic interdisciplinary work.

The internet helped bring people closer together. As soon as you heard about a paper in any journal on any subject, you could now find it online with ease, sometimes months before its formal publication. E-mail made it far easier to argue out ideas and exchange data, with as many people listening to the conversation as you liked. A few climate scientists went on to maintain blogs (notably realclimate.org), encouraging a still more universal interchange.

Still, the most important mechanism was the one that had sustained scientific communities for centuries—you went to meetings and talked with people. As one scientist described the system, "Most sucessful scientists develop networks of 'trusted' sources—people you know and get along with, but who are specialists in different areas... and who you can just call up and ask for the bottom line. They can point you directly to the key papers related to your question or give you the unofficial 'buzz' about some new high profile paper."[3]

For climate scientists, the process of meetings and discussion went a long step farther when the world's governments demanded a formal advisory procedure. The resulting Intergovernmental Panel on Climate Change (IPCC) was not really a single panel, but a nexus of uncounted international workshops, exchanges of draft reports, and arguments among individuals, all devoted to producing a single authoritative assessement every half dozen years. From the 1990s on the process engaged every significant climate scientist in the world (and many of the insignificant ones). At the time of its 2007 assessment, the IPCC process had grown to include 157 authors plus some 600 reviewers, giving a rough measure of the size of the scientific community on which the world's policy-makers now depended for crucial advice.[4]

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[1] Water resource managers: Milly et al. (2008). Doel (1997); Earth scientists had already begun to speak of "environmental sciences" as they coordinated Cold War research in the 1950s, Doel (2001).

[2] Richard Lindzen, Testimony before the Senate Environment and Public Works Committee, May 2, 2001, available as appendix to United States Congress (107:1) (2001).

[3] Gavin Schmidt, "AGU Hangover," http://www.realclimate.org/index.php?p=383, posted Dec. 24, 2006.

[4] IPCC (2007b).

In some fields the IPCC process became the central locus for arguments and conclusions.[1] This went farthest among computer modelers, whose efforts increasingly focussed on cooperative projects to produce results for the IPCC assessements. When climate modellers studied the details of each factor that went into their calculations, and when they sought large sets of data to check the validity of their results, they had to interact with every specialty that had anything to say about climate change. Every group felt an intense pressure to come up with answers, as demanded by the world's governments and by their own rising anxieties about the future. In countless grueling exchanges of ideas and data, the experts in each field hammered out agreements on precisely what they could, or could not, say with confidence about each scientific question. Their projections of future climate, and the IPCC reports in general, were thus the output of a great engine of interdisciplinary research. In the world of science this was a social mechanism altogether unprecedented in its size, scope, complexity and efficiency—as well as in its importance for future policy.

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[1] For example, in studying effects on climate of atmospheric chemistry and aerosols, from the mid 1990s the main advances were consolidated in international workshops under IPCC auspices. Le Treut et al. (2007), p. 109.