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## General Circulation Models of Climate

This essay is partly based, by permission, on an essay by Paul Edwards (University of Michigan)<sup>1</sup> rewritten and expanded by Spencer Weart. For additional information and references on this topic, see Edwards’ site at <http://www.aip.org/history/sloan/gcm/>

*The climate system is too complex for the human brain to grasp with simple insight. No scientist managed to devise a page of equations that explained the global atmosphere’s operations. With the coming of digital computers in the 1950s, a small American team set out to model the atmosphere as an array of thousands of numbers. The work spread during the 1960s as computer modelers began to make decent short-range predictions of regional weather. Modeling long-term climate change for the entire planet, however, was held back by lack of computer power, ignorance of key processes such as cloud formation, inability to calculate the crucial ocean circulation, and insufficient data on the world’s actual climate. By the mid 1970s, enough had been done to overcome these deficiencies so that Syukuro Manabe could make a quite convincing calculation. He reported that the Earth’s average temperature should rise a few degrees if the level of carbon dioxide gas in the atmosphere doubled. This was confirmed in the following decade by increasingly realistic models. Skeptics dismissed them all, pointing to dubious technical features and the failure of models to match some kinds of data. By the late 1990s these problems were largely resolved, and most experts found the predictions of overall global warming plausible. Yet modelers could not be sure that the real climate, with features their equations still failed to represent, would not produce some big surprise. (Rudimentary physical models without extensive calculations are covered in a separate essay on Simple Models of Climate, and there is a supplementary essay for the Basic Radiation Calculations that became part of the technical foundation of comprehensive calculations. For a quick tutorial on current climate modeling see Schmidt (2007).)*

"Dr. Richardson said that the atmosphere resembled London for in both there were always far more things going on than anyone could properly attend to."<sup>2</sup>

Climate is made by the general circulation of the atmosphere—the global pattern of air movements, with its semi-tropical trade winds, its air masses rising in the tropics to descend farther north, its cyclonic storms that carry energy and moisture through middle latitudes, and so forth. Many meteorologists suspected that shifts in this pattern were a main cause of climate change. They could only guess about such shifts, for the general circulation was poorly mapped before the 1940s (even the jet streams remained to be discovered). The Second World War and its aftermath brought a phenomenal increase in observations from ground level up to the

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<sup>1</sup> Edwards (2000b).

<sup>2</sup> Simpson (1929b), p. 74.

stratosphere, which finally revealed all the main features. Yet up to the 1960s, the general circulation was still only crudely known, and this knowledge was strictly observational.

From the 19th century forward, many scientists had attempted to explain the general pattern by applying the laws of the physics of gases to a heated, rotating planet. All their ingenious efforts failed to derive a realistic mathematical solution. The best mathematical physicists could only offer simple arguments for the character of the circulation, arguments which might seem plausible but in fact were mere hand-waving.<sup>1</sup> And with the general global circulation not explained, attempts to explain climate change in terms of shifts of the pattern were less science than story-telling.

The solution would come by taking the problem from the other end. Instead of starting with grand equations for the planet as a whole, one might seek to find how the circulation pattern was built up from the local weather at thousands of points. But the physics of local weather was also a formidable problem.

Early in the 20th century a Norwegian meteorologist, Vilhelm Bjerknes, argued that weather forecasts could be calculated from the basic physics of the atmosphere. He developed a set of seven “primitive equations” describing the behavior of heat, air motion, and moisture. The solution of the set of equations would, in principle, describe and predict large-scale atmospheric motions. Bjerknes proposed a “graphical calculus,” based on weather maps, for solving the equations. His methods were used and developed until the 1950s, but the slow speed of the graphical calculation methods sharply limited their success in forecasting. Besides, there were not enough accurate observational data to begin with.<sup>2</sup>

In 1922, the British mathematician and physicist Lewis Fry Richardson published a more complete numerical system for weather prediction. His idea was to divide up a territory into a grid of cells, each with its own set of numbers describing its air pressure, temperature, and the like, as measured at a given hour. He would then solve the equations that told how air behaved (using a method that mathematicians called finite difference solutions of differential equations). He could calculate wind speed and direction, for example, from the difference in pressure between two adjacent cells. These techniques were basically what computer modelers would eventually employ. Richardson used simplified versions of Bjerknes’s “primitive equations,” reducing the necessary arithmetic computations to a level where working out solutions by hand seemed feasible.

The number of computations was so great that Richardson scarcely hoped his idea could lead to practical weather forecasting. Even if someone assembled a “forecast-factory” employing tens of thousands of clerks with mechanical calculators, he doubted they would be able to compute

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<sup>1</sup> For the history of work on the general circulation, see Lorenz (1967), 59ff.

<sup>2</sup> Nebeker (1995); for Bjerknes and scientific meteorology, see also Friedman (1989).

weather faster than it actually happens. But if he could make a model of a typical weather pattern, it could show meteorologists how the weather worked.

So Richardson attempted to compute how the weather over Western Europe had developed during a single eight-hour period, starting with the data for a day when there had been coordinated balloon-launchings measuring the atmosphere at various levels. The effort cost him six weeks of pencil-work (done in 1917 as a relief from his duties as an ambulance-driver on the Western Front). It ended in complete failure. At the center of Richardson's simulacrum of Europe, the computed barometric pressure climbed far above anything ever observed in the real world. "Perhaps some day in the dim future it will be possible to advance the calculations faster than the weather advances," he wrote wistfully. "But that is a dream." Taking the warning to heart, meteorologists gave up any hope of numerical modeling.<sup>1</sup>

## Numerical Weather Prediction (1945-1965)

The alternative to the failed numerical approach was to keep trying to find a solution in terms of mathematical functions—a few pages of equations that an expert might comprehend as easily as a musician reads music. Through the 1950s, some leading meteorologists tried a variety of such approaches, working with simplified forms of the physics equations that described the entire global atmosphere. They managed to get mathematical models that reproduced some features of atmospheric layers, but they were never able to convincingly show the features of the general circulation—not even something as simple and important as the trade winds. The proposed solutions had instabilities. They left out eddies and other features that evidently played crucial roles. In short, the real atmosphere was too complex to pin down in a few hundred lines of mathematics. "There is very little hope," the leading climatologist Bert Bolin declared as early as 1952, "for the possibility of deducing a theory for the general circulation of the atmosphere from the complete hydrodynamic and thermodynamic equations."<sup>2</sup>

That threw people back on Richardson's program of numerical computation. What had been hopeless with pencil and paper might possibly be made to work with the new digital computers. A handful of extraordinary machines, feverishly developed during the Second World War to break enemy codes and to calculate atomic bomb explosions, were leaping ahead in power as the Cold War demanded ever more calculations. In the lead, energetically devising ways to simulate nuclear weapons explosions, was the Princeton mathematician John von Neumann. Von Neumann saw parallels between his explosion simulations and weather prediction (both are problems of non-linear fluid dynamics). In 1946, soon after his pioneering computer ENIAC became operational, he began to advocate using computers for numerical weather prediction.<sup>3</sup>

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<sup>1</sup> Richardson (1922), forecast-factory p. 219, "dream" p. ix; see Nebeker (1995), ch. 6, esp. pp. 81-82; Lynch (2006).

<sup>2</sup> Bolin (1952), p. 107.

<sup>3</sup> Here and below: Aspray (1990); Nebeker (1995), ch. 10.

This was a subject of keen interest to everyone, but particularly to the military services, who well knew how battles could turn on the weather. Von Neumann, as a committed foe of Communism and a key member of the American national security establishment, was also concerned about the prospect of “climatological warfare.” It seemed likely that the U.S. or the Soviet Union could learn to manipulate weather so as to harm their enemies.

Under grants from the Weather Bureau, the Navy, and the Air Force, von Neumann assembled a small group of theoretical meteorologists at Princeton’s Institute for Advanced Study. (Initially the group was at the Army’s Aberdeen Proving Grounds, and later it also got support from the U.S. Atomic Energy Commission.) If regional weather prediction proved feasible, the group planned to move on to the extremely ambitious problem of modeling the entire global atmosphere. Von Neumann invited Jule Charney, an energetic and visionary meteorologist, to head the new Meteorology Group. Charney came from Carl-Gustaf Rossby’s pioneering meteorology department at the University of Chicago, where the study of weather maps and fluids had developed a toolkit of sophisticated mathematical techniques and an intuitive grasp of basic weather processes.

Richardson’s equations were the necessary starting-point, but Charney had to simplify them if he hoped to run large-scale calculations in weeks rather than centuries. Solutions for the atmosphere equations were only too complete. They even included sound waves (the noise of random sound waves, amplified through the computations, was one reason Richardson’s attempt had failed). Charney explained that it would be necessary to “filter out” these unwanted solutions, as one might use an electronic filter to remove noise from a signal, but mathematically.

Charney began with a set of simplified equations that described the flow of air along a narrow band of latitude. By 1949, his group had results that looked fairly realistic—sets of numbers that you could almost mistake for real weather diagrams, if you didn’t look too closely. In one characteristic experiment, they modeled the effects of a large mountain range on the air flow across a continent. Modeling was taking the first steps toward the computer games that would come a generation later, in which the player acts as a god: raise up a mountain range and see what happens! Soon the group proceeded to fully three-dimensional models for a region.<sup>1</sup>

All this was based on a few equations that could be written on one sheet of paper. It would be decades before people began to argue that modelers were creating an entirely new kind of science; to Charney, it was just an extension of normal theoretical analysis. “By reducing the mathematical difficulties involved in carrying a train of physical thought to its logical conclusion,” he wrote, “the machine will give a greater scope to the making and testing of physical hypotheses.” Yet in fact he was not using the computer just as a sort of giant calculator representing equations. With hindsight we can see that computer models conveyed insights in a

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<sup>1</sup> Charney (1949); for a comprehensive discussion, Charney and Eliassen (1949); the first experiment (raising up the Himalayas) in a GCM was Mintz (1965).

way that could not come from physics theory, nor a laboratory setup, nor the data on a weather map, but in an altogether new way.<sup>1</sup>

The big challenge was still what it had been in the traditional style of physics theory: to combine and simplify equations until you got formulas that gave sensible results with a limited amount of computation. To be sure, the new equipment could handle an unprecedented volume of computations. However, the most famous computers of the 1940s and 1950s were dead slow by comparison with a simple laptop computer of later years. Moreover, a team had to spend a good part of its time just fixing the frequent breakdowns. A clever system of computation could be as helpful as a computer that ran five times faster. Developing usable combinations and approximations of meteorological variables took countless hours of work, and a rare combination of mathematical ingenuity and physical insight. And that was only the beginning.

To know when you were getting close to a realistic model, you had to compare your results with the actual atmosphere. To do that you would need an unprecedented number of measurements of temperature, moisture, wind speed, and so forth for a large region—indeed for the whole planet, if you wanted to check a global model. Largely because of military needs, during the war and afterward networks had been established to send up thousands of balloons that radioed back measurements of the upper air. For the first time the atmosphere was seen not as a single layer, as represented by a surface map, but in its full three dimensions. By the 1950s, the weather over continental areas, up to the lower stratosphere, was being mapped well enough for comparison with results from rudimentary models.<sup>2</sup>

The first serious weather simulation Charney's team completed was two-dimensional. They ran it on the ENIAC in 1950. Their model, like Richardson's, divided the atmosphere into a grid of cells; it covered North America with 270 points about 700 km apart. Starting with real weather data for a particular day, the computer solved all the equations for how the air should respond to the differences in conditions between each pair of adjacent cells. Taking the outcome as a new set of weather data, it stepped forward in time (using a step of three hours) and computed all the cells again. The authors remarked that between each run it took them so long to print and sort punched cards that "the calculation time for a 24-hour forecast was about 24 hours, that is, we were just able to keep pace with the weather." The resulting forecasts were far from perfect, but they turned up enough features of what the weather had actually done on the chosen day to justify pushing forward.<sup>3</sup>

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<sup>1</sup> Charney (1949), pp. 371-72; for general discussion of heuristic modeling (including Charney's filtering), see Dalmedico (2001).

<sup>2</sup> An example of important mathematical work is Phillips (1951); for all this history, see Nebeker (1995), pp. 87, 141-51, 183; Smagorinsky (1983); also Smagorinsky (1972); Kutzbach (1996), pp. 362-68.

<sup>3</sup> Charney et al. (1950), quote p. 245; Platzman (1979).

The Weather Bureau and units of the armed forces established a Joint Numerical Weather Prediction Unit, which in May 1955 began issuing real-time forecasts in advance of the weather.<sup>1</sup> They were not the first: since December 1954 a meteorology group at the University of Stockholm had been delivering forecasts to the Royal Swedish Air Force Weather Service, sometimes boasting better accuracy than traditional methods.<sup>2</sup> At their best, these models could give fairly good forecasts up to three days ahead. Yet with the limited computing power available, they had to employ simplifying assumptions, not the full primitive equations of Bjerknes and Richardson. Even with far faster computers, the teams would have been limited by their ignorance about many features of weather, such as how clouds are formed. It would be well over a decade before the accuracy of computer forecasts began to reliably outstrip the subjective guesswork of experienced human forecasters.<sup>3</sup>

These early forecasting models were regional, not global in scale. Calculations for numerical weather prediction were limited to what could be managed in a few hours by the rudimentary digital computers—banks of thousands of glowing vacuum tubes that frequently burned out, connected by spaghetti-tangles of wiring. Real-time weather forecasting was also limited by the fact that a computation had to start off with data that described the actual weather at a given hour at every point in a broad region. That was always far from perfect, for the instruments that measured weather were often far apart and none too reliable. Besides, the weather had already changed by the time you could bring the data together and convert it to a digital form that the computers could chew on. It was not for practical reasons that meteorologists wanted to push on to model the entire general circulation of the global atmosphere. Their interest was theoretical, a hope of understanding at last how the climate system worked—why there are deserts in some places, wet zones in others, and so forth.

That was a fundamentally different type of problem from forecasting. Weather prediction is what physicists and mathematicians call an “initial value” problem, where you start with the particular set of conditions found at one moment and compute how the system evolves, getting less and less accurate results as you push forward in time. Calculating the climate is a “boundary value” problem, where you define a set of unchanging conditions, the physics of air and sunlight and the geography of mountains and oceans, and compute the unchanging *average* of the weather that these conditions determine. (To see how climate might change, modelers would eventually have to combine these two approaches, but that would have to wait until they could compute something resembling the present climate.) That computation, the general circulation model, became a holy grail for theoretical meteorologists.

Norman Phillips in Princeton took up the challenge. He was encouraged by “dishpan” experiments carried out in Chicago, where patterns resembling weather had been modeled in a rotating pan of water that was heated at the edge. For Phillips this proved that “at least the gross

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<sup>1</sup> Nebeker (1995).

<sup>2</sup> Bergthorsson et al. (1955).

<sup>3</sup> For operational forecasting, see also Cressman (1996).

features of the general circulation of the atmosphere can be predicted without having to specify the heating and cooling in great detail.” If such an elementary laboratory system could model a hemisphere of the atmosphere, shouldn’t a computer be able to do as well? To be sure, the computer at Phillips’s disposal was as primitive as the dishpan (its RAM held all of five kilobytes and its magnetic-drum memory held ten). So his model had to be extremely simple. By mid-1955 Phillips had developed improved equations for a two-layer atmosphere. To avoid mathematical complexities, his grid covered not a hemisphere but a cylinder, 17 cells high and 16 in circumference. He drove circulation by putting heat into the lower half, somewhat like the dishpan experimenters only with numbers rather than an electrical coil. The calculations turned out a plausible jet stream and the evolution of a realistic-looking weather disturbance over as long as a month.

This settled an old controversy over what processes built the pattern of circulation. For the first time scientists could see, among other things, how giant eddies spinning through the atmosphere played a key role in moving energy and momentum from place to place. Phillips’s model was quickly hailed as a “classic experiment”—the first true General Circulation Model (GCM).<sup>1</sup>

Von Neumann immediately called a conference to publicize Phillips’s triumph, drumming up government funding for a long-term project. The effort got underway that same year, 1955, under the direction of Joseph Smagorinsky at the U.S. Weather Bureau near Washington, DC. Smagorinsky’s goal was the one first envisaged by von Neumann and Charney, a general circulation model of the entire three-dimensional global atmosphere built directly from the primitive equations.<sup>2</sup> In 1958, he invited Syukuro (“Suki”) Manabe to join the lab. Manabe was one of a group of young men who had studied physics at Tokyo University in the difficult years following the end of the Second World War. These ambitious and independent-minded students had few opportunities for advancement in Japan, and several wound up as meteorologists in the United States. With Smagorinsky and others, Manabe built one of the most vigorous and long-lasting GCM development programs in the world.

Smagorinsky and Manabe put into their model the way rain fell on the surface and evaporated, they put in how radiation passing through the atmosphere was impeded not only by water vapor but also by ozone and carbon dioxide gas (CO<sub>2</sub>), they put in how the air exchanged water and heat with simplified ocean, land, and ice surfaces, and much more. Manabe spent many hours in the library studying such esoteric topics as how various types of soil absorbed water. The huge complexities of the modeling required contributions from several others. “This venture has

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<sup>1</sup> Phillips (1956); on dishpans, see also Norman Phillips, interview by T. Hollingsworth, W. Washington, J. Tribbia and A. Kasahara, Oct. 1989, p. 32, copies at National Center for Atmospheric Research, Boulder, CO, and AIP. See also quote by Phillips in Lewis (2000), p. 104, and see *ibid.* *passim* for a detailed discussion of this work; “Classic”: Smagorinsky (1963), p. 100; already in 1958 Mintz called it a “landmark,” see Arakawa (2000), pp. 7-8.

<sup>2</sup> Smagorinsky (1983).

demonstrated to me,” Smagorinsky wrote, “the value if not the necessity of a diverse, imaginative, and dedicated working group in large research undertakings.”

By 1965 they had a reasonably complete three-dimensional model that solved the basic equations for an atmosphere divided into nine levels. This was still highly simplified, with no geography—land and ocean were blended into a single damp surface, which exchanged moisture with the air but could not take up heat. Nevertheless, the way the model moved water vapor around the planet looked gratifyingly realistic. The printouts showed a stratosphere, a zone of rising air near the equator (creating the doldrums, a windless zone that becalmed sailors), a subtropical band of deserts, and so forth. Many details came out wrong, however.<sup>1</sup>

*From the early 1960s on, modeling work interacted crucially with fields of geophysics such as hydrology (soil moisture and runoff), glaciology (ice sheet formation and flow), meteorological physics (cloud formation and precipitation, exchanges between winds and waves, and so forth). Studies of local small-scale phenomena—often stimulated by the needs of modelers—provided basic parameters for GCM’s. Those developments are not covered in these essays.*

In the late 1950s, as computer power grew and the need for simplifying assumptions diminished, other scientists around the world began to experiment with fully three-dimensional, many-leveled models based on the primitive equations of Bjerknes and Richardson. An outstanding case was the work of Yale Mintz in the Department of Meteorology of the University of California, Los Angeles. Already in the early 1950s Mintz had been trying to use the temperamental new computers to understand the circulation of air—“heroic efforts” (as a student recalled) “during which he orchestrated an army of student helpers and amateur programmers to feed a prodigious amount of data through paper tape to SWAC, the earliest computer on campus.”<sup>2</sup> Phillips’s pioneering 1956 paper convinced Mintz that numerical models would be central to progress in meteorology. He embarked on an ambitious program (far too ambitious for one junior professor, grumbled some of his colleagues). Unlike Smagorinsky’s team, Mintz sometimes had to scramble to get access to enough computer time.<sup>3</sup> But like Smagorinsky, Mintz had the rare vision and drive necessary to commit himself to a research program that must take decades to reach its goals. And like Smagorinsky, Mintz recruited a young Tokyo University graduate, Akio Arakawa, to help design the mathematical schemes for a general circulation model. In the first of a number of significant contributions, Arakawa devised a novel and powerful way to represent the flow of air on a broad scale without requiring an impossibly large number of computations.

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<sup>1</sup> Manabe et al. (1965); it was “the first model bearing a strong resemblance to today’s atmospheric models” according to Mahlman (1998), p. 89; see also Smagorinsky (1963), quote p. 151; Smagorinsky et al. (1965). See also Manabe, interview by P. Edwards, March 14, 1998, AIP.

<sup>2</sup> Arakawa et al. (1994).

<sup>3</sup> Johnson and Arakawa (1996), pp. 3216-18.

*A supplementary essay on Arakawa's Computation Device describes his scheme for computing fluid flow, a good example of how modelers developed important (but sometimes controversial) techniques.*

From 1961 on, Mintz and Arakawa worked away at their problem, constructing a series of increasingly sophisticated GCMs. By 1964 they had produced a climate computed for an entire globe, with only a two-layer atmosphere but including realistic geography—the topography of mountain ranges was there, and a rudimentary treatment of oceans and ice cover. Although the results missed some features of the real world's climate, the basic wind patterns and other features came out more or less right.<sup>1</sup> The model, packed with useful techniques, had a powerful influence on other groups. Arakawa was becoming especially interested in a problem that was emerging as a main barrier to progress—accounting for the effects of cumulus clouds. The smallest single cell in a global model that a computer can handle, even today, is far larger than an individual cloud. Thus the computer calculates none of the cloud's details. Models have had to get by with a “parameterization,” a set of numbers (parameters) that represent the net behavior of all the clouds in a cell under given conditions. Through the decades, Arakawa and others would spend countless hours developing and exchanging ways to attack this critical problem.<sup>2</sup>

Modeling techniques and entire GCMs spread by a variety of means. In the early days, as Phillips recalled, modelers had been like “a secret code society.” The machine-language computer programs were “an esoteric art which would be passed on in an apprentice system.”<sup>3</sup> Over the years, programming languages became more transparent and codes were increasingly well documented. Yet there were so many subtleties that a real grasp still required an apprenticeship on a working model. Commonly, a new modeling group began with some version of another group's model. A post-doctoral student might take a job at another institution, bringing along his old team's computer code. The new team he assembled would start off working with the old code and then set to modifying it. Others built new models from scratch. Through the 1960s and 1970s, important GCM groups appeared at institutions from New York to Australia.

Americans dominated the field during the first postwar decades. That was assured by the government funding that flowed into almost anything related to geophysics, computers, and other subjects likely to help in the Cold War. The premier group was Smagorinsky's Weather Bureau unit (renamed the Geophysical Fluid Dynamics Laboratory in 1963), with Manabe's groundbreaking models. In 1968, the group moved from the Washington, DC area to Princeton, and it eventually came under the wing of the U.S. National Oceanic and Atmospheric

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<sup>1</sup> The oceans were given an infinite heat capacity (fixed temperature), while land and ice had zero capacity. Mintz (1965) (done with Arakawa); this is reprinted in Bates et al. (1993); see Lorenz (1967), p. 133; Arakawa (1970).

<sup>2</sup> Arakawa and Schubert (1974) was a major step, and briefly reviews the history.

<sup>3</sup> Norman Phillips, interview by T. Hollingsworth, W. Washington, J. Tribbia, and A. Kasahara, Oct. 1989, p. 23, copies at National Center for Atmospheric Research, Boulder, CO, and AIP..

Administration. Almost equally influential were the Mintz-Arakawa group at UCLA, and an effort that got underway in 1964 at the National Center for Atmospheric Research (NCAR) in Boulder, Colorado under Warren Washington and yet another Tokyo University graduate, Akira Kasahara. Less visible was a group at RAND Corporation, a defense think-tank in Santa Monica, California. Their studies, based on the Mintz-Arakawa model, were driven by the Department of Defense's concern about possibilities for deliberately changing a region's climate. Although the RAND results were published only in secret "gray" reports, the work produced useful techniques that became known to other modelers.

*For a list and a "family tree" of models, see Paul Edwards's essay<sup>1</sup> and **GCM Web site** <http://www.aip.org/history/sloan/gcm/>*

## Credible Climate Prediction (1965-1979)

Although the modelers of the 1950s and early 1960s got results good enough to encourage them to persevere, they were still a long way from reproducing the details of the Earth's actual circulation patterns and climate zones. In 1965, a blue-ribbon panel of the U.S. National Academy of Sciences reported on where GCMs stood that year. The panel reported that the best models (like Mintz-Arakawa and Smagorinsky-Manabe) calculated simulated atmospheres with gross features "that have some resemblance to observation." There was still much room for improvement in converting equations into systems that a computer could work through within a few weeks. To do much better, the panel concluded, modelers would need computers that were ten or even a hundred times more powerful.<sup>2</sup>

Yet even if the computers had been vastly faster, the simulations would still have been unreliable. For they were running up against that famous limitation of computers, "garbage in, garbage out." Some sources of error were known but hard to drive out, such as getting the right parameters for factors like convection in clouds. To diagnose the failings that kept GCMs from being more realistic, scientists needed an intensified effort to collect and analyze aerological data—the actual profiles of wind, heat, moisture, and so forth, at every level of the atmosphere and all around the globe. The data in hand were still deeply insufficient. Continent-scale weather patterns had been systematically recorded only for the Northern Hemisphere's temperate and arctic regions and only since the 1940s. Through the 1960s, the actual state of the entire general circulation remained unclear. For example, the leisurely vertical movements of air had not been measured at all, so the large-scale circulation could only be inferred from the horizontal winds. As for the atmosphere's crucial water and energy balances, one expert estimated that the commonly used numbers might be off by as much as 50%.<sup>3</sup> Smagorinsky put the problem

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<sup>1</sup> Edwards (2000b).

<sup>2</sup> National Academy of Sciences (1966), vol. 2, pp. 65-67.

<sup>3</sup> Lorenz (1967), pp. 26, 33, 90-91, ch. 5 passim.

succinctly in 1969: “We are now getting to the point where the dispersion of simulation results is comparable to the uncertainty of establishing the actual atmospheric structure.”<sup>1</sup>

In the absence of a good match between atmospheric data and GCM calculations, many researchers continued through the 1960s to experiment with simple models for climate change. A few equations and some hand-waving gave a variety of fairly plausible descriptions for how one or another factor might cause an ice age or global warming. There was no way to tell which of these models was correct, if any. As for the present circulation of the atmosphere, some continued to work on pencil-and-paper mathematical models that would represent the planet’s shell of air with a few fundamental physics equations, seeking an analytic solution that would bypass the innumerable mindless computer operations. They made little headway. In 1967, Edward Lorenz, an MIT professor of meteorology, cautioned that “even the trade winds and the prevailing westerlies at sea level are not completely explained.” Another expert more bluntly described where things stood for an explanation of the general circulation: “none exists.” Lorenz and a few others began to suspect that the problem was not merely difficult, but impossible in principle. Climate was apparently not a well-defined system, but only an average of the ever-changing jumble of daily thunderstorms and storm fronts.<sup>2</sup>

Would computer modelers ever be able to say they had “explained” the general circulation? Many scientists looked askance at the new method of numerical simulation as it crept into more and more fields of research. This was not theory, and it was not observation either; it was off in some odd new country of its own. People were attacking many kinds of scientific problems by taking a set of basic equations, running them through hundreds of thousands of computations, and publishing a result that claimed to reflect reality. Their results, however, were simply stacks of printout with rows of numbers. That was no “explanation” in the traditional sense of a model in words or diagrams or equations, something you could write down on a few pages, something your brain could grasp intuitively as a whole. The numerical approach “yields little insight,” Lorenz complained. “The computed numbers are not only processed like data but they look like data, and a study of them may be no more enlightening than a study of real meteorological observations.”<sup>3</sup>

Yet the computer scientist could “experiment” in a sense, by varying the parameters and features of a numerical model. You couldn’t put a planet on a laboratory bench and vary the sunlight or the way clouds were formed, but wasn’t playing with computer models functionally equivalent? In this fashion you could make a sort of “observation” of almost anything, such as the effect of

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<sup>1</sup> Smagorinsky (1970), p. 33 (speaking at a 1969 conference); similarly, see Smagorinsky (1972), p. 21; “future computer needs will be tempered by the degree to which... we can satisfy the requirements for global data.” National Academy of Sciences (1966), vol. 2, p. 68; Wilson and Matthews (1971), p. 112-13.

<sup>2</sup> Lorenz (1967), quote p. 10; “As for a satisfactory explanation of the general circulation... none exists”: Rumney (1968), p. 63.

<sup>3</sup> Lorenz (1967), p. 8, see pp. 134-35, 145, 151.

doubling the level of greenhouse gases. Through many such trials you might eventually come to understand how the real world operated. Indeed you might be able to observe the planet more clearly in graphs printed out from a model than in the clutter of real-world observations, so woefully inaccurate and incomplete. As one scientist put it, “in many instances large-scale features predicted by these models are beyond our intuition or our capability to measure in the real atmosphere and oceans.”<sup>1</sup>

Sophisticated computer models were gradually displacing the traditional hand-waving models where each scientist championed some particular single “cause” of climate change. Such models had failed to come anywhere near to explaining even the simplest features of the Earth’s climate, let alone predicting how it might change. A new viewpoint was spreading along with digital computing. Climate was not changed by any single cause, the modelers said, but was the outcome of a staggeringly intricate complex of interactions, which could only be comprehended in the working-through of the numbers themselves.

GCMs were not the only way to approach this problem. Scientists were developing a rich variety of computer models, for there were many ways to slice up the total number of arithmetic operations that a computer could run through in whatever time you could afford to pay for. You could divide up the geography into numerous cells, each with numerous layers of atmosphere; you could divide up the time into many small steps, and work out the dynamics of air masses in a refined way; you could make complex calculations of the transfer of radiation through the air; you could construct detailed models for surface effects such as evaporation and snow cover... but you could not do all these at once. Different models intended for different purposes made different trade-offs.

One example was the work of Julian Adem in Mexico City, who sought a practical way to predict climate anomalies a few months ahead. He built a model that had low geographical resolution but incorporated a large number of land and ocean processes. John Green in London pursued a wholly different line of attack, aimed at shorter-term weather prediction. His analysis concentrated on the actions of large eddies in the atmosphere and was confined to idealized mathematical equations. It proved useful to computer modelers who had to devise numerical approximations for the effects of the eddies. Other groups chose to model the atmosphere in one or two dimensions rather than all three.<sup>2</sup> The decisions such people made in choosing an approach involved more than computer time. They also had to allocate another commodity in short supply—the time they could spend thinking. *This essay does not cover the entire range of models, but concentrates on those which contributed most directly to greenhouse effect studies. For models in one or two dimensions, see the article on Basic Radiation Calculations.*

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<sup>1</sup> Kellogg and Schneider (1974), p. 1166.

<sup>2</sup> See the “pyramid” typology of models developed in Shine and Henderson-Sellers (1983); McGuffie and Henderson-Sellers (1997), pp. 44, 55 and passim; Adem (1965); Green (1970).

None of the concepts of the 1960s inspired confidence. The modelers were missing some essential physics, and their computers were too slow to perform the millions of computations needed for a satisfactory solution. But as one scientist explained, where the physics was lacking, computers could do schematic “numerical experiments” directed toward revealing it.<sup>1</sup> By the time modelers got their equations and parameters right, surely not many years off, the computers would have grown faster by another order of magnitude or so and would be able to handle the necessary computations. In 1970, a report on environmental problems by a panel of top experts declared that work on computer models was “indispensable” for the study of climate change.<sup>2</sup>

The growing community of climate modelers was strengthened by the progress of computer systems that carried out detailed calculations on short time-scales for weather prediction. This progress required much work on parameterization—schemes for representing cloud formation, interactions between waves and winds, and so forth. Such studies accelerated as the 1970s began.<sup>3</sup> The weather forecasting models also required data on conditions at every level of the atmosphere at thousands of points around the world. Such observations were now being provided by the balloons and sounding rockets of an international World Weather Watch, founded in the mid 1960s. The volume of data was so great that computers had to be pressed into service to compile the measurements. Computers were also needed to check the measurements for obvious errors (sometimes several percent of the thousands of observations needed to be adjusted). Finally, computers would massage the data with various smoothing and calibration operations to produce a unified set of numbers suitable for calculations. Global data and global models were no longer distinct entities, but parts of a single system for representing the world.<sup>4</sup> The weather predictions became accurate enough—looking as far as three days ahead—to be economically important. That built support for the meteorological measurement networks and computer studies necessary for climate work.

An example of the crossover could be found at NASA’s Goddard Institute for Space Studies in New York City. A group there under James (Jim) Hansen had been developing a weather model as a practical application of its mission to study the atmospheres of planets. For one basic component of this model, Hansen developed a set of equations for the transfer of radiation through the atmosphere, based on work he had originally done for studies of the planet Venus. The same equations could be used for a climate model, by combining them with the elegant method for computing fluid dynamics that Arakawa had developed.

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<sup>1</sup> A.R. Robinson (about ocean modeling) in Reid et al. (1975), p. 356.

<sup>2</sup> “The use of mathematical computer models of the atmosphere is indispensable in achieving a satisfactory understanding...” Matthews et al. (1971), p. 49; a followup study the next year, gathering together the world’s leading climate experts, likewise endorsed research with GCMs. Wilson and Matthews (1971). The section on GCMs was drafted by Manabe.

<sup>3</sup> Nebeker (1995), p. 179.

<sup>4</sup> Edwards (2000a); Nebeker (1995), p. 176.

In the 1970s, Hansen assembled a team to work up schemes for cloud physics and the like to put into a model that would be both fast-running and realistic. An example of the kind of detail they pursued was a simple equation they devised to represent the reflection of sunlight from snow. They included the age of the snow layer (as it gradually melted away) and the “masking” by vegetation (snowy forests are darker than snowy tundra). To do the computations within a reasonable time, they had to use a grid with cells a thousand kilometers square, averaging over all the details of weather. Eventually they managed to get a quite realistic-looking climate. It ran an order of magnitude faster than some rival GCMs, permitting the group to experiment with multiple runs, varying one factor or another to see what changed.<sup>1</sup> In such studies, the global climate was beginning to feel to researchers like a comprehensible physical system, akin to the systems of glassware and chemicals that experimental scientists manipulated on their laboratory benches.

Meanwhile the community of modelers devised more realistic parameters for various physical processes, and sharpened their mathematical techniques. A major innovation that spread during the 1970s took a new approach to the basic architecture of models. Some groups, instead of dividing the planet’s surface into a grid of thousands of square cells, took to dividing it into a tier of segments—hemispheres, quadrants, eighths, sixteenths, etc. (“spherical harmonics”). After doing a calculation on this abstracted system, they could combine and transform the numbers back into a geographical map. This “spectral transform” technique simplified many of the computations, but it was feasible only with the much faster new computers. For decades afterward, physicists who specialized in other fields of fluid dynamics were startled when they saw a climate model that did not divide up the atmosphere into millions of boxes, but used the refined abstraction of spherical harmonics. The method worked only because the Earth’s atmosphere has an unusual property for a fluid system—it is in fact quite nearly spherical.

The new technique was especially prized because it got around the trouble computers had with the Earth’s poles, where all the lines of longitude come together in a point and the mathematics gets weird. (The earliest models had avoided the poles altogether and computed climate on a cylinder, but that wouldn’t take you very far.) Spherical harmonics did not exhaust the ingenuity of climate modelers. For example, in the late 1990s, when people had begun to run separate computations for the atmospheric circulation and the equally important circulation of ocean currents, many groups introduced new coordinate schemes for their ocean models. They avoided problems with the North and South Poles simply by shifting the troublesome points onto a land mass.<sup>2</sup>

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<sup>1</sup> A more fundamental problem of detail was parameters for the absorption and scattering of solar radiation by clouds, aerosol particles, etc. Lacis and Hansen (1974); Hansen et al. (1983); Hansen, interview by Weart, Oct. 2000, AIP, and Hansen et al. (2000a), pp. 128-29.

<sup>2</sup> Edwards (2000b), p. 80 gives as references (which I have not seen) the following; Silberman (1954); Platzman (1960); Robert (1968); Orszag (1970); Bourke (1974); Eliassen et al. (1970). Ocean model scheme: Murray (1996).

Groups continued to proliferate, borrowing ideas from earlier models and devising new techniques of their own. Here as in most fields of science, Europeans had recovered from the war's devastation and were catching up with the Americans. In particular, during the mid-1970s a consortium of nations set up a European Centre for Medium-Range Weather Forecasts and began to contribute to climate modeling.

*For further technical and institutional developments and references, see Paul Edwards' essay<sup>1</sup> and **GCM Web site** <http://www.aip.org/history/sloan/gcm/>*

In their first decade or so of work the GCM modelers had treated climate as a given, a static condition. They had their hands full just trying to understand one year's average weather. Typical was a list that Mintz made in 1965 of possible uses for his and Arakawa's computer model. Mintz showed an interest mainly in answering basic scientific questions. He also listed long-range forecasting and "artificial climate control"—but not greenhouse effect warming or other possible causes of long-term climate change.<sup>2</sup> Around this time, however, a few modelers began to take an interest in global climate change as a problem over the long term. The discovery that the level of CO<sub>2</sub> in the atmosphere was rising fast prompted hard thinking about greenhouse warming, prompting conferences and government panels in which GCM experts like Smagorinsky participated.<sup>3</sup> Computer modelers began to interact with the community of carbon researchers. Another stimulus was Fritz Möller's discovery in 1963 that simple models built out of a few equations—the only models available for long-term climate change—showed grotesque instabilities. Everyone understood that Möller's model was unrealistic (in fact it had fundamental flaws). Nevertheless it raised a nagging possibility that mild perturbations, such as humanity itself might bring about, could trigger an outright global catastrophe.<sup>4</sup>

Manabe took up the challenge. He had a long-standing interest in the effects of CO<sub>2</sub>, not because he was worried about the future climate, but simply because the gas at its current level was a significant factor in the planet's heat balance. But when Möller visited Manabe and explained his bizarre results, Manabe decided to look into how the climate system might change. He and his colleagues were already building a model that took full account of water in all its forms. This was crucial. For it realistically included the feedback between the air's temperature and the amount of moisture the air would hold—and water vapor was the most powerful greenhouse gas. In particular, Manabe's group calculated the way rising columns of moisture-laden air conveyed heat from the surface into the upper atmosphere. To get a really sound answer, the entire atmosphere had to be studied as a tightly interacting system. The required computations were so extensive, however, that Manabe stripped down the model to a single one-dimensional column,

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<sup>1</sup> Edwards (2000b).

<sup>2</sup> Mintz (1965), p. 153.

<sup>3</sup> He recalled that the committee meetings prompted him to ask Manabe to add CO<sub>2</sub> to his radiation model. Smagorinsky, interview by Weart, March 1989, AIP. National Academy of Sciences (1966).

<sup>4</sup> Möller (1963).

which represented the atmosphere averaged over the globe (or in some runs, averaged over a particular band of latitude). His aim was to get a system that could be used as a basic building-block for a full three-dimensional GCM.<sup>1</sup>

In 1967, Manabe's group used the one-dimensional model to test what would happen if the level of CO<sub>2</sub> changed. Their target was something that would eventually become a central preoccupation of modelers: the climate's "sensitivity." Just how much would temperature be altered when something affected incoming and outgoing radiation (a change in the Sun's output of sunlight, say, or a change in CO<sub>2</sub>)? The method was transparent. Run a model with one value of the something (say, of CO<sub>2</sub> concentration), run it again with a new value, and compare the answers. Researchers since Arrhenius had pursued this with highly simplified models. They used as a benchmark the difference if the CO<sub>2</sub> level doubled.<sup>2</sup> That not only made comparisons between results easier, but seemed like a good number to look into. For it seemed likely that the level would in fact double before the end of the 21st century. The answer Manabe's group came up with was that global temperature would rise roughly 2°C (around 3-4°F).<sup>3</sup>

This was the first time a greenhouse warming calculation included enough of the essential factors—especially the correct accounting for water vapor feedback—to seem reasonable to many experts. For instance, Wallace Broecker, who would later play a major role in climate change studies, recalled that it was the 1967 paper "that convinced me that this was a thing to worry about."<sup>4</sup> The work drew on all the experience and insights accumulated in the labor to design GCMs, but it was no more than a first baby step toward a realistic three-dimensional model of the changing climate.

The next important step was taken in the late 1960s by Manabe, now at Princeton, along with a collaborator, Richard Wetherald. Their GCM was still highly simplified. In place of actual land and ocean geography they pictured a geometrically neat planet, half damp surface (land) and half wet (a "swamp" ocean). Worse, they could not predict cloudiness but just held it unchanged at the present level when they calculated the warmer planet with doubled CO<sub>2</sub>. However, they did incorporate the movements of water, predicting changes in soil moisture and snow cover on land, and they calculated sea surface temperatures well enough to show the extent of sea ice. The results, published in 1975, looked quite realistic overall.

The model with increased CO<sub>2</sub> had more moisture in the air, with an intensified hydrological cycle of evaporation and precipitation. That was what physicists might have expected for a warmer atmosphere on elementary physical grounds (if they had thought about it, which few

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<sup>1</sup> Manabe and Strickler (1964). For all this see Manabe, interview by Paul Edwards, March 15, 1998, AIP.

<sup>2</sup> E.g., Arrhenius (1896) (who also calculated for increases by factors of 1.5, 2.5, and 3 as well as lowered levels); Plass (1956b); Möller (1963).

<sup>3</sup> Manabe, interview by P. Edwards, March 14, 1998, AIP. Manabe and Wetherald (1967).

<sup>4</sup> Broecker, interview by Weart, Nov. 1997, AIP.

had). Actually, with so many complex interactions between soil moisture, cloudiness, and so forth, a simple argument could be in error. It took the model computation to show that this accelerated cycle really could happen, as hot soil dried out in one region and more rain came down elsewhere. The Manabe-Wetherald model also showed greater warming in the Arctic than in the tropics. This too could be predicted from simple reasoning. Not only did a more active circulation carry more heat poleward, but less snow and ice meant more absorption of sunlight by ground and sea. Again it took a calculation to show that what sounded reasonable on elementary principles was indeed likely in the real world.<sup>1</sup>

Averaged over the entire planet, for doubled CO<sub>2</sub> the computer predicted a warming of around 3.5°C. It all looked plausible. The results made a considerable impact on scientists, and through them on policy-makers and the public.

Manabe and Wetherald warned that “it is not advisable to take too seriously” the specific numbers they published.<sup>2</sup> They singled out the way the model treated the oceans as a simple wet surface. On our actual planet, the oceans absorb large quantities of heat from the atmosphere, move it around, and release it elsewhere.<sup>3</sup> Another and more subtle problem was that Manabe and Wetherald had not actually computed a climate *change*. Instead they had run their model twice to compute two equilibrium states, one with current conditions and one with doubled CO<sub>2</sub>. In the real world, the atmosphere would pass through a series of changes as the level of the gas rose, and there were hints that the model could end up in different states depending on just what route it took.

Even if those uncertainties could be cleared up, there remained the old vexing problem of clouds. As the planet got warmer the amounts of cloudiness would probably change at each level of the atmosphere in each zone of latitude, but change how? There was no reliable way to figure that out. Worse, it was not enough to have a simple number for cloud cover. Scientists were beginning to realize that clouds could either tend to cool a region (by reflecting sunlight) or warm it (by trapping heat radiation from below, especially at night). The net effect depended on the types of cloud and how high they floated in the atmosphere. A better prediction of climate change would have to wait on general improvements.

Progress was steady, thanks to the headlong advance of electronic computers. From the mid 1950s to the mid 1970s, the power available to modelers increased by a factor of thousands. That meant modelers could put in more factors in more complex ways, they could divide the planet into more segments to get higher resolution of geographical features, and they could run models to represent longer periods of time. The models no longer had gaping holes that required major

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<sup>1</sup> Manabe and Wetherald (1975); preliminary results were reported in Wilson and Matthews (1971). Their planet had “land” surface at high latitudes and was confined to less than one-third of the globe. They computed nine atmospheric levels.

<sup>2</sup> Manabe and Wetherald (1975), p. 13.

<sup>3</sup> Hart and Victor (1993), p. 655.

innovations, and the work settled into a steady improvement of existing techniques. At the foundations, modelers devised increasingly more sophisticated and efficient schemes of computation. As input for the computations they worked endlessly to improve parameterizations describing various processes. From around 1970 on, many journal articles appeared with ideas for dealing with convection, evaporation of moisture, reflection from ice, and so forth.<sup>1</sup>

The most essential element for progress, however, was better data on the real world. Strong efforts were rapidly extending the observing systems. For example, in 1959 the physicist Lewis Kaplan found an ingenious way to use measurements of infrared radiation from satellites to find the temperature at different levels of the atmosphere, all around the world. Ten years later NASA's Nimbus 3 satellite began providing these measurements, a fundamental check on model results. In particular, the reflection of sunlight at each latitude from Manabe's 1975 model planet agreed pretty well with the actual numbers for the Earth, as measured by Nimbus 3 (see above).<sup>2</sup>

Encouragement came from a 1972 model by Mintz and Arakawa (unpublished, like much of their work), which managed to simulate in a rough way the huge changes as the sunlight shifted from season to season. During the next few years, Manabe and collaborators published a model that produced entirely plausible seasonal variations. To modelers, the main point of such work was to gain insight into the dynamics of climate through close inspection of their printouts. (They could study, for example, just what role the ocean surface temperature played in driving the tropical rain belt from one hemisphere to the other as the seasons changed.) To everyone else, seasons were a convincing test of the models' validity. It was almost as if a single model worked for two quite different planets—the planets Summer and Winter. A 1975 review panel felt that with this success, realistic numerical climate models "may be considered to have begun."<sup>3</sup>

Basic problems remained unsolved, such as predicting cloudiness. And new difficulties appeared. For example, scientists began to realize that the way clouds formed, and therefore how much they helped to warm or cool a region, could be strongly affected by the haze of dust and chemical particles floating in the atmosphere. Little was known about how these aerosols helped or hindered the formation of different types of clouds. Another surprise came when two scientists pointed out that the reflectivity of clouds and snow depends on the angle of the sunlight—and in polar regions the Sun always struck at a low angle.<sup>4</sup> Figuring how sunlight might warm an ice cap

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<sup>1</sup> Nebeker (1989), p. 311.

<sup>2</sup> Kaplan (1959); Wark and Hilleary (1969); for atmospheric measurements in general see Conway (in press), chap. 2.

<sup>3</sup> Mintz et al. (1972); as cited by GARP (1975), p. 200; importance of the seasonal cycle to check climate models was noted e.g. in Wilson and Matthews (1971), p. 145; Manabe had a rough seasonal simulation by 1970 and published a full seasonal variation in 1974. Manabe, interview by P. Edwards, March 14, 1998, AIP. Manabe et al. (1974); an example of a later test is Warren and Schneider (1979).

<sup>4</sup> The "neglect of zenith angle dependence" had led to overestimates of ice-albedo feedback in some models. Lian and Cess (1977), p. 1059.

was as complicated as the countless peculiar forms taken by snow and ice themselves. Little of this had been explored through physics theory. Nor had it been measured in the field, for it was only gradually that model-makers realized how much they suffered from the absence of reliable measurements of the parameters they needed to describe the action of dust particles, snow surfaces, and so forth. Overall, as Smagorinsky remarked in 1972, modelers still needed “to meet standards of simulation fidelity considerably beyond our present level.”<sup>1</sup>

Modelers felt driven to do better, for people had begun to demand much more than a crude reproduction of the present climate. Weather disasters and the energy crisis of the early 1970s had put greenhouse warming on the political agenda (at least for the more well-informed people). It was now a matter of public debate whether the computer models were correct in their predictions of how CO<sub>2</sub> emissions would raise global temperatures. Newspapers reported disagreements among prominent scientists. Some experts suspected that factors overlooked in the models might keep the climate system from warming at all, or might even bring on cooling instead. “Meteorologists still hold out global modeling as the best hope for achieving climate prediction,” a senior scientist observed in 1977. “However, optimism has been replaced by a sober realization that the problem is enormously complex.”<sup>2</sup>

The problem was so vexing that the President’s Science Adviser (who happened to be a geophysicist) asked the National Academy of Sciences to study the issue. The Academy appointed a panel, chaired by Jule Charney and including other respected experts who had been distant from the recent climate debates. They convened at Woods Hole in the summer of 1979. Charney’s group compared two independent GCMs, one constructed by Manabe and the other by Hansen—elaborate three-dimensional models that used different physical approaches and different computational methods for many features. The panel found differences in detail but solid agreement for the main point: the world would get warmer as CO<sub>2</sub> levels rose.

But might both GCMs share some fundamental unrecognized flaw? As a basic check, the Charney panel went back to the models of one-dimensional and two-dimensional slices of atmosphere, which various groups were using to explore a wider range of possibilities than the GCMs could handle. These models showed crudely but directly the effects of adding CO<sub>2</sub> to the atmosphere. All the different approaches, simplified in very different ways, were in rough overall agreement. They came up with figures that were at least in the same ballpark for the temperature in an atmosphere with twice as much CO<sub>2</sub> (the level projected for around the middle of the 21st century).<sup>3</sup>

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<sup>1</sup> Smagorinsky (1972), pp. 35-36.

<sup>2</sup> Specifically, “a few scientists can be found who privately suggest that because of complex feedback phenomena the net effect of increased CO<sub>2</sub> might be global cooling.” Abelson (1977).

<sup>3</sup> “Our confidence in our conclusion... is based on the fact that the results of the radiative-convective and heat-balance model studies can be understood in purely physical terms and are verified by the more complex GCM’s. The last... agree reasonably well with the simpler

To make their conclusion more concrete, the Charney panel decided to announce a specific range of numbers. They argued out among themselves a rough-and-ready compromise. Hansen's GCM predicted a 4°C rise for doubled CO<sub>2</sub>, and Manabe's latest figure was around 2°C. Splitting the difference, the panel said they had rather high confidence that as CO<sub>2</sub> reached this level the planet would warm up by about three degrees, plus or minus fifty percent: in other words, 1.5-4.5°C (2.7-8°F). They concluded dryly, "We have tried but have been unable to find any overlooked or underestimated physical effects" that could reduce the warming.<sup>1</sup>

## Ocean Circulation and Real Climates (1969-1988)

In the early 1980s, several groups pressed ahead toward more realistic models. They put in a reasonable facsimile of the Earth's actual geography, and replaced the wet "swamp" surface with an ocean that could exchange heat with the atmosphere. Thanks to increased computer power the models were now able to handle seasonal changes as a matter of course. It was also reassuring when Hansen's group and others got a decent match to the rise-fall-rise curve of global temperatures since the late 19th century, once they put in not only the rise of CO<sub>2</sub> but also changes in emissions of volcanic dust and solar activity.

Adding a solar influence was a stretch, for nobody had figured out any plausible way that the superficial variations seen in numbers of sunspots could affect climate. To arbitrarily adjust the strength of the presumed solar influence in order to match the historical temperature curve was guesswork, dangerously close to fudging. But many scientists suspected there truly was a solar influence, and adding it did improve the match. Sometimes a scientist must "march with both feet in the air," assuming a couple of things at once in order to see whether it all eventually works out.<sup>2</sup> Reassured that they might be on the right track, in the 1980s climate modelers increasingly looked toward the future. When they introduced a doubled CO<sub>2</sub> level into their improved models, they consistently found the same few degrees of warming.<sup>3</sup>

The skeptics were not persuaded. The Charney panel itself had pointed out that much more work was needed before models would be fully realistic. The treatment of clouds remained a central uncertainty. Another great unknown was the influence of the oceans. Back in 1979 the Charney panel had warned that the oceans' enormous capacity for soaking up heat could delay an atmospheric temperature rise for decades. Global warming might not become obvious until all

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models..." National Academy of Sciences (1979), p. 12.

<sup>1</sup> National Academy of Sciences (1979), pp. 2, 3; see Stevens (1999), pp. 148-49; for Manabe's account see also Stokstad (2004). Hansen's model was not published until 1983.

<sup>2</sup> Hansen et al. (1981); for details of the model, see Hansen et al. (1983). I heard "march with both feet in the air" from physicist Jim Faller, my thesis adviser.

<sup>3</sup> Doubling: e.g., Manabe and Stouffer (1980); additional landmarks: Washington and Meehl (1984); Hansen et al. (1984); Wilson and Mitchell (1987). All three used a "slab" ocean 50m or so deep to store heat seasonally, and all got 3-5°C warming for doubled CO<sub>2</sub>.

the surface waters had warmed up, which would be too late to take timely precautions.<sup>1</sup> This time lag was not revealed by the existing GCMs, for these computed only equilibrium states. The models, lacking nearly all the necessary data and thwarted by formidable calculational problems, simply did not account for the true influence of the oceans.

Oceanographers were coming to realize that large amounts of energy were carried through the seas by a myriad of whorls of various types, from tiny convection swirls up to sluggish eddies a thousand kilometers wide. Calculating these whorls, like calculating all the world's individual clouds, was beyond the reach of the fastest computer. Again parameters had to be devised to summarize the main effects, only this time for entities that were far worse observed and understood than clouds. Modelers could only put in average numbers to represent the heat that they knew somehow moved vertically from layer to layer in the seas, and the energy somehow carried from warm latitudes toward the poles. They suspected that the actual behavior of the oceans might work out quite differently from their models. And even with the simplifications, to get anything halfway realistic required a vast number of computations, indeed more than for the atmosphere.

Manabe was keenly aware that if the Earth's future climate were ever to be predicted, it was "essential to construct a realistic model of the joint ocean-atmosphere system."<sup>2</sup> He shouldered the task in collaboration with Kirk Bryan, an oceanographer with meteorological training, who had been brought into the group back in 1961 to build a stand-alone numerical model of an ocean. The two got together to construct a computational system that coupled together their separate models. Manabe's winds and rain would help drive Bryan's ocean currents, while in return Bryan's sea-surface temperatures and evaporation would help drive the circulation of Manabe's atmosphere. At first they tried to divide the work: Manabe would handle matters from the ocean surface upward, while Bryan would take care of what lay below. But they found things just didn't work that way for studying a coupled system. They moved into one another's territory, aided by a friendly personal relationship.

Bryan and Manabe were the first to put together in one package approximate calculations for a wide variety of important features. They not only incorporated both oceans and atmosphere, but added into the bargain feedbacks from changes in sea ice. Moreover, they included a detailed scheme that represented, region by region, how moisture built up in the soil, or evaporated, or ran off in rivers to the sea.

Their big problem was that from a standing start it took several centuries of simulated time for an ocean model to settle into a realistic state. After all, that was how long it would take the surface currents of the real ocean to establish themselves from a random starting-point. The atmosphere, however, readjusts itself in a matter of weeks. After about 50,000 time steps of ten minutes each, Manabe's model atmosphere would approach equilibrium. The team could not conceivably

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<sup>1</sup> National Academy of Sciences (1979), p. 2.

<sup>2</sup> Manabe et al. (1979), p. 394.

afford the computer time to pace the oceans through decades in ten-minute steps. Their costly Univac 1108, a supercomputer by the standards of the time, needed 45 minutes to compute the atmosphere through a single day. Bryan's ocean could use longer time steps, say a hundred minutes, but the simulated currents would not even begin to settle down until millions of these steps had passed.

The key to their success was a neat trick for matching the different timescales. They ran their ocean model with its long time steps through twelve days. They ran the atmosphere model with its short time-steps through three hours. Then they coupled the atmosphere and ocean to exchange heat and moisture. Back to the ocean for another twelve days, and so forth. They left out seasons by using annual average sunlight to drive the system.

Manabe and Bryan were confident enough of their model to undertake a heroic computer run, some 1100 hours long (more than 12 full days of computer time devoted to the atmosphere and 33 to the ocean). In 1969, they published the results in an unusually short paper, as Manabe recalled long afterward—"and still I am very proud of it."<sup>1</sup>

Bryan wrote modestly at the time that "in one sense the... experiment is a failure." For even after a simulated century, the deep ocean circulation had not nearly reached equilibrium. It was not clear what the final climate solution would look like.<sup>2</sup> Yet it was a great success just to carry through a linked ocean-atmosphere computation that was at least starting to settle into equilibrium. The result looked like a real planet—not our Earth, for in place of geography there was only a radically simplified geometrical sketch, but in its way realistic. It was obviously only a first draft with many details wrong, yet there were ocean currents, trade winds, deserts, rain belts, and snow cover, all in roughly the right places. Unlike our actual Earth, so poorly observed, in the simulation one could see every detail of how air, water, and energy moved about.

Following up, in 1975 Manabe and Bryan published results from the first coupled ocean-atmosphere GCM that had a roughly Earth-like geography. Looking at their crude map, one could make out continents like North America and Australia, although not smaller features like Japan or Italy. The supercomputer ran for fifty straight days, simulating movements of air and sea over nearly three centuries. "The climate that emerges," they wrote, "includes some of the basic features of the actual climate." For example, it showed the Sahara and the American Southwest as deserts, but plenty of rain in the Pacific Northwest and Brazil. Manabe and Bryan had not shaped their equations deliberately to bring forth such features. These were "emergent features," emerging spontaneously out of the computations. The computer's output looked roughly like the actual climate only because the modelers had succeeded in roughly representing the actual operations of the atmosphere upon the Earth's geography.

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<sup>1</sup> Manabe, interview by P. Edwards, March 14, 1998. The time steps were explained in a communication to me by Manabe, 2001. The short paper is Manabe and Bryan (1969); details are in Manabe (1969); Bryan (1969a).

<sup>2</sup> Bryan (1969a), p. 822.

“However,” Manabe and Bryan admitted, their model had “many unrealistic features.” For example, it still failed to show the full oceanic circulation. After all, the inputs had not been very realistic—for one thing, the modelers had not put in the seasonal changes of sunlight. Still, the results were getting close enough to reality to encourage them to push ahead.<sup>1</sup> By 1979, they had mobilized enough computer power to run their model through more than a millennium while incorporating seasons.<sup>2</sup>

Meanwhile a team headed by Warren Washington at NCAR in Colorado developed another ocean model, based on Bryan’s, and coupled it to their own quite different GCM. Since they had begun with Bryan’s ocean model it was not surprising that their results resembled Manabe and Bryan’s, but it was still a gratifying confirmation. Again the patterns of air temperature, ocean salinity, and so forth came out roughly correct overall, albeit with noticeable deviations from the real planet, such as tropics that were too cold. As Washington’s team admitted in 1980, the work “must be described as preliminary.”<sup>3</sup> Through the 1980s, these and other teams continued to refine coupled models, occasionally checking how they reacted to increased levels of CO<sub>2</sub>. These were not so much attempts to predict the real climate as experiments to work out methods for doing so.

The results, for all their limitations, said something about the predictions of the atmosphere-only GCMs. As the Charney panel had pointed out, the oceans would delay the appearance of global warming for decades by soaking up heat. Hansen’s group therefore warned in 1985 that a policy of “wait and see” might be wrongheaded. A mild temperature rise in the atmosphere might not become apparent until much worse greenhouse warming was inevitable. Also as expected, complex feedbacks showed up in the ocean circulation, influencing just how the weather would change in a given region. Aside from that, including a somewhat realistic ocean did not turn up anything that would alter the basic prediction of future warming. Once again it was found that simple models had pointed in the right direction.<sup>4</sup>

A few of the calculations showed a disturbing new feature—a possibility that the ocean circulation was fragile. Signs of rapid past changes in circulation had been showing up in ice cores and other evidence that had set oceanographers to speculating. In 1985, Bryan and a collaborator tried out a coupled atmosphere-ocean model with a CO<sub>2</sub> level four times higher than at present. They found signs that the world-spanning “thermohaline” circulation, where differences in heat and salinity drove a vast overturning of sea water in the North Atlantic, could come to a halt. Three years later Manabe and another collaborator produced a simulation in which, even at present CO<sub>2</sub> levels, the ocean-atmosphere system could settle down in one of two

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<sup>1</sup> Manabe et al. (1975); Bryan et al. (1975); all this is reviewed in Manabe (1997).

<sup>2</sup> Manabe et al. (1979).

<sup>3</sup> Washington et al. (1980), quote p. 1887.

<sup>4</sup> Hoffert et al. (1980); Schlesinger et al. (1985); Harvey and Schneider (1985); “yet to be realized warming calls into question a policy of ‘wait and see’,” Hansen et al. (1985); ocean delay also figured in Hansen et al. (1981); see discussion in Hansen et al. (2000a), pp. 139-40.

states—the present one, or a state without the overturning. Some experts worried that global warming might indeed shut down the circulation.<sup>1</sup> Halting the steady flow of warm water into the North Atlantic would bring devastating climate changes in Europe and perhaps beyond.

As oceanographer Wallace Broecker remarked, the GCMs had been *designed* to come to equilibrium, giving an illusion of stability. Only now, as scientists got better at modeling ocean-atmosphere interactions, might they find that the climate system was liable to switch rapidly from one state to another. Acknowledging the criticism, Hansen and a few other modelers were undertaking the protracted computer runs that were necessary to show the real effects of raising CO<sub>2</sub>. Instead of separately computing “before” and “after” states, they computed the entire “transient response,” plodding month after month through a century or more. By 1988, Hansen had enough confidence in his model to issue strong public pronouncements about the imminent threat of global warming.<sup>2</sup>

This was pushing the state of the art to its limit, however. Most model groups could barely handle the huge difficulties of constructing three-dimensional models of both ocean circulation and atmospheric circulation, let alone link the two together and run the combination through a century or so.

The climate changes that different GCMs computed for doubled CO<sub>2</sub>, reviewers noted in 1987, “show many quantitative and even qualitative differences; thus we know that not all of these simulations can be correct, and perhaps all may be wrong.”<sup>3</sup> Skeptics pointed out that GCMs were unable to represent even the present climate successfully from first principles. Anything slightly unrealistic in the initial data or equations could be amplified a little at each step, and after thousands of steps the entire result usually veered off into something impossible. To get around this, the modelers had kept one eye over their shoulder at the real world. They adjusted various parameters (for example, the numbers describing cloud physics), “tuning” the models and running them again and again until the results looked like the real climate. These adjustments were not calculated from physical principles, but were fiddled until the model became stable. It was possible to get a crude climate representation without the tuning, but the best simulations relied on this back-and-forth between model and observation. If models were tuned to match current climate, the critics asked, how reliably could they calculate a future, different state?

One way to check that was to see whether models could make a reasonable facsimile of the Earth during a glacial period—virtually a different planet. If you could reproduce a glacial climate with the same physical parameters for clouds and so forth that you used for the current planet, that would be evidence the models were not arbitrarily trimmed just to reproduce the present. But first you would need to know what the conditions had actually been around the world during an ice age. That required far more data than paleoclimatologists had turned up. Already in 1968 a

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<sup>1</sup> Bryan and Spelman (1985); Manabe and Stouffer (1988).

<sup>2</sup> Broecker (1987a), p. 123; Hansen et al. (1988).

<sup>3</sup> Schlesinger and Mitchell (1987), p. 795.

meteorologist warned that henceforth reconstructing past climate would not be limited by theory so much as by “the difficulty of establishing the history of paleoenvironment.” Until data and models were developed together, he said, atmospheric scientists could only gaze upon the ice ages with “a helpless feeling of wonderment.”<sup>1</sup>

To meet the need, a group of oceanographers persuaded the U.S. government to fund a large-scale project to analyze ooze extracted from the sea bottom at numerous locations. The results, combined with terrestrial data from fossil pollen and other evidence, gave a world map of temperatures at the peak of the last ice age. As soon as this CLIMAP project began publishing its results in 1976, modelers began trying to make a representation for comparison. The first attempts showed only a very rough agreement, although good enough to reproduce essential features such as the important role played by the reflection of sunlight from ice.<sup>2</sup>

At first the modelers simply worked to reproduce the ice age climate over land by using the CLIMAP figures for sea surface temperatures. But when they tried to push on and use models to calculate the sea surface temperatures, they ran into trouble. The CLIMAP team had reported that in the middle of the last ice age, tropical seas had been only slightly cooler than at present, a difference of barely 1°C. That raised doubts about whether the climate was as sensitive to external forces (like greenhouse gases) as the modelers thought. Moreover, while the tropical seas had stayed warm during the last ice age, the air at high elevations had certainly been far colder. That was evident in lower altitudes of former snowlines detected by geologists on the mountains of New Guinea and Hawaii. No matter how much the GCMs were fiddled, they could not be persuaded to show such a large difference of temperature with altitude. A few modelers contended that the tropical sea temperatures must have varied more than CLIMAP said. But they were up against an old and strongly held scientific conviction that the lush equatorial jungles had changed little over millions of years, testifying to a stable climate. (This was an echo of traditional ideas that the entire planet’s climate was fundamentally stable, with ice ages no more than regional perturbations at high latitudes and elevations.)<sup>3</sup>

On the other hand, by 1988 modelers had passed a less severe test. Some 8,000 years ago the world had gone through a warm period—presumably like the climate that the greenhouse effect

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<sup>1</sup> Mitchell (1968), p. iii.

<sup>2</sup> Gates (1976a); Gates (1976b); another attempt (citing the motivation as seeking an understanding of ice ages, not checking model validity): Manabe and Hahn (1977).

<sup>3</sup> The pioneering indicator of variable tropical seas was coral studies by Fairbanks, starting with Fairbanks and Matthews (1978); snowlines: e.g., Webster and Streten (1978); Porter (1979); for more bibliography, see Broecker (1995), pp. 276-77; inability of models to fit: noted e.g., in Hansen et al. (1984), p. 145 who blame it on bad CLIMAP data; see discussion in Rind and Peteet (1985); Manabe did feel that ice age models came close enough overall to give “some additional confidence” that the prediction of future global warming “may not be too far from reality.” Manabe and Broccoli (1985), p. 2650. There were also disagreements about the extent of continental ice sheets and sea ice.

was pushing us toward. One modeling group managed to compute a fairly good reproduction of the temperature, winds, and moisture in that period.<sup>1</sup>

Meanwhile all the main models had been developed to a point where they could reliably reproduce the enormously different climates of summer and winter. That was a main reason why a review panel of experts concluded in 1985 that “theoretical understanding provides a firm basis” for predictions of several degrees of warming in the next century.<sup>2</sup> So why did the models fail to match the relatively mild sea-surface temperatures along with cold mountains reported for the tropics in the previous ice age? Experts could only say that the discrepancies “constitute an enigma.”<sup>3</sup>

A more obvious and annoying problem was the way models failed to tell how global warming would affect a particular region. Policy-makers and the public were less interested in the planet as a whole than in how much warmer their own particular locality would get, and whether to expect wetter or dryer conditions. Already in 1979, the Charney panel’s report had singled out the absence of local climate predictions as a weakness. At that time the modelers who attacked climate change had only tried to make predictions averaged over entire zones of latitude. They might calculate a geographically realistic model through a seasonal cycle, but nobody had the computer power to drive one through centuries. In the mid 1970s, when Manabe and Wetherald had introduced a highly simplified geography that divided the globe into land and ocean segments without mountains, they had found, not surprisingly, that the model climate’s response to a raised CO<sub>2</sub> level was “far from uniform geographically.”<sup>4</sup>

During the 1980s, modelers got enough computer power to introduce much more realistic geography into their climate change calculations. They began to grind out maps in which our planet’s continents could be recognized, showing climate region by region in a world with doubled CO<sub>2</sub>. However, for many important regions the maps printed out by different groups turned out to be incompatible. Where one model predicted more rainfall in the greenhouse future, another might predict less. That was hardly surprising, for a region’s climate depended on particulars like the runoff of water from its type of soil, or the way a forest grew darker as snow melted. Modelers were far from pinning down such details precisely. A simulation of the present climate was considered excellent if its average temperature for a given region was off by only a few degrees and its rainfall was not too high or too low by more than 50% or so. On the positive side, the GCMs mostly did agree fairly well on global average predictions. But the large differences in regional predictions emboldened skeptics who cast doubt on the models’ fundamental validity.<sup>5</sup>

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<sup>1</sup> COHMAP (1988); also quite successful was Kutzbach and Guetter (1984).

<sup>2</sup> MacCracken and Luther (1985b), p. xxiv.

<sup>3</sup> “enigma:” Broecker and Denton (1989), p. 2468.

<sup>4</sup> Manabe and Wetherald (1980), p. 99.

<sup>5</sup> MacCracken and Luther (1985b), see pp. 266-67; Mitchell et al. (1987); Grotch (1988).

A variety of other criticisms were voiced. The most prominent came from Sherwood Idso. In 1986 he calculated that for the known increase of CO<sub>2</sub> since the start of the century, models should predict something like 3°C of warming, which was far more than what had been observed. Idso insisted that something must be badly wrong with the models' sensitivity, that is, their response to changes in conditions.<sup>1</sup> Other scientists gave little heed to the claim. It was only an extension of a long and sometimes bitter controversy in which they had debated Idso's arguments and rejected them as shamefully oversimplified.

Setting Idso's criticisms aside, there undeniably remained points where the models stood on shaky foundations. Researchers who studied clouds, the transfer of radiation through the atmosphere and other physical features warned that more work was needed before the fundamental physics of GCMs would be entirely sound. For some features, no calculation could be trusted until more observations were made. And even when the physics was well understood, it was no simple task to represent it properly in the computations. "The challenges to be overcome through the use of mathematical models are daunting," a modeler remarked, "requiring the efforts of dedicated teams working a decade or more on individual aspects of the climate system."<sup>2</sup> As Manabe regretfully explained, so much physics was involved in every raindrop that it would never be possible to compute absolutely everything. "And even if you have a perfect model which mimics the climate system, you don't know it, and you have no way of proving it."<sup>3</sup>

Indeed philosophers of science explained to anyone who would listen that a computer model, like any other embodiment of a set of scientific hypotheses, could never be "proved" in the absolute sense one could prove a mathematical theorem. What models could do was help people sort through countless ideas and possibilities, offering evidence on which were most plausible. Eventually the models, along with other evidence and other lines of reasoning, might converge on an explanation of climate which—if necessarily imperfect, like all human knowledge—could be highly reliable.<sup>4</sup>

Through the 1980s and beyond, however, different models persisted in coming up with noticeably different numbers for climate in one place or another. These qualitative divergences could be significant for anyone trying to plan for change, at least when one looked closely at particular phenomena such as the projected changes in soil moisture for a given region. Worse, some groups suspected that even apparently correct results were sometimes generated for the wrong reasons. Above all, they recognized that their modeling of cloud formation was still scarcely justified by the little that was known about cloud physics. Even the actual cloudiness of various regions of the world had been measured in only a sketchy fashion. (Until satellite

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<sup>1</sup> Idso (1986); Idso (1987).

<sup>2</sup> E.g., "discouraging... deficiencies" are noted and improvements suggested by Ramanathan et al. (1983), see p. 606; one review of complexities and data deficiencies is Kondratyev (1988), pp. 52-62, see p. 60; "challenges":Mahlman (1998), p. 84.

<sup>3</sup> Manabe, interview by Weart, Dec. 1989.

<sup>4</sup> Oreskes et al. (1994); Norton and Suppe (2001).

measurements became available later in the decade, most models used data from the 1950s that only gave averages by zones of latitude, and only for the Northern Hemisphere. Modelers mirrored the set to represent clouds in the Southern Hemisphere, with the seasons reversed—although of course the distribution of land, sea, and ice is very different in the two halves of the planet.) Having come this far, many modelers felt a need to step back from the global calculations. Reliable progress would require more work on fundamental elements, to improve the sub-models that represented clouds, snow, vegetation, and so forth.<sup>1</sup> Modelers settled into a long grind of piecemeal improvements.

## After 1988

“There has been little change over the last 20 years or so in the approaches of the various modeling groups,” an observer remarked in 1989. He thought this was partly due to a tendency “to fixate on specific aspects of the total problem,” and partly to limited resources. “The modeling groups that are looking at the climate change process,” he noted, “are relatively small in size compared to the large task.”<sup>2</sup> The limitations not only in resources but in computer power, global data, and plain scientific understanding kept the groups far from their goal of precisely reproducing all the features of climate. Yet under any circumstances it would be impossible to compute the current climate perfectly, given the amount of sheer randomness in weather systems. Modelers nevertheless felt they now had a basic grasp of the main forces and variations in the atmosphere. Their interest was shifting from representing the current climate ever more precisely to studies of long-term climate change.

The research front accordingly moved from atmospheric models to coupled ocean-atmosphere models, and from calculating stable systems to representing the “transient response” to changes in conditions. Running models under different conditions, sometimes through simulated centuries, with rising confidence the teams drew rough sketches of how climate could be altered by various influences—and especially by changes in greenhouse gases. They were now reasonably sure that they knew enough to issue clear warnings of future global warming to the world’s governments.<sup>3</sup>

As GCMs incorporated ever more complexities, modelers needed to work ever more closely with one another and with people in outside specialties. Communities of collaboration among experts had been rapidly expanding throughout geophysics and the other sciences, but perhaps nowhere so obviously as in climate modeling. The clearest case centered around NCAR. It lived up to its name of a “National Center” (in fact an international center) by developing what was explicitly a

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<sup>1</sup> Zonally averaged cloud climatology: London (1957). Schlesinger and Mitchell (1987); McGuffie and Henderson-Sellers (1997), p. 55. My thanks to Dr. McGuffie for personal communications.

<sup>2</sup> Dickinson (1989), p. 101-02.

<sup>3</sup> The 1990 Intergovernmental Panel on Climate Change report drew especially on the Goddard Institute model, Hansen et al. (1988).

“Community Climate Model.” The first version used pieces drawn from the work of an Australian group, and the European Centre for Medium-Range Weather Forecasts, and several others. In 1983 NCAR published all its computer source codes along with a “Users’ Guide” so that outside groups could run the model on their own machines. The various outside experiments and modifications in return informed the NCAR group. Subsequent versions of the Community Climate Model, published in 1987, 1992, and so on, incorporated many basic changes and additional features—for example, the Manabe group’s scheme for handling the way rainfall was absorbed, evaporated, or ran off in rivers. The version released in 2004 was called the Third Community Climate *System* Model, CCSM3, reflecting the ever increasing complexity. NCAR had an exceptionally strong institutional commitment to building a model that could be run on a variety of computer platforms, but in other ways their work was not unusual. By now most models used contributions from so many different sources that they were all in a sense “community” models.<sup>1</sup>

The effort was no longer dominated by American groups. At the Hadley Centre for Climate Prediction and Research in the United Kingdom and the Max Planck Institute for Meteorology in Germany, in particular, groups were starting to produce pathbreaking model runs. By the mid 1990s, some modelers in the United States feared they were falling behind. One reason was that the U.S. government forbade them from buying foreign supercomputers, a technology where Japan had seized the lead. National rivalries are normal where groups compete to be first with the best results, but competition did not obstruct the collaborative flow of ideas.

An important example of massive collaboration was a 1989 study involving groups in the United States, Canada, England, France, Germany, China, and Japan. Taking 14 models of varying complexity, the groups fed each the same external forces (using a change in sea surface temperature as a surrogate for climate change), and compared the results. The simulated climates agreed well for clear skies. But “when cloud feedback was included, compatibility vanished.” The models varied by as much as a factor of three in their sensitivity to the external forces, disagreeing in particular on how far a given increase of CO<sub>2</sub> would raise the temperature.<sup>2</sup> A few respected meteorologists concluded that the modelers’ representation of clouds was altogether useless.

Three years later, another comparison of GCMs constructed by groups in eight different nations found that in some respects they all erred in the same direction. Most noticeably, they all got the present tropics a bit too cold. It seemed that “all models suffer from a common deficiency in some aspect of their formulation,” some hidden failure to understand or perhaps even to include some mechanisms.<sup>3</sup> On top of this came evidence that the world’s clouds would probably change

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<sup>1</sup> A brief history is in Kiehl et al. (1996), pp. 1-2, available at <http://www.cgd.ucar.edu/cms/ccm3/history.shtml>; see also Anthes (1986), p. 194. Bader et al. (2005) summarize the interagency politics of the project.

<sup>2</sup> Cess et al. (1989); Cess et al. (1990) (signed by 32 authors).

<sup>3</sup> Boer et al. (1992), quote p. 12,774.

as human activity added dust, chemical haze, and other aerosols to the atmosphere. “From a climate modeling perspective these results are discouraging,” one expert remarked. Up to this point clouds had been treated simply in terms of moisture, and now aerosols were adding “an additional degree of complication.”<sup>1</sup>

Most experts nevertheless felt the GCMs were on the right track. In the multi-model comparisons, all the results were at least in rough overall agreement with reality. A test that compared four of the best GCMs found them all pretty close to the observed temperatures and precipitations for much of the Earth’s land surface.<sup>2</sup> Such studies were helped greatly by a new capability to set their results against a uniform body of world-wide data. Specially designed satellite instruments were at last monitoring incoming and outgoing radiation, cloud cover, and other essential parameters. It was now evident, in particular, where clouds brought warming and where they made for cooling. Overall, it turned out that clouds tended to cool the planet—strongly enough so that small changes in cloudiness would have a serious feedback on climate.<sup>3</sup>

There was also progress in building aerosols into climate models. When Mount Pinatubo erupted in the Philippines in June 1991, sharply increasing the amount of sulfuric acid haze in the stratosphere world-wide, Hansen’s group declared that “this volcano will provide an acid test for global climate models.” Running their model with the new data, they predicted a noticeable cooling for the next couple of years.<sup>4</sup> By 1995 their predictions for different levels of the atmosphere were seen to be on the mark. “The correlations between the predictions and the independent analyses [of temperatures],” a reviewer observed, “are highly significant and very striking.” The ability of modelers to reproduce Pinatubo’s effects was a particularly strong reason for confidence that the GCMs were sound.<sup>5</sup>

Incorporating aerosols into GCMs improved the agreement with observations, helping to answer a major criticism. Typical GCMs had a climate sensitivity that predicted about 3°C warming for a doubling of CO<sub>2</sub>. However, as Idso and others pointed out, the actual rise in temperature over the century had not kept pace with the rise of the gas. An answer came from models that put in the increase of aerosols. The aerosols’ cooling effect, it became clear, had tended to offset the greenhouse warming. By now computer power was so great that modeling groups could go beyond steady-state pictures and explore through time. In 1995, models at three centers (the Lawrence Livermore National Laboratory in California, the Hadley Centre, and the Max Planck Institute) all reproduced the overall trend of 20th-century temperature changes and even the

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<sup>1</sup> Albrecht (1989), p. 1230.

<sup>2</sup> Kalkstein (1991); as cited in Rosenzweig and Hillel (1998).

<sup>3</sup> Purdom and Menzel (1996), pp. 124-25; cloudiness and radiation budget: Ramanathan et al. (1989b); see also Ramanathan et al. (1989a).

<sup>4</sup> Hansen et al. (1992), p. 218. The paper was submitted in Oct. 1991.

<sup>5</sup> Carson (1999), p. 10; ex. of later work: Soden et al. (2002).

observed geographical pattern. Still more convincing, the correspondences between models and data had increased toward the end of the century as the greenhouse gas levels rose.<sup>1</sup>

This GCM work powerfully influenced the Intergovernmental Panel on Climate Change, appointed by the world's governments. In its reports the IPCC noted that the pattern of geographical and vertical distribution of atmospheric heating that the models computed for greenhouse warming was different from the pattern that other influences alone would produce. The similarity between the computed greenhouse effect's "signature" and the actual record of recent decades backed up the panel's official conclusion—a human influence on climate had probably been detected.<sup>2</sup>

Reproducing the present climate, however, did not guarantee that GCMs could reliably predict a quite different climate. The first hurdle was to reproduce the peculiar rise-fall-rise of temperature since the late 19th century. models could do that now, by putting in the history of aerosol and solar variations along with greenhouse gases. There were new data and theories arguing that it was not fudging to put in solar variations, which really could influence climate, and might well have been responsible for the warmth of 1940-1960. The models had also gotten good at the tricky task of mimicking the normal excursions of climate. On some fraction of runs, irregular patterns like the historical rise-fall-rise happened just by chance.

Still, scientists are always happier if they can reproduce an answer using independent methods. This had always been a problem with climate models, with their tendency to interbreed computer code and to rely on similar data sets. One solution to the problem was to cut down to the central question—how much would temperature change if you changed the CO<sub>2</sub> level?—and look for a completely different way to get an answer. The answer could be boiled down to a simple number, the climate's "sensitivity." This was usually expressed as the temperature change for a doubling of CO<sub>2</sub>. A new way to find this number, entirely separate from GCMs, was becoming available from ice core measurement, which recorded large swings of both temperature and CO<sub>2</sub> levels through previous ice ages. A big step forward came in 1992 when two scientists reconstructed not only the Last Glacial Maximum, with its lower temperature and CO<sub>2</sub> levels, but also the mid-Cretaceous Maximum (an era when, according to ample geological evidence, CO<sub>2</sub> levels had been much higher than at present and dinosaurs had baked in unusual warmth). The climate

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<sup>1</sup> Mitchell et al. (1995); similarity increasing with time: Santer et al. (1996).

<sup>2</sup> The 1990 report drew especially on the Goddard Institute model, viz., Hansen et al. (1988); the Hadley model with its correction for aerosols was particularly influential in the 1995 report according to Kerr (1995a); Carson (1999); "The probability is very low that these correspondences could occur by chance as a result of natural internal variability only." IPCC (1996), p. 22, see ch. 8; on problems of detecting regional variations, see Schneider (1994). The "signature" or "fingerprint" method was pioneered by Klaus Hasselmann's group at the Max Planck Institute, e.g., Hasselmann (1993). See also Santer et al. (1996).

sensitivity they found for both cases, roughly two degrees of warming for doubled CO<sub>2</sub>, was comfortably within the range offered by computer modelers.<sup>1</sup>

Confidence rose further in the late 1990s when the modelers' failure to match the CLIMAP data on ice-age temperatures was resolved. The breakthrough came when a team under Lonnie Thompson of the Polar Research Center at Ohio State University struggled onto a high-altitude glacier in the tropical Andes. They managed to drill out a core that recorded atmospheric conditions back into the last ice age. The results, they announced, "challenge the current view of tropical climate history..."<sup>2</sup> It was not the computer models that had been unreliable—it was the oceanographers' complex manipulation of their data as they sought numbers for tropical sea-surface temperatures. A variety of other new types of climate measures agreed that tropical ice age waters had turned significantly colder, by perhaps 3°C or more. That was roughly what the GCMs had calculated.

Debate continued, as some defended the original CLIMAP estimates with other types of data. Moreover, the primitive ice-age GCMs required special adjustments and were not fully comparable with the ocean-coupled simulations of the present climate. But there was no longer a flat contradiction with the modelers, who could now feel more secure in the way their models responded to things like the reflection of sunlight from ice and snow. The discovery that the tropical oceans had felt the most recent ice age put the last nail in the coffin of the traditional view of a planet where some regions, at least, maintained a stable climate.<sup>3</sup>

Another persistent problem was the poor quality of models that included the oceans. Modelers had long understood that reliable climate prediction would require coupling an atmospheric GCM to a full-scale ocean model. Such coupled models were beginning to dominate attention. They all tended to drift over time into unrealistic patterns. In particular, models seemed flatly unable to keep the thermohaline circulation going. The only solution was to tune the models to match real-world conditions by adjusting various parameters. The simplest method, used for instance by Suki Manabe in his influential global warming computations, was to fiddle with the flux of heat at the interface between ocean and atmosphere. As the model began to drift away from reality, it was telling him (as he explained), "Oh, Suki, I need this much heat here." And he would put heat into the ocean or take it away as needed to keep the results stable. Modelers

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<sup>1</sup> Martin and Covey (1992).

<sup>2</sup> Thompson et al. (1995), quote p. 50; see Krajack (2002) and Bowen (2005).

<sup>3</sup> A similar issue was a mismatch between GCMs and geological reconstructions of tropical ocean temperatures during warm periods in the distant past, which was likewise resolved (at least in part) in favor of the models, see Pearson et al. (2001); the sensitivity of tropical climate was adumbrated in 1985 by a Peruvian ice core that showed shifts in the past thousand years, Thompson et al. (1985); new data: especially Mg in forams, Hastings et al. (1998); see Bard (1999); Lee and Slowey (1999); for the debate, Bradley (1999), pp. 223-26; see also discussion in IPCC (2001), pp. 495-96.

would likewise force transfers of water and so forth, formally violating the laws of physics to compensate for their models' deficiencies.<sup>1</sup>

The workers who used this technique argued that it was fair play for finding the effects of greenhouse gases, so long as they imposed the same numbers when they ran their model for different gas levels. Some other modelers roundly criticized flux adjustments as “fudge factors” that could bring whatever results the modelers sought. A few scientists who were skeptical about global warming brought the criticism into public view, arguing that GCMs were too faulty to prove that action must be taken on greenhouse gases. If a model was tuned to match the present climate, why believe it could tell us anything at all about a changed situation? The argument was too technical to attract much public attention. The modelers themselves, reluctant to give ammunition to critics of their enterprise, mostly carried on the debate privately with their colleagues.

Around 1998, different groups published consistent simulations of the ice age climate based on the full armament of coupled ocean-atmosphere models. This was plainly a landmark, showing that the models were not so elaborately adjusted that they could work only for a climate resembling the present one. The work called for a variety of ingenious methods, along with brute force—one group ran its model on a supercomputer for more than a year.<sup>2</sup> Later studies not only reproduced the ice age climate as well as it could be measured, but got an excellent match to another period when the weather patterns had been different from now, the warm “mid-Holocene” about 6000 years ago.

Better still, by 1999 a couple of computer groups simulating the present climate managed to do away altogether with flux adjustments while running their models through centuries. Their results had reasonable seasonal cycles and so forth, not severely different from the results of the earlier flux-adjusted models. Evidently the tuning had not been a fatal cheat. Models without flux adjustments soon became standard.<sup>3</sup>

Another positive note was the plausible representation of middle-scale phenomena such as the El Niño-Southern Oscillation (ENSO). This irregular cycle of wind patterns and water movement in the tropical Pacific Ocean became a target for modelers once it was found to affect weather powerfully around the globe. Such mid-sized models, constructed by groups nearly independent

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<sup>1</sup> Manabe, interview by Paul Edwards, March 15, 1998, AIP; Manabe and Stouffer (1988).

<sup>2</sup> These results helped convince me personally that there was unfortunately little chance that global warming was a mirage. “Landmark”: Rahmstorf (2002), p. 209, with refs.

<sup>3</sup> Carson (1999), pp. 13-17 (for Hadley Centre model of J.M. Gregory and J.F.B. Mitchell); Kerr (1997a) (for NCAR model of W.M. Washington and G.A. Meehl); Shackley et al. (1999). Recent simulations must still start by specifying ice sheets and CO<sub>2</sub> levels as boundary conditions, but give results fully compatible with high-quality land paleovegetation data and the less certain sea temperature data. Jansen et al. (2007).

of the GCM researchers, offered an opportunity to work out and test solutions to tricky problems like the interaction between winds and waves. By the late 1990s, specially designed regional models showed some success in reproducing the structure of El Niños (although predicting them remained as uncertain as predicting any specific weather pattern months in advance). As global ocean-atmosphere models improved, they began to spontaneously generate their own El Niño-like cycles.

Meanwhile other groups confronted the problem of the North Atlantic thermohaline circulation, spurred by evidence from ice and ocean-bed cores of drastic shifts during glacial periods. By the turn of the century modelers had produced convincing simulations of these past changes.<sup>1</sup> Manabe's group looked to see if something like that could happen in the future. Their preliminary work in the 1980s had aimed at steady-state models, which were a necessary first step, but unable by their very nature to see changes in the oceans. Now the group had enough computer power to follow the system as it evolved, plugging in a steady increase of atmospheric CO<sub>2</sub> level. They found that sometime in the next few centuries, global warming could seriously weaken the ocean circulation.<sup>2</sup>

Progress in handling the oceans underpinned striking successes in simulating a wide variety of changes. Modelers had now pretty well reproduced not only simple geographical and seasonal averages from July to December and back, but also the spectrum of random regional and annual fluctuations in the averages—indeed it was now a test of a good model that a series of runs showed a variability similar to the real weather. Modelers had followed the climate through time, matching the 20th-century temperature record. Exploring unusual conditions, modelers had reproduced the effects of a major volcanic eruption, and even the ice ages. All this raised confidence that climate models could not be too far wrong in their disturbing predictions of future transformations. Plugging in a standard 1% per year rise in greenhouse gases and calculating through the next century, an ever larger number of modeling groups with ever more sophisticated models all found a significant temperature rise.<sup>3</sup>

Yet the models were far from proven beyond question. The most noticeable defect was that when it came to representing the present climate, models that coupled atmosphere to oceans were notably inferior to plain atmosphere-only GCMs. That was no wonder, since arbitrary assumptions remained. For example, oceanographers had not solved the mystery of how heat is transported up or down from layer to layer of sea water. The modelers relied on primitive average parameterizations, which new observations cast into doubt.

The deficiencies were not severe enough to prevent several groups from reproducing all the chief features of the atmosphere-ocean interaction. In particular, in 2001 two groups using coupled models matched the rise of temperature that had been detected in the upper layers of the world's

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<sup>1</sup> E.g., Ganopolski and Rahmstorf (2001).

<sup>2</sup> Manabe and Stouffer (1993).

<sup>3</sup> Ice ages without flux adjustments, e.g., Khodri et al. (2001).

oceans. They got a good match only by putting in the rise of greenhouse gases. By 2005, computer modelers had advanced far enough to declare that temperature measurements over the previous four decades gave an unequivocal “signature” of the greenhouse effect. The pattern of warming in different ocean basins neatly matched what models predicted would arise, after some delay, from the solar energy trapped by humanity's emissions into the atmosphere. Nothing else could produce such a warming pattern, not the observed changes in the Sun's radiation, emissions from volcanoes, or any other proposed “natural” mechanism.<sup>1</sup>

Yet if modelers now understood how the climate system *could* change and even how it *had* changed, they were far from saying precisely how it *would* change in future. Only models that incorporated a much more realistic ocean would be able to predict, beyond the general global warming, the evolution of climate region by region over the coming decades.

Looking farther afield, the future climate system could not be determined very accurately until ocean-atmosphere GCMs were linked interactively with models for changes in vegetation. Dark forests and bright deserts not only responded to climate, but influenced it. Since the early 1990s the more advanced numerical models, for weather prediction as well as climate, had incorporated descriptions of such things as the way plants take up water through their roots and evaporate it into the atmosphere. Changes in the chemistry of the atmosphere also had to be incorporated, for these influenced cloud formation and more. All these complex interactions were tough to model.

Meanwhile, vegetation and atmospheric chemistry needed to be linked with still more problematic models—ones that projected future human social and economic life. Over longer time scales, modelers would also need to consider changes in ocean chemistry, ice sheets, ecosystems, and so forth. When people talked now of a “GCM” they no longer meant a “General Circulation Model,” built from the traditional equations for weather. “GCM” now stood for “Global Climate Model” or even “Global Coupled Model,” incorporating many things besides the circulation of the atmosphere. These simulations strained the resources of the newest and biggest supercomputers, some of which were built with climate modelling primarily in mind.

As for modeling the atmosphere itself, experts still had plenty of work to do. The range of modelers' predictions of global warming for a doubling of CO<sub>2</sub> remained broad, anywhere between roughly 1.5 and 4.5°C. The ineradicable uncertainty was caused largely by ignorance of what would happen to clouds as the world warmed. Much was still unknown about how aerosols helped to form clouds, what kinds of clouds would form, and how the various kinds of clouds would interact with radiation. That problem came to the fore in 1995, when a controversy was triggered by studies suggesting that clouds absorbed much more radiation than modelers had thought. Through the preceding decade, modelers had adjusted their calculations to remove

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<sup>1</sup> Levitus et al. (2001); Barnett et al. (2001) (with no flux adjustments); Barnett et al. (2005) with two high-end models and much better data (from Levitus's group), concluding there is “little doubt that there is a human-induced signal” (p. 287). Hansen et al. (2005) found that “Earth is now absorbing 0.85/- 0.15 Watts per square meter more energy from the Sun than it is emitting to space,” an imbalance bound to produce severe effects.

certain anomalies in the data, on the assumption that the data were unreliable. Now careful measurement programs indicated that the anomalies could not be dismissed so easily. As one participant in the controversy warned, “both theory and observation of the absorption of solar radiation in clouds are still fraught with uncertainties.”<sup>1</sup>

As the 21st century began, experts continued to think of new subtleties in the physics of clouds which that significantly affect the models’ predictions. For example, the most respected critic of global warming predictions, Richard Lindzen, started a vigorous debate by speculating that as the oceans warmed, tropical clouds would become more numerous and reflect more sunlight.<sup>2</sup> Struggling with a swarm of such technical uncertainties, experts could not even say whether such cloud feedbacks would tend to hold back global warming, or hasten it. The uncertainties remained the main reason different GCMs gave different predictions for warming in the coming century, ranging from only a degree or two Celsius to half a dozen degrees.<sup>3</sup>

It was also disturbing that modelers had trouble calculating a correct picture of the temperature structure of the atmosphere. While global warming was observed at the Earth’s surface, satellite and balloon measurements showed essentially no warming at middle levels of the atmosphere, which was not at all what the models predicted. “Contrarians” who insisted that global warming was a myth seized on this discrepancy, claiming it proved the public should disbelieve all expert opinion on climate change. The experts buckled down to more rigorous inspection of the data. In 2004 and 2005, new publications described errors in the first reports (for example, they had not properly taken account of how instruments in the balloons heated up when struck by sunlight). The mid-level atmosphere had been warming up after all, and just about the way the models predicted. (Meanwhile the high stratosphere was getting cooler, which was just what modelers had predicted would result from the increase of greenhouse gases.)<sup>4</sup>

Critics kept pointing out every imperfection in the models as publicly as possible. It was not just that there were questionable assumptions in the equations. There were also undeniable problems in the basic physical data that the models relied on, and uncertainties in the way the data were manipulated to fit things together. The models still needed special adjustments to get plausible ice ages. And when modelers tried to simulate the climate of the Cretaceous epoch—a super-greenhouse period a hundred millions years ago that had been far warmer than the present and with a CO<sub>2</sub> level several times higher—the results were far from the climate pattern geologists reported.

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<sup>1</sup> “fraught.” Li et al. (1995); for background and further references on “anomalous absorption,” see Ramanathan and Vogelman (1997); IPCC (2001), pp. 432-33.

<sup>2</sup> Lindzen et al. (2001).

<sup>3</sup> A classic experiment on cloud parameterization was Senior and Mitchell (1993). Somerville et al. (2007), p. 114; IPCC (2001), pp. 427-31; Randall et al. (2007) pp. 636-38.

<sup>4</sup> Sherwood et al. (2005); Mears and Wentz (2005); Karl et al. (2006) (online at <http://www.climate-science.gov/Library/sap/sap1-1/finalreport>).

But for a climate more or less like the present, the factors that modelers failed to understand seemed to have gotten incorporated somehow into the parameters. For the models did produce reasonable weather patterns for such different conditions as day and night, summer and winter, the effects of volcanic eruptions and so forth. At worst, the models were somehow all getting right results for wrong reasons—flaws which would only show up after greenhouse gases pushed the climate beyond any conditions that the models were designed to reproduce. If there were such deep-set flaws, that did not mean, as some critics implied, that there was no need to worry about global warming. As other experts pointed out, if the models were faulty, the future climate changes could be *worse* than they predicted, not better.

Those who still denied there was a serious risk of climate change could not reasonably dismiss computer modeling in general. That would throw away much of the past few decades' work in many fields of science and engineering, and even key business practices. The challenge, then, was to produce a simulation that did *not* show global warming. It should have been easy to try, for by the 1990s, millions of people owned personal computers more powerful than anything commanded by the climate modelers of the 1970s. But no matter how people fiddled with climate models, whether simple one- or two-dimensional models or full-scale GCMs, the answer was the same. If your model could reproduce something resembling the present climate, and then you added some greenhouse gases, the model always displayed a severe risk of future global warming.<sup>1</sup> (*My personal computer is running a real climate model in its idle minutes. You can join the many helping this important experiment: visit [climateprediction.net](http://climateprediction.net).*)

The modelers had reached a point where they could confidently declare what was *reasonably likely* to happen. They did not claim they would ever be able to say what *would certainly* happen. Different model runs continued to offer a range of possible future temperatures, thanks mainly to the different ways they tried to figure how clouds worked. Worse, the various GCMs stubbornly continued to give a wide range of predictions for particular regions. Some things looked quite certain, like higher temperatures in the Arctic (hardly a prediction now, for such warming was becoming blatantly visible in the weather data). But for many of the Earth's populated places, the models could not reliably tell the local governments whether to brace themselves for more droughts, more floods, or neither or both.

By the dawn of the 21st century, climate models had become a crucial source of information for policy-makers and the public. Much of the modelers' attention now focussed on working up results for the reports of the Intergovernmental Panel on Climate Change, rather than formal publication in obscure scientific journals. Struggling to provide a better picture of the changes that would come upon the planet as a whole and in each of its regions, the community of modelers grew larger and better organized. Projects to compare the models devised by different groups became a major ongoing activity. This cooperative framework forced the groups to agree on schemes for representing features of climate and formats for reporting their data, and once that was done, computer outputs could serve as raw material for groups that had nothing to do with

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<sup>1</sup> Varying parameters ([climateprediction.net](http://climateprediction.net)): Stainforth et al. (2005). See remarks in Jones and Mann (2004), p. 28.

the originators. That opened new paths for criticism and experimentation. A joint archive was established, which by 2007 contained more than 30 terabytes of data utilized by more than 1000 scientists.<sup>1</sup>

There were about a dozen major teams now, and the pictures of overall global warming that their ocean-atmosphere GCMs computed were converging. The decades of work by teams of specialists, backed up by immense improvements in computers and data, had gradually built up confidence. It was largely thanks to their work that, as the editor of *Science* magazine announced in 2001, a “consensus as strong as the one that has developed around this topic is rare in the history of science.”<sup>2</sup> Nearly every expert now agreed that the old predictions were solid—raising the CO<sub>2</sub> level was all but certain to warm the globe.

The computer numbers got an independent check. Other scientists were studying past climates clear back to the ice ages, lining up temperature changes with changes in greenhouse gases and other influences such as aerosols blown into the atmosphere from volcanoes. By 2006 they had arrived at fairly good numbers, which agreed well with what the computers calculated. When experts were pinned down to be specific, they might say there was scarcely one chance in twenty that the doubling of CO<sub>2</sub> (what was expected to come before the end of the century) would warm the planet less than 1.5°C. The upper limit was harder to fix, since doubled CO<sub>2</sub> would push the atmosphere into a state not seen for tens of millions of years, but a rise greater than 6°C seemed equally unlikely.<sup>3</sup> The most likely number for climate sensitivity was 3°C.

What do current models predict? See the essay on impacts.

*For current GCMs see:*

Hadley Centre: <http://www.metoffice.gov.uk/research/hadleycentre/>

NASA-GISS: <http://www.giss.nasa.gov/>

NCAR: <http://www.cgd.ucar.edu>

Princeton: [www.gfdl.gov](http://www.gfdl.gov)

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<sup>1</sup> Pioneer Model Intercomparison Projects were Cess et al (1989) (see above) and Gates et al. (1999). See Somerville et al. (2007), p. 118; Randall et al. (2007) p. 594; Lawrence Livermore National Laboratory, “About the WCRP CMIP3 Multi-Model Dataset Archive at PCMDI,” [http://www-pcmdi.llnl.gov/ipcc/about\\_ipcc.php](http://www-pcmdi.llnl.gov/ipcc/about_ipcc.php).

<sup>2</sup> Kennedy (2001). Presumably he meant complex recent topics, not simple scientific facts nor long-accepted theories such as relativity.

<sup>3</sup> Hegerl et al. (2006) and an even lower upper limit according to Annan and Hargreaves (2006), see also Kerr (2006).

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