Rafe Pomerance and Michael Oppenheimer

Chapter 26. 1986: Greenhouse Testimony to Congress

Rafe Pomerance came to my office to discuss our 1981 *Science* paper. The date of this meeting is uncertain, although both Rafe and I remember the meeting vividly.

Pomerance was a gangly six foot four, stooping as if to apologize for his height. He spoke loudly, but in a friendly manner. It was easy to like him. It surprised me when, as we discussed coaching youth, he implied that he was a good athlete – it was hard to imagine him playing soccer (football) – but he was earnest, so I believed him. He was trying to educate himself on climate change. He was an environmentalist, and clearly not a scientist. I assumed that he was a lawyer, but actually he majored in history, which may be good for dealing with politicians.

That first meeting is described colorfully by Nathaniel Rich in <u>Losing Earth, The decade we almost stopped climate change</u>, which formed the entire issue of *New York Times Magazine* on 5 August 2018. Henceforth I term that prodigious article *Losing Earth*. It's true, as Rich wrote, that my office contained at least 30-40 piles of papers, each with a piece of cardboard on top identifying the topic (methane, volcanic aerosols, etc.), but most piles were only several inches tall – only a few piles that included computer output needed to be measured in feet!

Rich is an exceptional writer and he should also be congratulated for digging out a lot of good and useful information on the climate wars in the period 1979-1989. But beware that the editorial wrapper the *Times* created, including the title, prologue and epilogue, creates some big misimpressions (I have not researched the relative responsibilities of editors and the author). Earth was not lost in the 1980s. Even today we can still create a bright future for young people. Also a possible insinuation that the public is to blame for our present situation is wrong, in my opinion. There are plenty of real villains in the story, including we scientists, as I will show.

Rafe and I went our separate ways after that first meeting. I was continuing to work hard, as indicated by the piles of research papers in my office, but not quite as all-consumed as when we lived a few blocks from my office. Living in Ridgewood, New Jersey, I left for work by 6 AM on weekdays to beat the traffic, but I usually made it home before dark, in time to play ball with Erik. I put up a basketball hoop in the driveway and played against Erik and his friend Danny Burns. Somehow, in the end, they always won. I coached biddy basketball, Little League baseball, and girls' softball after Kiki was old enough to play.

Still, Anniek gently reminds me, I left most of the family work for her. On Sundays I would get up while everyone else was still asleep and drive to my office in 25 minutes – nary a car on the road, it seemed – and stay until mid-afternoon. It was quiet in my office then. Such times are needed in science. Science is hard. It is very difficult to do good science if you don't have blocks of quiet time without interruptions.

What could our small research group do? We were in competition with large modeling groups funded by NOAA and NSF. Goddard Space Flight Center was intent on building its own large group in Greenbelt. It did not make sense to mimic their climate modeling approach.

We should do something relevant to human-caused climate change, preferably something that the public and lawmakers could appreciate. Instead of doubled CO_2 experiments, why not run climate simulations in which CO_2 and other gases changed year-by-year as observed? We could check whether the model produced global temperature change similar to observations.

The ocean posed the greatest difficulty. The ocean model needed to include realistic ocean heat capacity – otherwise the climate model would not produce an accurate rate of global warming. However, the ocean does more than provide heat capacity: it transports energy, mainly from low latitudes to high latitudes. This dynamical transport of energy also needs to be included, so that the model can produce a realistic geographical distribution of climate.

We did not have a dynamic ocean model yet. We were way behind Suki Manabe and Kirk Bryan, who started at least 10 years before us. It would take years to build an ocean model. We did not have an oceanographer in our small group and we wanted results quickly.

Gary Russell made the suggestion. Then everyone said it was obvious. The basic idea was to assume that the horizontal transport of energy by the ocean would not change much during the next few decades. In other words, we would assume that dramatic changes such as a complete shutdown of North Atlantic Deep Water were not likely to occur in the 20th century.

We ran the climate model with observed sea surface temperatures and calculated energy fluxes into or out of the ocean surface at each gridbox. From these data and observations over a period of years of the seasonally-varying ocean mixed-layer depth, we could infer ocean heat transports at each ocean gridbox.[1] We then took this inferred ocean heat transport as a fixed quantity and allowed the model to calculate ocean temperature.

Thus we had a climate model, including a simple ocean model with specified ocean transports, that we could use for climate simulations to predict the effect of climate forcings that changed year by year. This was exciting, but we still had a problem: our ancient, slow, computer.

The computer, used by students, post-docs, and GISS staff members, was busy most of the day. However, there were large blocks of time overnight and on the week-end when we could let our global climate model take over the computer.

We needed a model "control run" with 1958 atmospheric condition first, so that we could start experiments when Dave Keeling's measurements began. Then we could run a few experiments with different greenhouse gas scenarios to examine the effect of alternative energy futures.

This gave us a tool for some interesting climate studies. However, our climate model runs would take a long time – almost three years, as it turned out! In the meantime, while waiting for the runs to finish, I wanted to try to address a question being asked by people who were genuinely skeptical about the importance of global warming.

Is warming of a few degrees important? Doubled CO_2 is expected to cause global warming of about 3°C. Three degrees does not sound like much. What's the big deal?

If we convert 3°C to Fahrenheit, it seems a little more impressive: 5.4°F. Also, we know that warming over land will be more than warming over ocean. Global warming of 3°C implies about 7-9°F warming over land. That sounds like it might be important.

Still, critics argue, people move from Minneapolis to St. Louis, or even to Dallas. Such moves can entail a climate change of 7-9°F or more. People adapt to such climate changes.

People, it seemed to me, pay attention to temperature mainly when it is extreme, so I defined a specific problem for a student in our Summer Institute on Planets and Climate: How would doubled CO_2 alter the number of summer days with temperature above 90°F? Above 100°F?

How could we estimate that? The Texas-sized gridboxes of our climate model did not yield results for specific cities and model error at any gridbox was typically a few degrees. The way to deal with these model limitations was to focus on temperature *change*. Climate models can do a good job of computing the response to a climate forcing despite systematic model errors.

The student's task was to use real-world daily temperature records for a given city; count the number of days that a given temperature limit was exceeded during the past 30 years. Then add the model-calculated temperature change for the gridbox in which the city was located, and again count the number of days in which the temperature limit would be exceeded.

Omaha and Washington, DC were the two cities I chose, one near my hometown, the other where lawmakers reside. With doubled CO_2 the number of days per year with temperature above 100°F (38°C) in Omaha increased from three per year in 1951-1980 to 20 per year. In Washington it increased from 0.7 per year (7 days in 10 years) to 12 days per year.

Anniek – from the cool Netherlands – considers 90°F (32°C) to be a heat wave. The number of such days increased from about 35 per year to 85 per year in both Omaha and Washington.

The number of nights in which the temperature did not fall below 80°F (27°C) increased in Omaha from 2 nights in 10 years to 9 nights per year and in Washington DC from four nights in 10 years to 19 nights per year.

These numbers might grab a lawmaker's attention! Three degrees Celsius really is a big deal.

I drafted a short paper on these results after the 1983 Summer Institute on Planets and Climate. However, this was the period when I was editing papers for the Ewing volume, writing our paper for that volume, and struggling to deal with the two friendly elephants who supported us, so the paper lay for two years in a pale green folder under the roller deck on my file cabinet by the window looking out on Broadway. It seemed to be waiting for an appropriate moment.

The second time that Rafe Pomerance came to my office was in July 1985. He was accompanied by Michael Oppenheimer. Rafe and Michael made a powerful team. Pomerance was well-connected in Washington and Oppenheimer was a first-rate scientist.

Oppenheimer, who was then chief scientist for Environmental Defense Fund, played a lead role in finding a solution to the acid rain problem. Specifically, he advocated the cap-and-trade approach, which allowed utilities to collaborate in achieving an inexpensive 50 percent reduction of sulfur emissions from power plants.

Pomerance was in contact with Congressional staffers about the possibility of hearings and climate legislation. Pomerance's follow-up letter to our meeting – and my letter of 4 August in response – remind me of the topics of our discussion. We talked first about climate sensitivity and the delayed climate response based on our 1984 paper in the Ewing volume.

I could say for sure that climate sensitivity was high. It was no longer mainly a model result as it had been in the 1979 Charney study. Paleoclimate data allowed a more certain assessment.

I could also say for sure that there was substantial additional global warming "in the pipeline" even without additional increase of atmospheric greenhouse gases. I gave them a copy of a paper[2] on that topic that had been accepted for publication in *Science*.

Given their quest for intelligible testimony to Congress, I dusted off the folder[3] with the draft "Effect of Doubled CO_2 on Severity of Summers in the United States" from the 1983 Summer Institute and made a photocopy for them. In my 4 August letter to Pomerance and Oppenheimer, I warned them that I would not submit the paper for publication for at least another year.

The rationale I gave was that the 1982 El Chichón volcanic eruption and the tropical Pacific Ocean cool phase that would follow the 1982 El Niño would tend to make the middle 1980s relatively cool, so there was a better chance of a hot summer in a year or two, and thus a paper about hot summers would have much more impact. That rationale proved to be prescient.

I hoped Pomerance and Oppenheimer would not move too fast on a hearing. I wanted to have results from our climate simulations with greenhouse gases changing year by year. The model was plodding along, but we needed at least another year to finish runs for three scenarios.

Climate simulations for Scenario A were well advanced. Scenario A was the high forcing scenario, with continued exponentially increasing amounts of CO_2 and CFCs. Scenario A reached a forcing of 4 W/m^2, equivalent to doubled CO_2, by 2025.

We had also started simulations for scenario B, probably the most realistic scenario. It had near linear growth of climate forcing, achieved by phasing out CFC emissions and reducing growth of fossil fuel use to about half of the rapid growth rate of the first three decades after World War II. Scenario B reached 4 W/m^2 forcing early in the second half of the 21st century.

Scenario C was extreme on the low side, with growth of the climate forcing stopping after year 2000. That would require CO_2 emissions to be reduced by about 50 percent in 2001, with emissions continuing to decline slowly after that.

By the summer of 1986 our climate model run with scenario A had reached 2020 and scenario B had reached early 1980s. Fortunately, Senate hearings would occur in 1986, 1987 and 1988. Simulations were complete for all three scenarios by the time of the November 1987 hearing.

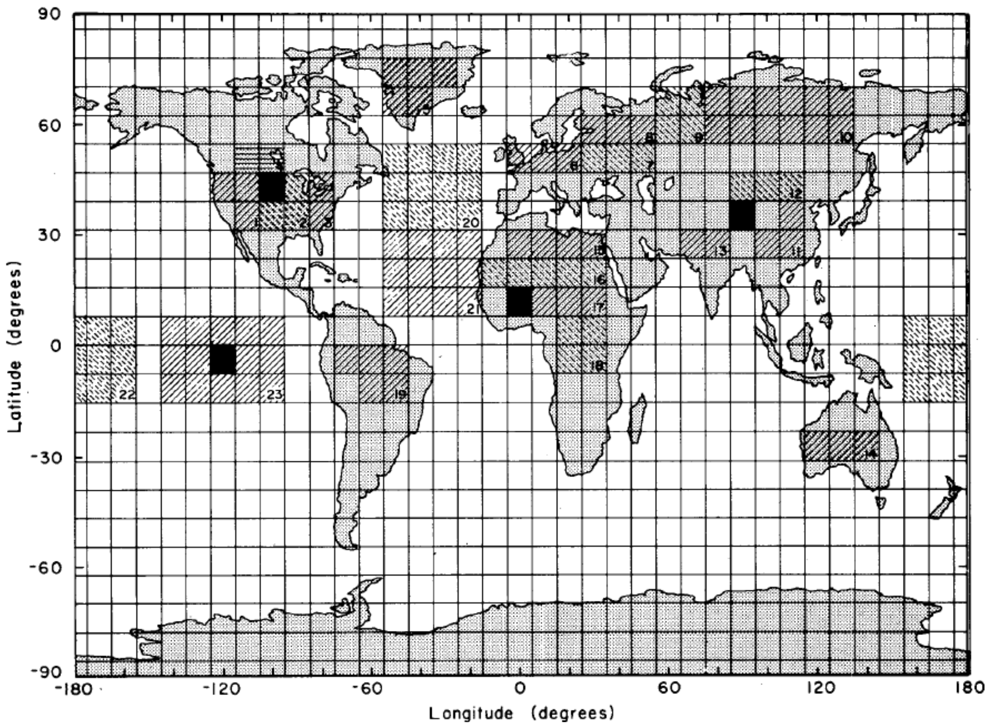

Fig. 26.2. Grid spacing in 8°×10° climate model.

Ozone Depletion, The Greenhouse Effect and Climate Change was the title of the hearing held in the United States Senate on 10-11 June 1986. Mixing of the ozone and climate topics was the idea of Curtis Moore, a Republican staffer, who persuaded Pomerance that lumping the two problems togetherhad the best chance of stirring political interest in climate.

It was a deft move, even though it confused the two issues for the public. The ozone hole – springtime loss of ozone over Antarctica – was discovered in 1985. If ozone loss spread to populated regions, it would deprive the public of protection from harmful UV radiation. The immediacy of the ozone threat offset the slow, futuristic pace of the global warming issue.

Robert Watson and Sherry Rowland were the lead witnesses on ozone. Watson had been a laboratory chemist at the Jet Propulsion Laboratory before moving to NASA Headquarters to run the stratospheric research program. His British accent helped give him a stage presence that was in sharp contrast to most straight-laced NASA scientists.

As the third witness, the first on climate, I responded to a list of seven questions in the invitation letter from Senator John Chafee, Chairman of the Subcommittee on Environmental Pollution of the Committee on Environment and Public Works. This list was obviously a construction of Rafe Pomerance, carefully crafted to educate the audience about how climate research worked, why they should believe that we knew what we were talking about, whether climate change was of practical importance, and how further research should proceed.

The questions were open-ended, about the nature of climate models, how we test them, and the role of paleoclimate. A question about our prediction of the number of days that the temperature in Washington, D.C. would exceed 90°F and 100°F made Rafe's fingerprints crystal clear.

I prepared a 20-page document for my written testimony, including eight figures and a table showing how doubled CO_2 would alter the number of days per year with temperature exceeding 100°F or 90°F in Washington, New York, Chicago, Omaha, Denver, Los Angeles, Dallas and Memphis. My written testimony would be included in the Congressional Record, but to increase its easy availability, I published the document in a government report in August 1986.[4]

My first chart summarized the essence of a global climate model (GCM).[5] The upper half was a global map with our coarse model grid (Fig. 26.2). The lower half listed and described the fundamental equations solved in a GCM – conservation of energy, mass and momentum and the ideal gas law – and sketched a "cartoon" of key physical processes.

Our modeling philosophy differed a bit from most large modeling groups, so it is appropriate to note a few idiosyncrasies. The 8°×10° (latitude×longitude) resolution allowed us to make long simulations, even with a slow computer. The global map is used only to determine the fraction of land within each gridbox.

The rectangular world that we prefer for our maps (Fig. 26.2) makes Antarctica and Greenland appear larger than they really are, but we view that as a merit. It is the optimum projection for locating a specific latitude and longitude. Rather than wasting white space on the page, as many projections do, our rectangular map instead provides higher resolution of important polar regions. I keep a world globe by my desk. I believe that a globe plus quantitative latitude-longitude maps make the best combination for thinking about global problems.

Shaded regions on the map are a particular choice of places where data was saved at a high frequency for study of physical processes, for example, for comparison with field studies. In those days, data storage space was limited, so we could not store data for every gridbox.

The main results that I showed were graphs and maps of warming calculated by our GCM for scenario A and the partially completed scenario B. I concluded that global warming should rise above the noise level of natural variability within the next several years, and by early in the 21st century Earth would be warmer than at any time in the past 100,000 years. I ended with an appeal for support of global observations as global change was underway, noting that "prestigious groups such as the Earth Systems Sciences Committee," chaired by Francis Bretherton of the National Center for Atmospheric Research, had defined required observations in detail.

The 1986 hearing was educational for me. Oral communication was not my forte and I did not enjoy it. I learned that some of my charts had too much detail and that answers to questions needed to be brief.

Senator Chafee asked a simple question "Do any of you believe that we need more scientific data before we could reach the conclusion that what is taking place now, if continued, will increase the temperature on the globe?" The obvious answer was "no," but my answer and Sherry Rowland's were too long-winded. Bob Watson said simply: "No. I believe global warming is inevitable. It is only a question of the magnitude and the timing."

The hearing was a success. It got the public attention Pomerance and Oppenheimer sought. "Global warming is inevitable. It is only a question of the magnitude and the timing" was in all of the papers. I grumbled to my sister Lois – whose house I stayed at while in Washington –

“that statement says nothing, magnitude and timing are everything, if the magnitude is negligible, there is no effect.” Yet it was a good, effective statement.

Scientists must aim to be clear in communicating with the public, but it is not easy. Einstein said that a theory or explanation should be as simple as possible, but not simpler. The climate story is inherently complex. Realistic communication with the public is a challenge.

I had received inklings that NASA preferred that I stick to research and leave communication with Congress to NASA Headquarters, so I had to think about that, but it did not take very long. I was certain that I understood climate change a lot better than Headquarters did. And I preferred to speak for myself.

I would try again the next year, but for the moment I had other things on my mind: baseball.

Fathers coach little league in Ridgewood. There was one good coach in town, Lew Dickinson. He pitched in college and knew baseball. He loved kids, his job was working with them at the **Y**. His wife was a school teacher. He always said the right thing to encourage kids. He did not favor his son on his team. In other words, he was impossible to emulate successfully.

Erik was 11 in 1985 when I inherited the Renato’s Pizza team from a retiring father. We got second place to Lew’s team. Being ultra-competitive, I scouted the 10-year old league. One kid, Gene Ret as pitcher, struck out 15 batters in a five-inning game. In the spring 1986 draft I picked next to last, but nobody else had scouted: Gene Ret was still available. With Gene, Erik and Dave Huffman we had outstanding pitching. We did not lose a single game in 1986, an unheard-of record. As East Ridgewood champions, we played in the traditional crosstown game at the end of the school year, easily defeating the West Ridgewood champions.

Our players enjoyed their swagger as the “green machine,” named for their green Renato shirts. Yes, I know, it might have been better to have a competitive league, but I am telling you the way it was, not the way it should have been. I warned you that Lew was the only good coach.

I was asked to coach the East Ridgewood team for the Little League World Series. Kids have so many activities now that they don’t play enough baseball – they need practice. I put up a batting cage in our backyard. I bought a six-foot-by-six-foot piece of chain-link fence, put weights on it, and dragged it behind my Dasher Diesel to smooth out the lumpy field at Hawes School.

I hit infield and outfield practice from home plate with Kiki’s softball glove on my left hand and a bat in the right. After hitting the ball toward a fielder I caught the return throw in the glove, flipped the ball in the air, and hit it to another fielder. I could easily hit the ball over the fence with one hand. Other fathers somehow got the idea that I had been a semipro player. Ha, ha. I didn’t tell them that the last time I played, at age 15, I couldn’t get the ball out of the infield.

We got a bye in the first round, so I drove to the town where West Ridgewood was playing. We would play the winner of that game. Both teams made several errors. West Ridgewood blew the game, but it seemed clear that our team was better than the winning team.

Danged Robert Burns again: schemes gang aft agley. Erik woke the next morning with 105-degree fever. Anniek stayed with him. The other team had saved their ace, assuming they could beat West Ridgewood. Gene pitched well, but we lost. As losers, we had to play again the next day. Erik dragged himself out of bed, too weak to pitch, but he played second base. Dave pitched well and Gene hit a home run, but we lost. Season over.

Oh, well. I could focus on science. I wanted to be better prepared for the next testimony to Congress, expected to be sometime in 1987. I hoped that by then our climate simulations would be complete for all three scenarios of future greenhouse gases.

Also I wanted to complete a paper documenting our method of extracting global temperature change from weather station measurements. The paper should be peer-reviewed and include an error estimate, so that we could use the temperature analysis to check whether global warming was beginning to rise above the level of natural variability.

I thought that I would be prepared to communicate clearly. What I did not figure on was the growth of a number of phenomena behind the scenes. Somebody, clearly, did not want us to communicate. But that story would be almost as complicated as the science.

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[1] Russell, G.L., J.R. Miller and L.C. Tsang, Seasonal oceanic heat transports computed from an atmospheric model, Dynam. Atmos. Oceans, 9, 253-271, 1985.
[2] Hansen, J., G. Russell, A. Lacis, I. Fung, D. Rind and P. Stone, Climate response times: dependence on climate sensitivity and ocean mixing, *Science*, **229**, 857-859, 1985.
[3] I still have the folder; apologies to the student co-author, Paul Ashcraft, who was an undergraduate at Texas A&M University in 1983, for my failure to shepherd the paper to publication.
[4] Hansen, J., A. Lacis, D. Rind, G. Russell, I. Fung, P. Ashcraft, S. Lebedeff, R. Ruedy and P. Stone, The greenhouse effect: projections of global climate change, pp. 199-218, in Titus, J.G., *Effects of Changes in Stratospheric Ozone and Global Climate*, EPA/UNEP Report, Washington, D.C., 379 pp., August 1986.
[5] I use GCM to refer to three-dimensional global climate models, i.e., including atmospheric dynamics.