Chapter 19. Coming Down to Earth

Walter Sullivan's article[1] on our *Science* paper was published on the front page of the New York Times while we were in Europe, with the headline "Study Finds Warming Trend That Could Raise Sea Levels." It was a good summary including a nice graphic of global temperature change in the prior 100 years, showing that temperature had not yet regained the peak value obtained in 1940, but overall there was a temperature rise of 0.4°C in 100 years.

Sullivan's final paragraph read: "As 'an appropriate strategy' the report proposes emphasis on energy conservation and development of alternative energy sources while using fossil fuels 'as necessary' in the coming decades."

Sullivan queried Steve Schneider about the paper in a phone interview, summarizing Steve's view as "conclusions about the extent of warming and how quickly it will occur would be reasonable if the assumptions on which they are based prove valid, but that many can be challenged." Sullivan discussed several of the assumptions.

Notably, Sullivan then wrote: "These uncertainties are, to a large extent, recognized in the new report, signed by Dr. James Hansen and six colleagues at the space studies institute." It was satisfying to realize that Sullivan had read the paper carefully.

A New York Times lead editorial, a few days later, titled 'Heating Up the Atmosphere," concluded: "The greenhouse effect is still too uncertain to warrant total alteration of energy policy. But this latest study offers fair warning; that such a change may yet be required is no longer unimaginable."

A week later, on 5 September, there was a longer lead editorial on our paper in the Washington Post. The second paragraph read: "An important new contribution, by a team of NASA atmospheric physicists, now concludes that the carbon dioxide *is* causing a warming, one large enough to have very unfavorable consequences in the next century."

The final paragraph of the Post editorial was: "There are still many hedges to these findings, and much research that must be done before they are confirmed. The authors predict, however, that the warming will be clearly measurable – over and above normal climate variations – by the 1990s, and perhaps even in this decade. Since the amount of warming depends most strongly on energy growth and the type of fuels used, and since it takes decades and billions of dollars to make major shifts in energy use, it is not too soon to begin thinking seriously about how carbon dioxide should affect U.S. energy choices now."

Articles appeared in newspapers and magazines over the next few weeks. The fact that these were scattered over such a long period makes me think that there was never a NASA press release. It may have all been generated by the article of the widely respected Walter Sullivan, which article is dated at least a few days earlier than the other articles and editorials.

I had no time to spend wondering about such things.

A. Thomas Young was the Director of Goddard Space Flight Center. Presumably, he was the person who selected me to be the second director of the Goddard Institute for Space Studies.

I was in awe of Tom Young, if not afraid of him. He was the most respected engineer in NASA, which itself was still an engineering organization of exceptional repute. Young was the Mission Director of Project Viking, which successfully landed two spacecraft on the surface of Mars, and did it on schedule in July and August 1976 to celebrate the 200th birthday of the United States.

Young ran tight, professional staff meetings as Goddard Director. Dr. Jastrow was expected to attend certain regular meetings – once per month, I believe. In the final year or two of Jastrow's tenure, I sometimes attended in Jastrow's stead. Our presentation was brief: a few science highlights and then personnel and budget to show that our expenditures did not exceed our income. It was anomalous for such a small institute to be reporting at the highest level of a multi-billion-dollar organization, but GISS had high visibility, recent budget problems, and Jastrow had successfully extricated himself from reporting lower down.

A project manager at one of the staff meetings described a problem they were having in meeting schedule and budget. He concluded that they would "work hard and smart" to address the problem. Shortly thereafter, Tom Young said he wanted to return to "work hard and smart." He repeated the phrase several times in the next minute or two, berating it as the absence of a plan. With each repetition the project manager grew whiter and whiter.

I was nervous when Director Young visited GISS, but when we went to lunch at the Moon Palace, just the two of us, he was genial. He had a message for me. He said that he could never have succeeded with Viking if he obeyed the rules of the bureaucracy. You cannot break the law, do anything illegal, but you must focus on your objectives, not on bureaucratic rules.

I felt some justification for the blind carbon copies sent to Walter Sullivan and Rafe Pomerance.

A big decision was required. GISS would not be allowed to continue to report straight to the Goddard Director's office. I would have to choose: Space Sciences or Earth Sciences.

Space Sciences would mean reporting to George Pieper, the "biggest horse's ass in the agency," according to Jastrow.[2] I had also developed a disdain for Pieper, because of his petulant attack on our Galileo Jupiter experiment.

Earth Sciences would mean reporting to Les Meredith, the aw-shucks Iowan, a Van Allen protégé. Meredith and I got along well, perhaps because I had done an enormous amount of work for him, writing a large fraction of the "Green Book" climate plan.

Dr. Thaddeus preferred Space Sciences, which was where his astronomical observations were relevant. However, he agreed that Les Meredith was a good guy, whom he could live with.

I made many mistakes, both personal and institutional, while I was director of GISS, but the biggest boner of all was my choice of Earth Sciences and Meredith. This became apparent in less than a year, when Meredith ordered me to move GISS to Greenbelt.

Long-term consequences of my choice are speculative – one never knows what "the road not taken" would have brought. It is conceivable, if unlikely, that we might have been able to affect the nature of NASA's "Mission to Planet Earth," which was initiated within a decade. If Space Sciences had a role in that mission, the mission just might have been defined more by science and less by engineers. What I can say with more confidence – I would bet $100 to a donut – is that, if I had chosen Space Sciences, today we would have small satellites monitoring global aerosol and cloud properties. However, that's a story for my next book.

Dr. Jastrow showed no signs of vacating his office. I went to see him, and said that I would be pleased if he kept an office at the Institute, but I wanted to move into the Director's office. If my memory is right, I said that he would be welcome to take my office, which was a corner office, as good as any office in the building, except the Director's suite.

Dr. Jastrow was angry. "Have Carl[3] put wood paneling on your office!" He asserted that he had an arrangement with Hans Mark, who he knew well "from the old country," whatever that meant. The arrangement, Jastrow said, was that he could keep his office.

Hans Mark was Deputy Administrator of NASA. Jim Beggs was a new political appointee as Administrator. Hans Mark was a long-time NASA professional. On a day-to-day basis Hans Mark was running NASA.

I suppose that I should have gone through the Goddard chain of command, but I called Hans Mark directly and explained the situation. He said that he would let Dr. Jastrow know that he needed to move to a different office. I responded that it had become clear to me that Jastrow needed to vacate the building. Given my shy low-key personality and Jastrow's demanding nature, it would not work to have him in the building. Jastrow would demand support and resources, which I had no money to cover. Mark agreed. Jastrow was soon gone.

Dr. Jastrow was treated rudely in another way. Columbia science professors were distraught that many students used Jastrow's *Stars, Planets and Life* to satisfy their science requirement, instead of their more hard-core science courses. They persuaded Columbia, and Barnard, Columbia's sister college, to drop Jastrow's class as a course satisfying the science requirement.[4]

Dr. Jastrow was teaching his course in the fall of 1982 when informed that his compensation had been cut in half. He announced his resignation in front of a classroom full of shocked students and walked away.[5] Jastrow continued to teach at Dartmouth and founded the Marshall Institute in Washington, DC, where he supported "Star Wars," the Reagan Administration's plan to protect the U.S. from attack by nuclear missiles, and became a climate change denier.

Upon his 80th birthday I wrote *A Little Story*[6] about Dr. Jastrow, available on my website.

Reactions of the scientific community to our paper were not a big surprise. I sent the paper prior to publication to 30 top relevant scientists asking for criticisms, getting responses such as:

From the venerable Hubert Lamb[7] (Univ. East Anglia): "It is very informative and very valuable. But I do have one critical comment. In my view there is a dangerous fallacy in the argument that the anthropogenic warming effect will become apparent and go beyond the "noise" level of the natural climatic fluctuations at any specifiable time. The trouble is that the range of natural climatic fluctuations is not constant…"

H.E. Landsberg (Univ. Maryland): "My principal reaction is that you are moving things too fast. We are still quite uncertain about the real global or even northern hemisphere temperatures and the noise level is certainly very high. Also the role of the ocean is far from cleared … events may be very much slowed down before any CO_2 warming can be uniquely identified. Another point must also be made with respect to the level of fossil fuel use. How certain are the extrapolations … will we be saved at the last moment by fusion energy? Finally my faith in climate models of the current crop is still low. While we must pursue this approach vigorously,

as you have done, the intricacies of nature are not readily incorporated into relatively simple physical and mathematical formulations. Nonetheless, your work is a very fine contribution to the debate and I hope that it will soon be published."

Norbert Untersteiner (Director, Ocean Programs, NOAA): "It seems to be an interesting paper, but since you are asking for criticisms I will mention some. Almost all papers on CO_2 and climate change seem to suggest that a global warming would be undesirable. Who knows? As you point out, there may be a wide range of different regional effects … under these circumstances … does anyone seriously believe that mankind, as it presently operates, will ever be capable of adjusting its use of energy in unison and according to principles that are not dictated by profit? The only other comment I have concerns the ice sheets and their effect on sea level …we know far too little about the present dynamics of the ice sheet, to even predict the sign of a global warming effect."

These reactions were representative of the reaction of other leading scientists. Scientists by their nature are skeptics. So their reactions were appropriate and not surprising, especially considering that global temperature had not yet reached the level of 1940.

So why did I think that we could make predictions with a reasonably high level of confidence?

Because the predictions were based on the physics of greenhouse gases, which was understood from theory and laboratory data, and on the overall physics of the climate system, which was understood reasonably well. Planets provided a good check. So did paleoclimate change. And our new result, the observed warming over the prior century, agreed well with theory.

So why did we emphasize multiple testable predictions, in the abstract and paper?

That is a standard way to do science, but there was an additional reason: the delayed response of the climate system. That delayed response makes a policy of "wait and see" dangerous, because consequences may become out of humanity's control. The skepticism of scientists adds to the delayed response problem, because scientists are never fully satisfied – skepticism will remain.

If testable predictions worked out within a decade or two, that may aid arguments for needed energy policies. It may be possible to get action a little sooner than otherwise would be the case.

The best laid schemes o' mice an' men Gang aft agley (Robert Burns, in *To a Mouse*). Our climate research plans began to go awry in 1981 even before our *Science* paper was published, but it was not until late in 1981 that I was forced to admit our difficult situation.

NASA Headquarters informed me in early 1981 that with the change of Administration in the U.S., from Jimmy Carter to Ronald Reagan, NASA's climate research funding was being reduced. Henceforth, the Department of Energy (DoE) would support CO_2 climate research.

Fortunately, Bob Schiffer, the manager of the NASA climate research program, would continue to support the more general GISS climate modeling project under my direction at its existing level of $500K per year, which was the largest grant in Schiffer's program. He also would continue to support the cloud research project at GISS, which Bill Rossow was managing.

However, NASA could not continue to provide our CO_2 climate research funding, which was then $230K/year. NASA's expectation, or at least hope, was that our CO_2 research funding

would be picked up by the Department of Energy (DoE). DoE confirmed to me in a phone call that they planned to fund our CO_2 research.

I had to submit a proposal. But before that proposal was evaluated, there was a change of CO_2 program leadership at DoE, with the new (Reagan) Administration. In June I received a request to provide a supplemental briefing on the research at DoE's Germantown facility on 14 July.

I took Inez Fung and David Rind along, to be sure that we could answer any questions well. The presentation was to Frederick Koomanoff, who had recently taken over as Director, Carbon Dioxide Research Division, Office of Basic Energy Sciences, Department of Energy.

The briefing went well, I thought, but, as we drove back to the airport in our rented car Inez asked "Did that go well? I have no experience in such meetings." Neither did I, but I replied, at least approximately, "sure, we described the science well and could answer all their questions."

"But they didn't say anything about funding," Inez continued. I started to worry a bit. I was not Robert Jastrow. He would have known how to ask about the funding, without putting them off.

So when I got back to the office I made a milestone chart summarizing what we would do. It included (1) document and quantify uncertainty in our method of calculating global temperature change from weather station data; (2) investigate the potential for 'early detection' of greenhouse warming; (3) make a 3-D climate simulation for recent decades with actual transient changes of greenhouse gases; (4) program our 3-D model to include sources and sinks of CO_2, to examine potential information in seasonal and geographical variations of atmospheric CO_2.

I had galley proofs of our *Science* paper. I pasted them together as 10 pages to look like the journal. This article, I thought, surely would persuade Koomanoff that we were competent.

Somehow, it did not work. Koomanoff rejected our proposal: zero funding. However, he then agreed to have a meeting at NASA Headquarters to talk about the GISS CO_2 research proposal. It would be in the office of Bob Schiffer, the NASA climate program manager.

I felt buoyed during the flight to Washington. Koomanoff must be reconsidering, otherwise why did he agree to the meeting? There were innovative ideas in the proposal, and we included Wally Broecker and Peter Stone – world-leading experts on the carbon cycle and atmospheric dynamics – as co-investigators. He must have realized that it was a powerful proposal.

On the contrary. My scientific arguments had no impact. Koomanoff said that there were numerous criticisms of our work and our paper in *Science*. It seemed that Koomanoff's mind was made up beforehand. The meeting felt very strange. Why was it even held?

I was downcast, on the trip back home. I suspected that we were rejected not because we had an absence of good ideas to pursue, but because we had good ideas. The Department of Energy did not seem to want such research, especially if it generated publicity. Our *Science* paper probably worked against us. Realizing that did not make the DoE verdict any less devastating.

For the first time, I had to fire people. I had not anticipated that aspect of my job. I appealed to Goddard management, which found a one-time $40K contribution to ease the impact of the decrease in our 1982 budget. Fortunately, my post-doc found a job in his home country, India. Graduate student and co-author, Dan Johnson, had taken a leave of absence and did not return. However, I had to terminate support for two senior computer programmers.

Climate research at GISS differed from that at the centers with larger modeling groups. Climate researchers at NCAR, GFDL and the Hadley Centre, including computer programmers, were in "hard money" government jobs. Except for our handful of civil servants, the rest of the people at GISS were on soft money. Their salaries were dependent on successful research proposals.

I used the bridge funding from Goddard management to keep one of the people who had been supported by the CO_2 program: physicist Sergej Lebedeff. Sergej was working on the weather station temperature data. Real world data are even more interesting than models, and a crucial complement to the models. I hoped to get support from the Environmental Protection Agency (EPA). They had little research money, and they gave it out in dribs and drabs, rather than funding multi-year funding grants, but I expected EPA could cover one person.

Down to Earth with a thud. The Institute's problems were beleaguering. The Goddard Earth Science Directorate required frequent reporting that was bureaucratic, and seemed unnecessarily burdensome, as I traveled to Greenbelt for a staff meeting once every other week. Our computing situation was a special problem. It became clear that NASA had no intention to help us get a new computer or to refurbish the aging GISS building.

Why had things turned sour? In part, the problems seemed to be related to my original sin, the bcc to Walter Sullivan and Rafe Pomerance (I will get to the Pomerance part soon). Scientists tend to react negatively to scientists who get in the news, especially if they seem to court media attention. That probably made it harder to get positive scientific reviews.

Also we pushed the research envelope, and the scientific community is skeptical by nature. That is one reason I sent the paper to many relevant scientists before it was published. However, those prior communications did not reduce post-publication criticism. Three of the published criticisms[8] were by scientists who received preprints: MacCracken, Idso and Rasool.

Rasool's critique, *On Predicting Calamities*, rejected by *Science* but published by Steve Schneider in *Climatic Change*, accused us of emphasizing climate disaster to "get the attention of decision makers who control the funding." In my response, published with Rasool's critique, I emphasized the responsibility of scientists to point out consequences of their findings.[9]

MacCracken was DoE's chief climate scientist, so his criticisms probably did not help. However, it seems that DoE simply did not want to fund us. A university research group that planned to use our climate model simulations to study the effect of global warming on crops was told by DoE that they would not be funded if they used our model results. I felt that the politics of the new United States Administration, affected by the fossil fuel industry, was probably a factor in DoE's decision to renege on their promise to pick up our CO_2 research funding.

What can we learn from our 1981 *Science* paper? In science we usually learn more from our mistakes than from the things that we got right. Global warming since 1981 in the real world has actually been greater than predicted by our simple climate model. It is useful to understand why.

Numbers and graphs should not be forbidding. They can make the climate story much easier to understand. The next chapter looks at how two important parameters – fossil fuel carbon emissions and global temperature – have changed during the historical period.

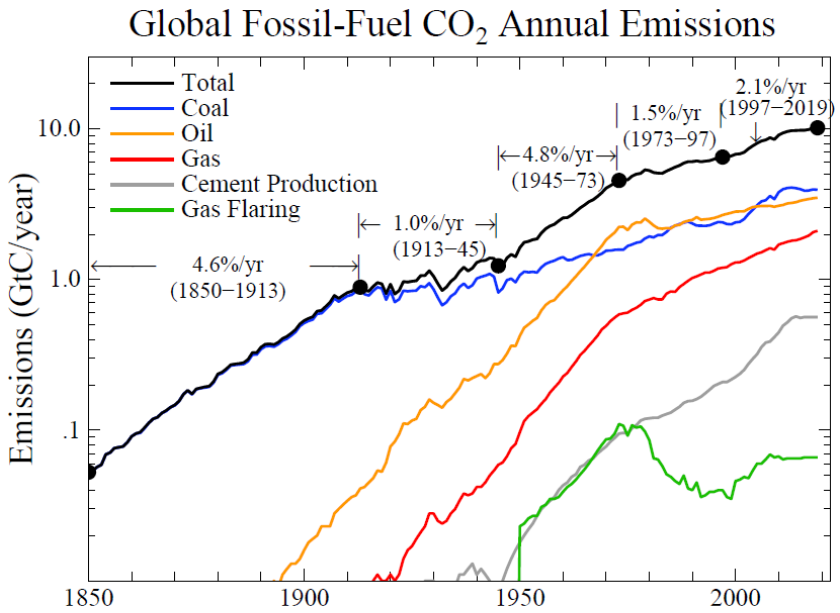

Fig. 20.1. Fossil fuel emissions each year. Emissions are in GtC (gigatons of carbon)

Chapter 20. How Well Did Our 1981 Model Do?

Quantitative graphs are important. We need graphs to understand reality and make wise decisions. If we make the data clear, science has the best chance of winning out, rather than the best salesperson or people fronting for special interests.

Let's start with the amount of CO_2 injected into the air each year by burning fossil fuels, that is, coal, oil and gas. The graph above shows emissions in gigatons of carbon (GtC).

A gigaton of carbon (1 GtC) is a billion metric tons (10^9 tons) of carbon. A GtC is the same as a petagram of carbon (PgC), 10^{15} grams, because there are a million (10^6) grams in a ton.

Emissions are often stated in the mass of CO_2, rather than the mass of carbon (C). Because C has atomic mass 12 and oxygen (O) has mass 16, the mass of CO_2 is 44. Therefore the mass of CO_2 produced by burning fossil fuels is about 3.67 (44/12) times greater than the mass of carbon.

Finally, note that the vertical scale of our emissions graph is logarithmic, so that we can show clearly the large range of emissions over time. Today's emissions are about 10 GtC per year. In the era spanning the two world wars (1913-45), emissions were about 1 GtC per year.

It is worth looking closely at the coal curve, the blue curve in the figure. Notice how coal use shot up beginning about 20 years ago, more than doubling its emissions.

The industrial revolution was powered almost entirely by coal, so prior to 1900 the curves for coal emissions and total emissions fall on top of each other. Note that oil emissions grew rapidly in the 20th century, yet today coal is the greatest source of CO_2!

Already in 1981 we knew that coal was the biggest issue. We could and should have prepared other technologies to generate electricity. Twenty years was enough time to do that, but instead we twiddled our thumbs. Actually, we did worse. We subsidized coal.

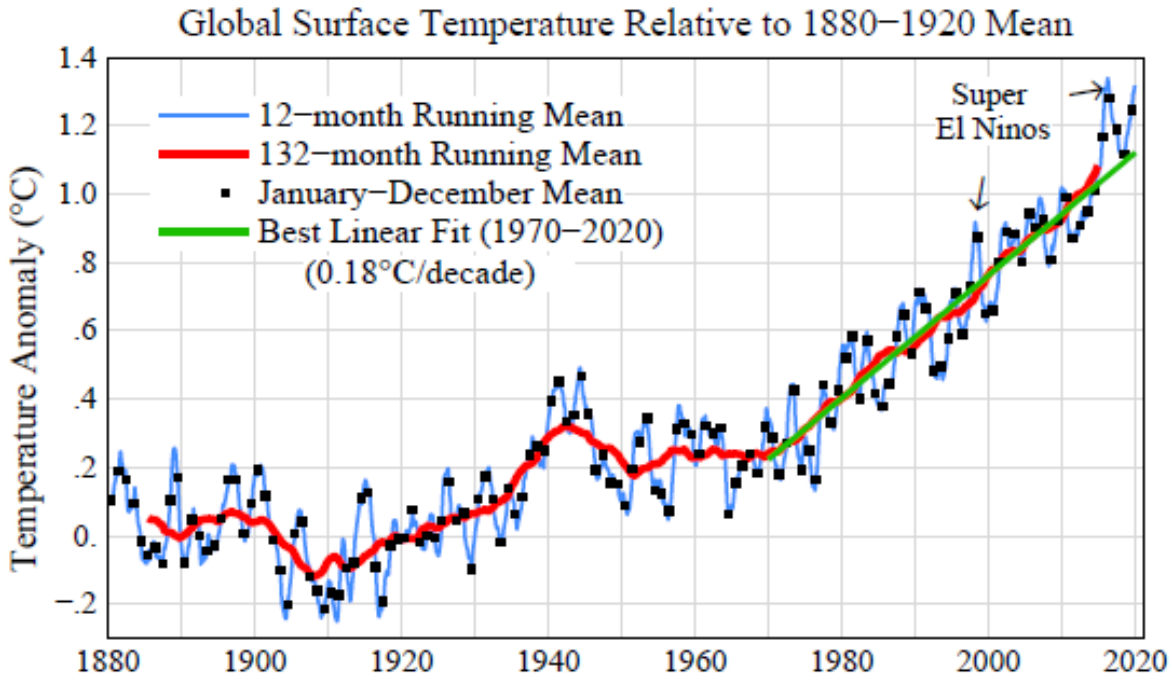

Fig. 20.2. Observed global surface temperature relative to 1880-1920.

However, I am getting ahead of the story. Let's develop more facts before we talk about policy.

Fossil fuel emissions grew more than 4%/year from the end of World War II until 1980. For the post-1980 world in our *Science* paper we examined three scenarios: fast growth (4%/year), slow growth (2%/year), and no growth. We took slow growth as being the most plausible.

Real world carbon emissions since 1980 grew at a rate of 1.8%/year. So our assumption that 'slow growth' (2%/year) was most likely turned out to be correct.

The final figure of our *Science* paper used the slow growth scenario to conclude that global warming would rise out of the noise level of natural variability by about 1990. That same figure predicted that global warming, relative to 1880, would reach 1°C in 2020, with 0.6°C of that warming during 1980-2020. Now let's compare the real world with the model.

Observed global temperature shown in the figure above is relative to the average for 1880-1920. The 1880-1920 mean is our best estimate for pre-industrial temperature. Although some human-made gases already caused warming in 1880-1920, that warming tended to be offset by higher than average volcanic aerosols in that period.

The global temperature anomaly reached +1°C in about 2015, thus about 5 years earlier than in our model. That is pretty good agreement, but it is worth taking the trouble to understand why our simple climate model slightly under-predicts the rate of global warming, because it forces us to think a bit about the physics of the climate system.

First let's explain nomenclature in our global temperature graph. The annual (Jan-Dec average) temperature is shown by the black squares on the 12-month running mean curve. The 12-month running mean contains more information: it provides a smooth curve that defines well the effect of volcanoes or El Niños. The 132-month (11-year) running mean smooths out short-term variability including much of the potential effect of solar variability.

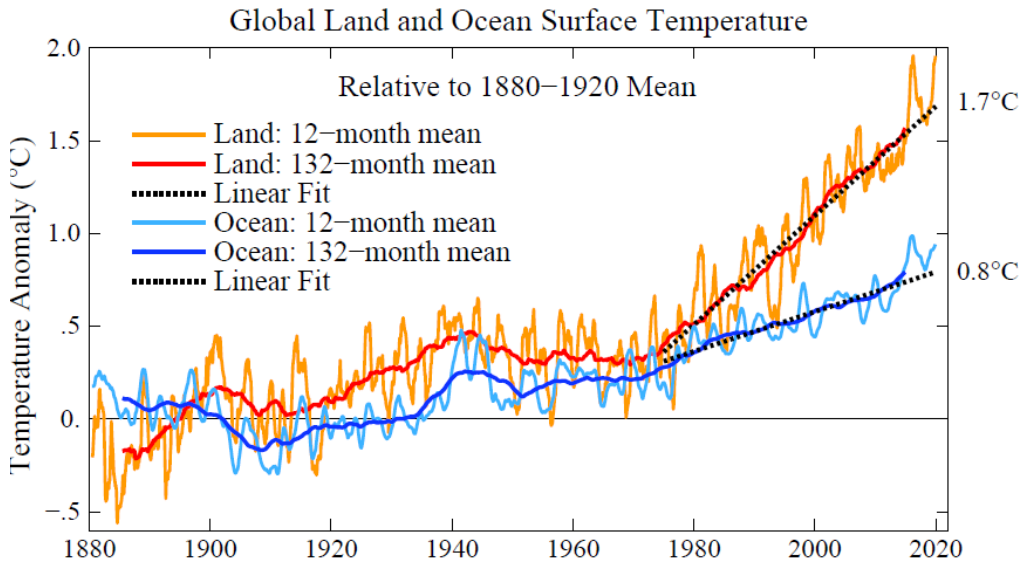

Fig. 20.3 Observed global surface temperature relative to 1880-1920 for land and ocean.

It is easy to see why our 1-D climate model under-predicted global warming, if we look at real world data for ocean and land regions separately, as shown in the above figure. By 2019 observed warming over ocean is +0.8°C and warming over land is +1.7°C. Because ocean covers about 70% of Earth's surface, global mean warming is 0.7×0.8 + 0.3×1.7 = 1.07°C.

Our simple 1-D model with ocean heat capacity at its foot gave an answer closer to that for an all-ocean planet. In the real world, ocean thermal inertia has some influence on the magnitude of warming over land, but because of the time required for mixing of marine and continental air, the land warms much more than the ocean.

We could tinker with the 1-D model to include two surfaces at its base, ocean and land, with an exchange of air between the ocean and land regions. However, a better solution is a 3-D climate model with realistic atmospheric dynamics that properly mixes marine and continental air.

Given our limited resources, we focused our efforts on a modeling path that I felt was most promising. That was a coarse resolution 3-D global model, which allowed realistic transport of energy between land and ocean, and transport of energy from low latitudes toward the poles.

One reason for our minimalist, coarse-resolution, approach to global modeling was our limited resources. We obtained money from EPA in 1982 to purchase two workstations. These could be dedicated to running the climate model, but they were slower than the 360/95 computer and the EPA support was a one-time shot – they would not entertain 3-year proposals.

Because of the termination of funding for our carbon dioxide and climate research, it was difficult for us to pay a graduate student, let alone hire a programmer or buy a computer.

So I was excited to receive an invitation to testify at a Congressional hearing in March 1982. The hearing was for the purpose of examining DoE's CO_2 and climate research program. Specifically, why was DoE reducing its funding for this program?

I wondered why they asked me to testify. Did they know about our difficult funding situation? This was our chance, it seemed, to restore our resources for research.

[1] Sullivan, W., Study finds warming trend that could raise sea levels, New York Times, 22 August, 1981.
[2] George Pieper may have been a wonderful person. Jastrow's quoted words were the first ones out of his mouth when Jastrow and I entered the office of the Director of Goddard Space Flight Center in Greenbelt. We had come to complain about Pieper's effort to get the GISS PPR (Photopolarimeter Radiometer) removed from the Galileo Mission. I had submitted the PPR proposal, and it was selected, without Pieper's knowledge, because I did not want him to lard the proposal with money for engineers at Greenbelt (see *End of the Rainbow*).
[3] Carl was manager of the support services contractor staff.
[4] Prof. Michael Rampino, now at New York University, in a note to me, questions whether Jastorw's course was any less "hard science" and thus the motives of the other professors.
[5] Zeiger, R., Jastrow resigns, cites sudden cut in honorarium, *Columbia Spectator*, 29 September 1982.
[6] Hansen, J. A Little Story About Dr. Robert Jastrow, www.columbia.edu/~jeh1
[7] Lamb's response is a precious long-hand letter in an aerogram that I still have in my folder for the 1981 Science paper. Aerograms were a mode of communication in that era, written on a tissue-thin 6-by-12 inch blue paper, which was folded twice to make a 6-by-4 inch envelope with a postal service warning to enclose nothing.
[8] Rasool, S.I.: On predicting calamities, Climatic Change 5, 201-202. 1983. MacCracken, M.C. and Idso, S.B.: Climatic effects of atmospheric carbon dioxide, Science 220, 873-875, 1983.
[9] I never understood the seeming anger in Rasool's critique. I suspected that he was disappointed that NASA did not choose him to replace Jastrow. Indeed, he was a good communicator and may have been more successful in obtaining institutional support for GISS. In later years we were on friendly terms and never discussed his critique.