Jule Charney

Chapter 17. Charney's Puzzle: Is Earth Sensitive?

President Jimmy Carter was concerned about growing United States dependence on oil from the Middle East. On 18 April 1977, just three months after assuming the Presidency, he delivered an *Address to the Nation on Energy*, while sitting in the White House, wearing a sweater, with the White House thermostat turned down.

Oil and gas supplies are limited, President Carter said, so "we need to shift to plentiful coal" and "we must start now to develop the new, unconventional sources of energy that we will rely on in the next century."

Conventional fossil fuels are the oil, gas and coal that can be readily extracted from large deposits in the ground without special efforts and expenditure of energy. Unconventional energies include tar sands and "tight" gas and oil deposits that are extracted by high pressure hydraulic fracturing (fracking) of rock formations. Coal gasification is another example of unconventional fuel. Because energy is required to extract these fuels, they are more carbon-intensive than the conventional fossil fuels, that is, they produce more CO_2 per unit of useful energy for the consumer. Unconventional fossil fuels also produce regional pollution, including expanding plumes of polluted groundwater.

"We've always been proud of our leadership in the world. Now we have a chance again to give the world a positive example," President Carter concluded.

Indeed. United States actions mattered. Between 1915 and 1950 the United States emitted 45 percent of global fossil fuel emissions, with 200 other nations emitting 55 percent. By 1977 emissions from other nations had climbed, but during Carter's presidency U.S. annual emissions were still more than one-quarter of global annual emissions.

President Carter obviously needed good scientific advice. President Carter's Science Adviser, Frank Press, requested advice from the President of the National Academy of Sciences, Philip Handler. The charge that the Science Adviser gave to the National Academy was broad, ending:

> ***"To summarize in concise and objective terms our best present understanding of the carbon dioxide/climate issue for the benefit of policymakers."***

That charge was a license to provide broad advice related to the crucial issue of energy policy. It should have been crystal clear that President Carter was in desperate need of advice on Earth's climate system. Fourteen years had passed since the 1965 PSAC CO_2 study headed by Roger Revelle. Understanding of the climate issue had advanced considerably.

Philip Handler chose a stellar group of meteorologists and oceanographers for the National Academy study of the issue: Akio Arakawa, D. James Baker, Bert Bolin, Jule Charney, Robert Dickinson, Richard Goody, Cecil Leith, Henry Stommel and Carl Wunsch. Jule Charney was the obvious choice to chair the study. It is likely that Handler made that decision and consulted with Charney before deciding on committee membership.

Charney chose a narrow focus, on climate sensitivity, for the National Academy study. This was consistent with the limited time for the study. The study group met for five days, 23-27 July 1979. Charney had continuing consultations with study group members and other scientists in the following weeks before completing a 33-page report.[1]

Charney's narrow focus was ingenious, yielding clear definition of the core scientific issue in global climate change. His sharp focus provided a quantitative framework for thinking about climate sensitivity. The value of Charney's framework has not diminished over time.

Charney knew there were many uncertainties about how fast atmospheric CO_2 would increase. His group had little expertise in the carbon cycle, the processes by which carbon compounds are interconverted in the environment as CO_2 is incorporated into living tissue by photosynthesis and returned to the atmosphere via respiration, decay of dead organisms, and fossil fuel burning.

However, CO_2 in the air was observed to be increasing rapidly. Keeling's data showed that CO_2 had passed 335 ppm (parts per million), was increasing more than 1 ppm per year, and the rate of growth was increasing as annual fossil fuel use continued to increase. It was known that a lot of fossil fuels were present in the ground, so, if fossil fuel use continued to increase, airborne CO_2 at some point would be twice as great as the preindustrial level, estimated to be about 280 ppm.

Charney defined an idealized gedanken problem: how much would global temperature increase if the amount of CO_2 in the air doubled from its preindustrial amount? Such a doubling would likely occur in the 21st century, if there were no efforts to constrain fossil fuel use.

The problem was idealized in several ways for the sake of being a tractable, well-defined problem. The global warming was defined as that which exists after the planet returns to near-equilibrium with space, in response to the planetary energy imbalance created by the added CO_2.

The report suggested that delay in attaining full warming could be as much as "a few decades." We now know that the study group greatly underestimated the delay caused by the ocean's thermal inertia. They also did not seem to recognize that the delay time is a very strong function of climate sensitivity, a matter that was not clarified until the mid-1980s.

The lag in climate response has important practical implications. It causes human-made climate change to be an intergenerational matter. One generation can cause climate change that becomes large only during later generations. Because of the importance of this climate change lag, we defer discussion of it to a later chapter, where we can be quantitative.

Climate feedbacks were a principal topic of the Charney report. Charney did not explicitly divide feedbacks into the categories of fast feedbacks and slow feedbacks, but the nature of the Charney study and report implicitly led to such a framework for analysis.

Consider first the 'no feedback' case. If the amount of CO_2 in the air is doubled and everything else is held fixed, how much will Earth's surface temperature increase? Everyone gets the same answer, if they do the radiation calculation accurately. Earth's surface and lower atmosphere must warm about 1.2°C (2.2°F) to restore energy balance with space.

As a reminder: increased CO_2 makes the atmosphere more opaque in the infrared part of the spectrum. Thus radiation to space occurs from greater altitudes, where it is colder. The amount of radiation to space is therefore reduced, and the planet is out of energy balance: less energy is emitted to space then is received from the Sun. So Earth warms until energy balance is restored.

Thus the no-feedback climate sensitivity is about 1.2°C for $2\times CO_2$. Doubled CO_2 is a forcing of about 4 W/m^2, so the no-feedback sensitivity can also be stated as 0.3°C per W/m^2. The conclusion would be that climate is not very sensitive, if there were no feedbacks.

However, if the world warms up 1.2°C, that will cause other things to change. Those changes, called climate feedbacks, can either amplify or diminish the no-feedback climate sensitivity.

Jule Charney was delighted that our model gave a different global warming than Manabe's. Charney had a predilection for 3-D GCMs, perhaps in part because he was instrumental in development of the fundamental equations at the core of the models, but mainly because these global models provide a framework within which the various climate feedbacks can interact.

Different results from two different models provided Charney something to chew on. Charney invited me to give a presentation on our results during their workshop at the Woods Hole Oceanographic Institute in Massachusetts. I was glad to have an excuse not to go.

Their workshop occurred during the first Summer Institute on Climate and Planets, which I was running. I was reluctant to reveal my ignorance before Charney's stellar committee, which included, for example, Robert Dickinson, recognized as a genius, who knew everything that I knew about climate feedbacks, and much more.

Instead, we scheduled a telecon on which I tried to answer Charney's questions. In addition, after their workshop ended, Charney sent Arakawa to stay with us a few days, examining computer output to try to understand our model and its simulated climate response.

Manabe's latest model yielded a climate sensitivity of 2°C for $2\times CO_2$, while our model gave almost 4°C. Charney called a few times after the workshop, while he was finishing the report, and we discussed some of the possible reasons for the difference.

Clouds were likely one reason. Manabe specified the cloud distribution in his model to make it as realistic as practical, and he kept the clouds the same in the $2\times CO_2$ experiment. So the clouds were warmer in the $2\times CO_2$ world, and the cloudtops radiated more energy to space.

Clouds were computed in our model, occurring at the atmospheric layers and at the times when the air became saturated. Some cloud types tend to occur at a given temperature, so in the

$2\times CO_2$ world as the atmosphere warmed these clouds moved to higher altitude. It is colder at the greater altitude, so the clouds radiate less energy to space than they would if they had stayed at the same altitude. Also the cloud cover decreased slightly in our $2\times CO_2$ world, which increased the amount of sunlight absorbed by Earth.

Sea ice was another difference between our models. Manabe's control run (climate simulation with $1\times CO_2$) had less sea ice around Antarctica than the real world, while our control run had more sea ice than observed. The area with sea ice decreases in the $2\times CO_2$ world, which is an amplifying feedback, because the ocean is much darker than sea ice, so it absorbs more sunlight. Because Manabe's model did not have as much sea ice as ours to begin with, the amplification of warming due to sea ice loss was less in Manabe's model than in our model.

Water vapor was the largest feedback in both models. The amount of water vapor that the air can hold is a strong function of temperature, as readily noticed in daily life. If we let outdoor air into the house in winter and heat it to room temperature, the relative humidity becomes very low – even if it is snowing outside, which implies that the humidity was near 100 percent outside. Our models were similar in this calculation, but multiple amplifying feedbacks reinforce each other. Therefore, because the cloud and sea ice feedbacks were larger in our model, the water vapor increase in our model was larger than in Manabe's model.

Charney decided that his central estimate for climate sensitivity was 3°C for $2\times CO_2$, which was about the midpoint between the two GCM results. Discussion of feedbacks in the Charney report, aided by 1-D models, would have implied 2.4°C as the most likely sensitivity. However, Charney told me that he trusted the 3-D models more, because they allowed interactions among the feedbacks and included expected amplification of warming at high latitudes.

Choosing an uncertainty range was more difficult. Charney settled on 3 ± 1.5°C for expected equilibrium global warming due to doubled CO_2. That is a large range, a factor of three, from 1.5°C to 4.5°C. Furthermore, he later clarified that the estimate only meant that there was at least a 50 percent chance that real world sensitivity was within the 1.5-4.5°C range!

Forty years later, the range would not be much narrower, if it relied entirely on climate models. One problem with models is that we are never certain that all significant processes are included. Also some processes, such as cloud formation, are difficult to simulate, and a small change of cloud cover can have a significant effect on the amount of solar energy absorbed by Earth.

Fortunately, Earth's climate history provides ways of assessing climate sensitivity that are potentially much more accurate, as we will discuss in due course. For now, I make only a few points of clarification. We must discuss the time scale of different climate processes, including the ocean and so-called "slow" climate feedbacks.

Charney's climate sensitivity is the 'fast-feedback' climate sensitivity. It includes the feedback effects of water vapor, clouds and sea ice. Today we understand that the water vapor feedback is strongly amplifying, the sea ice feedback is amplifying, and the cloud feedback is uncertain but believed to be near neutral or slightly amplifying. On net these fast feedbacks increase climate sensitivity from 1.2°C to about 3°C for doubled CO_2.

Charney's idealized gedanken problem assumes that the Greenland and Antarctic ice sheet sizes are fixed, consistent with an expectation that ice sheets change only on millennial time scales. However, given enough time, warming will cause ice sheets to shrink, exposing a darker surface that absorbs more sunlight, which causes more warming. Other slow feedbacks are also excluded in Charney's evaluation of climate sensitivity, for example, the melting of tundra with release of greenhouse gases CO_2, CH_4 and N_2O. Forests would expand to higher latitudes in the Northern Hemisphere in a world with 3°C warming, covering a much large area. That feedback was not included in either Manabe's model or our model.

"Earth system sensitivity" is the terminology used for climate sensitivity when all feedbacks are included. As we will see, Earth's history reveals that the slow feedbacks, on net, are also amplifying, so Earth's climate is even more sensitive than indicated by Charney's analysis.

Conclusions of Charney's report ended with: "To summarize, we have tried but have been unable to find any overlooked or underestimated physical effects that could reduce the currently estimated global warmings due to a doubling of atmospheric CO_2 to negligible proportions or reverse them altogether. However, we believe it quite possible that the capacity of the intermediate waters of the oceans to absorb heat could delay the estimated warming by several decades. It appears that the warming will eventually occur, and the associated regional climatic changes so important to the assessment of socioeconomic consequences may well be significant, but unfortunately the latter cannot yet be adequately projected."

The Preface to the Charney report, written by Verner E. Suomi, Chairman of the National Academy of Sciences Climate Research Board, states "The conclusions of this brief but intense investigation may be comforting to scientists but disturbing to policymakers. If carbon dioxide continues to increase, the study group finds no reason to doubt that climate changes will result and no reason to believe that these changes will be negligible. The conclusions of prior studies have been generally reaffirmed. However, the study group points out that the ocean, the great and ponderous flywheel of the global climate system, may be expected to slow the course of observable climatic change. A wait-and-see policy may mean waiting until it is too late."

Suomi correctly notes that the caveat about the delay caused by the ocean is not a benefit. It means that a "wait-and-see" approach by policymakers could be dangerous!

The ultimate charge to NAS was: *"To summarize in concise and objective terms our best present understanding of the carbon dioxide/climate issue for the benefit of policymakers."* Does the report adequately inform policymakers? Suomi's words "…comforting to scientists but disturbing to policymakers…" are relevant. But did the report disturb policymakers? Were we, the scientific community, clear enough, strong enough, in our warnings to policymakers?

For my group, the chance to interact with Charney was a privilege and good fortune. Charney treated us with the respect accorded more established researchers, despite the coarse resolution and unpublished status of our climate model.

Charney's approval was noticed by NASA Headquarters. Publicity surrounding the Charney report included the fact that our model results were a prominent part of the report. It is likely that Charney's approbation played a role in the decision of NASA to fund both the CO_2 and cloud research proposals! We received $100K funding immediately for the CO_2 research and approval for $230K per year beginning the next fiscal year. The cloud research also was funded, beginning, if I remember right, at a level of $100K per year.

I had stopped work altogether on the Farmers' Forecast. I had ammunition for any dispute with Dr. Jastrow. Our model with coarse resolution did a good job of simulating the atmosphere's general circulation, consistent with our initial proposal, which was guided by advice of atmospheric dynamist Prof. Peter Stone. I wanted to focus on the physics of long-term climate. Farmers' forecasting, essentially extended range weather forecasting, required a different focus.

Before any fight could occur, a referee stepped into the ring. The NASA Inspector General. He would alter our courses, both Jastrow's and mine.

The following two years were probably the best years of my research career. We had money for students and research associates, and I worked assiduously on a paper on CO_2 and climate. I thought we could say more than the Charney report had said about expected global warming.

Fig. 18.1. Anniek, Erik and Kiki in Copenhagen in 1981.

Chapter 18. Original Sin and the Inspector General

My original sin seems to have been committed in August 1981 in the Moon Palace with Dr. Jastrow and Walter Sullivan, the science writer for the New York Times. I was in a rush, getting ready for a flight to Amsterdam. Anniek and the kids had already been in Holland for 10 days, where I was supposed to meet them. We planned to borrow Anniek's sister's car and drive to Hamburg for an IAMAP (International Association of Meteorology and Atmospheric Physics) meeting. We would then drive to Copenhagen, the first time in Denmark for any of us. We could stay at a hotel right next to Tivoli Park and we could go see the mermaid in the harbor. It seemed to be such a good plan…but wait, I am getting ahead of myself.

One problem that I proposed for students during the 1979 Summer Institute on Planets and Climate was to estimate global temperature change in the past century. Weather stations around the world had been making observations that long. Wouldn't it be interesting to compare observed temperature change in the real world with what is expected due to increasing CO_2, using the climate sensitivity that the Charney study suggested?

Prior temperature analyses[2] emphasized the Northern Hemisphere, where most weather stations were located. However, if we think about Earth as a planet that we are exploring, we would be happy with the number of measurements in the Southern Hemisphere. The data could not yield the absolute global average temperature, because temperature varies a lot in short distances, but global warming should have a smoother distribution – if it were occurring.

Roy Jenne of the National Center for Atmospheric Research sent us a computer tape with data to 1978 taken at thousands of stations around the world. Dealing with those data was a bigger job than I guessed. There were short records, long records, broken records for different stations. The computer programming was too much for a student to complete during the Summer Institute. But now I had funding, so I hired a professional programmer, Sergej Lebedeff, to help.

The basic idea, in my proposed data analysis scheme, was that point measurements contain information for a large area. If a winter is unusually cold in New York, it is probably a cold winter in Philadelphia. Weather models and observations show that temperature anomalies have spatial scales of a few thousand kilometers. So we had a useful estimate of the temperature anomaly at a point if we had at least one station located within 1000 kilometers (600 miles).

The merit of this scheme was that it allowed us to extend temperature change estimates well into regions where few people lived, such as the Arctic and throughout Siberia. This idea worked quite well, as we would prove later.

What we found was that the world had become warmer over the period 1880-1978 by 0.4°C, which is about 0.7°F. This was a bit of a surprise, because analyses for the Northern Hemisphere had shown a strong cooling, about 0.5°C, between 1940 and 1970. There was a lot of variability in space and time, but we showed that, over the full period, there was global warming.

Was this warming caused by increasing CO_2? It would not be possible to prove that, but we could at least calculate the expected warming due to increasing CO_2 and compare this with observations to see if there was consistency.

We made calculations with a simple (1-D) climate model, so that we could examine many cases. We chose feedbacks that seemed reasonable to us, for example, water vapor was assumed to increase when the atmosphere became warmer. The climate model had a sensitivity of 2.8°C for $2\times CO_2$, which was near the middle of the range estimated by Charney.

We included the thermal inertia of a 100-meter thick ocean mixed layer, and we allowed heat to be exchanged with the deeper ocean as a diffusive process, with the diffusion coefficient based on the rate at which inert chemical tracers were observed to penetrate the real ocean.

We knew there were other significant climate forcings, some of which were measured. We assumed that the cooling effect of human-made aerosols tended to offset greenhouse warming by non-CO_2 gases, but that would not be true on a hemisphere-by-hemisphere basis or as a function of time. Indeed, aerosols were a good candidate for causing the 1940-1970 Northern Hemisphere cooling, but aerosol changes were unmeasured.

The calculated global warming over the 100-year period was consistent with observations. The large temporal variability in the observations prevented a stronger statement, but we could make testable predictions, which we noted in the paper's abstract:

> *Summary.* The global temperature rose by 0.2°C between the middle 1960s and 1980, yielding a warming of 0.4°C in the past century. This temperature increase is consistent with the calculated greenhouse effect due to measured increases of atmospheric carbon dioxide. Variations of volcanic aerosols and possibly solar luminosity appear to be primary causes of observed fluctuations about the mean trend of increasing temperature. It is shown that the anthropogenic carbon dioxide warming should emerge from the noise level of natural climate variability by the end of the century, and there is a high probability of warming in the 1980s. Potential effects on climate in the 21st century include the creation of drought-prone regions in North America and central Asia as part of a shifting of climatic zones, erosion of the West Antarctic ice sheet with a consequent worldwide rise in sea level, and opening of the fabled Northwest Passage.

The paper employed climate modeling, modern observations of ongoing climate change, and Earth's paleoclimate history. None of these alone, or even two in combination, would permit such extensive conclusions. The combination of all three permits greater insight.

I wanted the implications of the science for energy policy to be explicit and unambiguous, and thus wrote a concluding paragraph:

> Political and economic forces affecting energy use and fuel choice make it unlikely that the CO_2 issue will have a major impact on energy policies until convincing observations of the global warming are in hand. In light of historical evidence that it takes several decades to complete a major change in fuel use, this makes large climate change almost inevitable. However, the degree of warming will depend strongly on the energy growth rate and choice of fuels for the next century. Thus, CO_2 effects on climate may make full exploitation of coal resources undesirable. An appropriate strategy may be to encourage energy conservation and develop alternative energy sources, while using fossil fuels as necessary during the next few decades.

Publication of this paper required almost a year – I submitted it to *Science* three times and to *Nature* once. *Science* returned the first submission, saying that it was three times longer than they could publish. However, I knew that they sometimes published papers exceeding their nominal limit. I wanted the paper to include the entire story – from the energy balance of the planet to policy implications -- so I kept reducing the length 10-15 percent and resubmitting it.

I was confident that *Science* was interested in the paper, because the editor, Phil Abelson, commented on it to Jule Charney.[3] Abelson's comment was a criticism, about exponential growth in energy growth scenarios, but it showed he had looked at the paper in detail, so I was encouraged. I revised some scenarios for future energy use and resubmitted the paper.

Eventually *Science* accepted it! It was still 10 full journal pages, which was an unusually long article for that journal. It would finally appear in press[4] in late August 1981.

Meanwhile, during the long effort to publish the *Science* paper, GISS entered a quieter period of research. We survived departure of the large GARP-funded group in part by returning one floor of the GISS building to Columbia University, thus reducing our rent payment. We did not see much of Dr. Jastrow, who was busy writing books and teaching. Also, he worked with several scientists who were investigating use of satellite observations to explore earth resources.

Jastrow's textbook *Astronomy: Fundamentals and Frontiers*, co-written with high school teacher Malcolm Thompson and published initially in the early 1970s, was excellent but required revisions for successive editions. Annual enrollment in Jastrow's course for Columbia and Barnard students, *Stars, Planets and Life*, grew to more than 400. Popularly known as "*Astro Jastrow*," it was undemanding and thus a student favorite to fulfill their science requirement.

I was glad that Jastrow also began teaching at Dartmouth and bought a house there. All that time at Dartmouth delayed the anticipated confrontation over the Farmer's Forecast. That delay provided the time needed for an unexpected intervention.

Dr. Thaddeus, as the senior GISS scientist, suggested that he and I seek a meeting with the Goddard Director. Pat was concerned about the future of the Institute. Dr. Jastrow had been talking about perhaps partially retiring from the government, working only half-time.

I continually rejected Pat's suggestion that we seek a meeting. I argued for a delay, on the rationale that our case for Goddard support post-Jastrow would get stronger if we kept working hard and produced scientific results. Our climate research funding had just begun. I had high hopes that our paper for *Science* would be accepted and have a good impact.

I do not remember for sure which came first: my eventual consent for a trip to Goddard or news of the NASA Inspector General investigation of GISS. Probably it was the latter.

I vividly remember the trip. Pat was as bad as me about waiting to the last minute. When we hailed a cab on Broadway at 112th Street, it was less than half an hour until our flight departure from LaGuardia. There would be a later flight, but then we would be late to the meeting.

Pat urged the driver to go fast and shouted at the driver when it seemed he would go all the way to the stoplight at 125th Street. The driver turned just in time to screech around the corner onto La Salle. Pat sat back and said "many a flight has been caught because of this shortcut." It saved a minute or two; we caught our flight.

On the airplane, Pat and I agreed on our pitch to management. We were both in the midst of productive research and hoped to avoid interruption. If Jastrow were to retire, either of us was willing to serve as director or interim director. Neither of us depended on Jastrow for our research, and we did not want to change our research directions.

Pat did almost all of our talking. He was of Irish stock, bushy eyebrows, wavy hair, not very tall, but a strong body, as shown by his skill on the still rings (https://vimeo.com/48171442 at minute 11:18). He was articulate and enthusiastic, a burning ball of energy, as he talked about the significance of his work. He discovered numerous interstellar molecules, and with a 4-foot microwave receiver he was mapping the structure of the galaxy.

Pat's presentations were always the highlight of GISS science, but this time he added one unusual comment. He said that a Nobel Prize was likely to be given for the sort of work he was doing, and he "wouldn't mind" getting it.[5] Scientists normally would not say such a thing, even if they thought it. It made me wonder if he really wanted to be Director of GISS – but more likely he was just saying: look what you might lose if you don't support GISS.

We did not get any promises, but it was a friendly meeting. We went home with a hopeful feeling that Goddard would support us, with or without Jastrow.

The Inspector General's investigation came out of the blue. The scuttlebutt, from Jastrow's secretaries, was that a disgruntled employee at GISS filed a complaint. The complaint was that Dr. Jastrow used government resources for personal gain.

Surely Dr. Jastrow had NASA approval to teach at Columbia and Dartmouth. The universities paid him well, because his classes were huge. Such outside activity, while being paid a fulltime government salary, was permissible at that time, if approved by higher management levels. However, government resources were not to be used for the outside activity.

It was plain to see that Dr. Jastrow sometimes worked on lectures in the office during business hours. The university provided teaching assistants, but Dr. Jastrow also used his government support staff to assist him. This was true for his book writing, as well as his teaching. The IG documented examples, such as the government library books cut up by a government draftsman in making viewgraphs for Dr. Jastrow's class. Were such instances incidental, a minor offense?

The IG was impressive. He had the skills of a good lawyer and a law enforcement official. I wondered how NASA managed to keep such people. Government salaries are limited.

The IG interviewed many people, including all the senior staff members. He was curious about why the other senior staff members used “Bob” in reference to Dr. Jastrow, while I always used “Dr. Jastrow.” I shrugged; I didn’t know. The IG spent a lot of time sitting in my office.

I had the corner office on the 6th floor, just below Dr. Jastrow’s 7th floor office. There was a small adjoining office for my secretary, Brenda, a young, curvaceous, Cuban-American. Brenda was out one Friday, when the IG and I were talking in my office, so Brenda’s place was taken by one of Dr. Jastrow’s several secretaries – because Jastrow insisted that my phone always be covered.

The secretary, a very pleasant Chinese lady, stuck her head in the door and said “it’s Brenda – she wants to know if she can come in this weekend to use the typewriter.” Brenda was an aspiring writer, not a government employee, her salary paid via our support services contractor. I said “no, of course not.” The secretary, not too astute, was perplexed: “no means yes, right?”

“No! No means no – give me the phone!” “Brenda, don’t you know we are being investigated by the IG? You cannot use a government typewriter. We have to be whiter than white!” The IG sat there the whole time with a grin on his face that went from ear to ear.

Later I concluded that he was not investigating me. He already had decided that I was honest. He probably was trying to break me of any bad habits that I might have picked up.

Dr. Jastrow met with science writers occasionally. Once, in the summer of 1981, I was in Dr. Jastrow’s outer office when he was visited by Walter Sullivan, the venerable science writer for the New York Times. Dr. Jastrow introduced me as having an instrument on the Galileo Jupiter mission. Sullivan expressed interest in planetary science, so I was invited to go to lunch with them at the Moon Palace, kitty-corner across Broadway from GISS.

I did not have much to say about Jupiter. I could only relate the work Makiko and I had done. We disproved John Lewis’ proposed cloud layering on Jupiter We also inferred, based on methane and ammonia absorption lines, that carbon and nitrogen were more abundant in the Jupiter atmosphere than on the Sun.

However, at the last moment, as we were getting up to leave the Moon Palace, I mentioned our CO_2 *Science* paper about to come out in *Science*. He expressed interest, so I said that I would mail him a copy of the paper, along with a one-page summary of conclusions that I had written with the hope of sparking interest at Goddard Public Affairs.

Then I went back to my office and wrote instructions for Brenda. I still have a copy of my note – it is in the *Science* paper folder. In the note I asked Brenda, in addition to sending the paper and its summary to Goddard Public Affairs, to send a bcc, not a cc, of the summary and *Science* paper, to both Walter Sullivan and Rafe Pomerance. The bcc indicates that I realized I was violating bureaucracy procedures.

Inclusion of a bcc to Pomerance is something that Rafe and I puzzled over recently. We do not remember how I knew of his possible interest and his address. It is likely that he had called me. Months earlier I had sent a draft of the paper to 30 of the most relevant scientists in the world.

The interaction with Sullivan was my original sin. Consequences, as we will see, affected my life and the Institute for a decade. Somehow I did not learn my lesson. An analogous event decades later would have more significant consequences.

Anniek met me at Schiphol. We stayed one night with her sister Colette's family. Then we drove off in Colette's car to Hamburg.

Anniek says that it rained every day we were in Hamburg, but Anniek always made the best of any situation. She took Erik and Kiki to museums, while I went to meetings. When the kids doodled in the museum's sign-in book, the hostess found a blank book and told them they could fill the book with their drawings. Hamburg was very hospitable.

We did not have the opportunity to wait for Hamburg sun. I got a call from Goddard asking me to return to New York and become the Interim Director of the Goddard Institute for Space Studies. Dr. Jastrow was retiring.

One problem: the cloud climatology meeting, important for our newly funded cloud research program and my main reason for being in Hamburg, had not yet occurred. Problem solved: Goddard gave emergency travel approval for Bill Rossow to come over and take my place. My career in clouds research was over; Bill took over the clouds project forever.

A second problem: We had scheduled several days for one of our very infrequent vacations, to follow the Hamburg meeting. We shortened it. We took the car on a ferry to Denmark, spent one day in Copenhagen, and then drove along the Dutch delta works on the way to Leiden.

It was sunny while we were in Denmark. We walked out the hotel front door straight into Tivoli Park. We made a brief visit to the Copenhagen harbor to see the Little Mermaid on the rock overlooking the harbor. The photo I took there captures Erik and Kiki's personalities. Seven-year-old Erik was usually cheerful, while four-year-old Kiki was often more pensive.

After the long drive back to Leiden we went to a crepe restaurant on Steenstraat across the street from Beestenmarkt, the old animal market. It was a restaurant that Anniek and I had gone to when I was a post-doc in Leiden. While waiting for Kiki to finish at least half of her crepe, I doodled with a Rubik's cube, gave up, and handed it to Erik.

With his mop of hair hanging over his eyes, Erik started twisting the cube one way and another very fast. Then he handed me the completed cube. Two men in the next table watched in amazement, the one man exclaiming something in Dutch. After they left, Anniek told us what he had said: "I knew there was an easy way to solve it! Tonight I'm going to figure it out!"

What they did not know was that I had brought an article with a prescription for solving the cube. Erik and I each succeeded in memorizing it in rainy Hamburg, but I had forgot a step. Erik could do it faster than I could – my excuse was that his small fingers were more dexterous.

I left the family behind the next morning and headed for New York. If I had realized the reception that was waiting, I would not have been so eager.

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[1] Charney, J., Arakawa, A., Baker, D., Bolin, B., Dickinson, R., Goody, R., Leith, C., Stommel, H., and Wunsch, C.: Carbon Dioxide and Climate: A Scientific Assessment, Natl. Acad. Sci. Press, Washington, DC, 33p, 1979.
[2] Understanding Climatic Change, National Academy of Sciences, Washington, 239 pp., ISBN 0-309-02323-8.

[3] I sent the first draft of the paper to 30 of the most relevant experts. Charney had a positive reaction and said that the paper was a good complement to his report. In my reply to Charney I asked him to put in a word with Abelson.
[4] Hansen, J., D. Johnson, A. Lacis, S. Lebedeff, P. Lee, D. Rind, and G. Russell: Climate impact of increasing atmospheric carbon dioxide. *Science*, **213**, 957-966, 1981.
[5] Pat never got the Nobel, even though one was given for satellite measurements of the cosmic microwave background radiation, a project that was hatched in Pat's office with Pat as an intellectual leader. Pat arguably could have been given a Nobel for his remarkable work in mapping the Milky Way with his small microwave telescope.