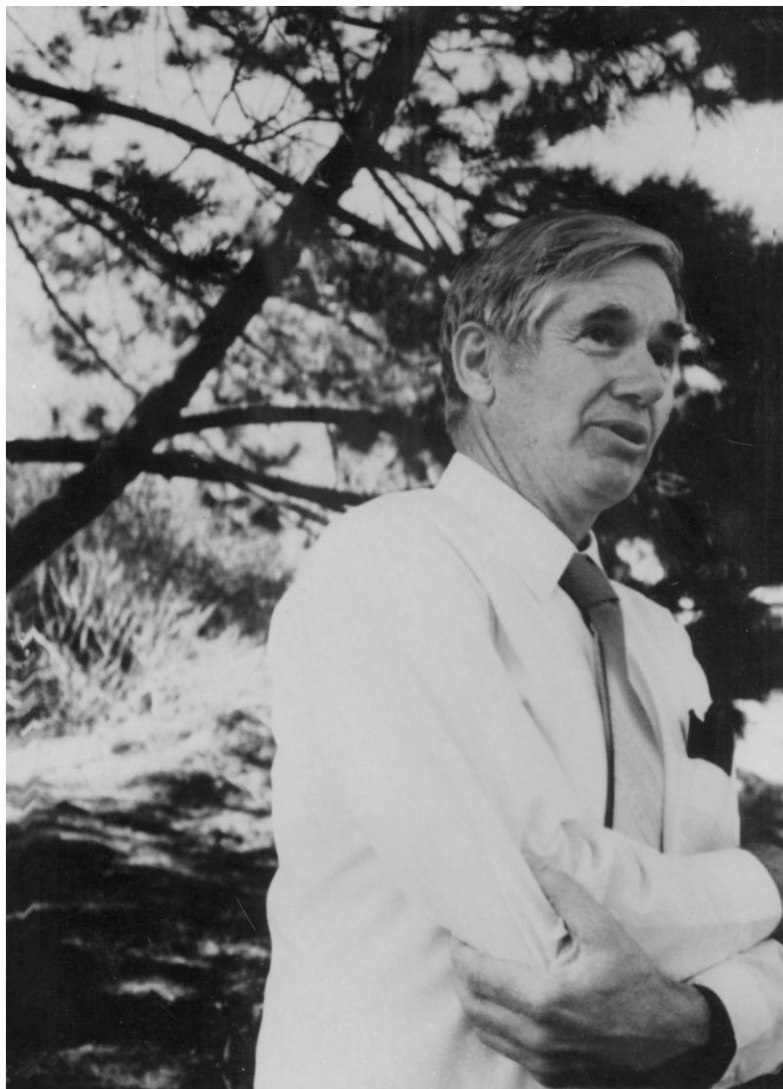


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*Charles D. Ideling*

# REWARDS AND PENALTIES OF MONITORING THE EARTH

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## ABSTRACT

When I began my professional career, the pursuit of science was in a transition from a pursuit by individuals motivated by personal curiosity to a worldwide enterprise with powerful strategic and materialistic purposes. The studies of the Earth's environment that I have engaged in for over forty years, and describe in this essay, could not have been realized by the old kind of science. Associated with the new kind of science, however, was a loss of ease to pursue, unfettered, one's personal approaches to scientific discovery. Human society, embracing science for its tangible benefits, inevitably has grown dependent on scientific discoveries. It now seeks direct deliverable results, often on a timetable, as compensation for public sponsorship. Perhaps my experience in studying the Earth, initially with few restrictions and later with increasingly sophisticated interaction with government sponsors and various planning committees, will provide a perspective on this great transition from science being primarily an intellectual pastime of private persons to its present status as a major contributor to the quality of human life and the prosperity of nations.

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## PROLOGUE

At editorial request, the following sketch is focused on a particular aspect of my career: my desire to measure atmospheric carbon dioxide. For much of my professional career, this desire met with heavy opposition from certain agencies of the US Government that wanted such measurements to be managed principally, or even solely, as in-house programs of the federal bureaucracy. I have attempted to intertwine the portrayal of this struggle with a narrative of the concurrent gain in knowledge from my measurements which repeatedly helped me to argue for their continuance.

As biographical background, I begin with a general account of my childhood and school years, followed by how I first became involved in measuring carbon dioxide. With the beginning of the disputes with the agencies, most of what follows bears, however, specifically on my studies of atmospheric carbon dioxide, although throughout my career at the Scripps Institution of Oceanography I also pursued studies of the carbon cycle in the oceans. Near the end I remark on some of the consequences of the inexorable increase in carbon dioxide in the air, which I have witnessed first hand for over 40 years.

## 1928–1953: GROWING UP WITH SCIENCE AND THE ARTS

### *Parents*

My parents came from very different backgrounds. My father was raised on the western fringe of the Bitterroot Valley in western Montana, on a 160-acre

homestead. After completing the eighth grade in a two-room school house, he went to work as a farm hand and lumber jack. In contrast, my mother grew up near Boston in a family that cherished tracing its ancestry back to the American Revolution.

Shortly after my father finished grammar school his mother died. Soon after that, his father contracted Rocky Mountain spotted fever and almost died. The farm was abandoned. Moving with his father and two sisters to the West Coast just as the First World War was ending, he worked in a Seattle shipyard and then joined the Merchant Marine. On his first cruise he aspired to become an officer. Told that this required a high-school education, he returned to Seattle, completed high school in 3 years, and won a scholarship to Yale University. Lacking a knowledge of Latin, necessary for earning a Bachelor of Arts degree, he was awarded the rarely bestowed degree of Bachelor of Philosophy when he graduated in 1927.

At Yale he met my mother, who had become a graduate student in English Literature—rare for a woman at that school in those times. Marriage terminated her graduate work. I was born in 1928. Two years later the family, now including a baby sister, moved to Illinois where my childhood was spent in a succession of Chicago suburbs.

My father was profoundly influenced by his education at Yale. His childhood beliefs, strongly conditioned by teachings from the Bible, were repeatedly challenged by his studies in sociology and natural history. In 1929, in Chicago just before the stock market crashed, he joined an investment banking firm. During the ensuing Great Depression, although personally successful, he became distressed about the economic future of our nation. In the late 1930s he quit his work in order to study, teach, and preach banking reform, thereby plunging our family into the poverty he was distressed about. Toward the end of his relatively short life, he collaborated with economists of the University of Chicago in trying to explain the business cycle. He had become convinced that booms and busts stemmed from expansions and contractions of the money supply largely under control of the banking system.

### *Early Years*

Growing up in the midwest near Chicago, I was exposed so extensively to my father's ideas in economics and banking that I abandoned a curriculum in chemistry at the University of Illinois simply to avoid taking a required course in economics. I was nevertheless sympathetic to his ideas and to his faith that the world could be made better by devotion to just causes.

My father had a general knowledge of science and mathematics, which he made use of near the end of his life while studying the business cycle. When I was about five years old, he excited in me an interest in astronomy. In a darkened



choice. Engineering and physics, however, were taught on a different semester sequence than liberal arts. In physics I was able to take only an introductory course, taught without calculus to huge classes of freshmen. I chose to major in chemistry. I didn't particularly like chemistry and repeatedly doubted that I had made the right choice.

Soon the war was over and the draft age was raised to 19. I kept going to college.

At the beginning of my third year at Illinois, I abandoned the school's curriculum in chemistry. This was partly a result of being disenchanted with a required course on how to use the library for such boring tasks as looking up organic chemicals in a compendium called "Beilstein." The direct reason, however, was to avoid a required course in economics. I felt quite passionately that my exposure to economics at home had been enough. Refused a waiver on the grounds that chemists should all be exposed to economics, I elected to become a general liberal arts major. As an unexpected outcome, I needed fewer credits to graduate. After 7 semesters, I graduated in the spring of 1948 and had to decide what to do next. When entering the university, I had drifted into chemistry without much thought of the consequences. Now I took the easiest route again.

### *Graduate School*

My mother had grown up next door to a family named Dole. One of the sons, Malcolm, about her age, had graduated from Harvard, married, and become a professor of chemistry at Northwestern University. During my childhood, the Doles and my parents often played card games together. I attracted Dr. Dole's attention at the age of 5 or 6 by multiplying two-digit numbers, using some trick technique I had learned from my father but can today no longer recall. I probably hadn't mastered all combinations, but my demonstration evidently impressed Dr. Dole. He kept me in mind and in 1948 offered me a graduate fellowship at Northwestern. I accepted without applying to any other school.

Accepting so soon was probably a mistake. I had just turned 20 with only a minimal education in chemistry, albeit from a university department with a good reputation. My work ethic was to follow directions on a short-term basis. On my first day at Northwestern, Professor Dole asked me to clean up the chemistry laboratory where I would work. When I reported back for the next assignment, Dr. Dole dropped what he was doing and found me a new task. I donned pallbearer's gloves, provided for cleanliness, and he taught me to make weight measurements on an analytic balance. Over the days that followed, I interrupted him many times, asking for additional instructions.

After a month or so in this work mode, I received from Dole a series of journal articles to read on polymer chemistry, which I was totally unprepared

to appreciate. I tried for a long time to read the texts, but I couldn't concentrate. Dr. Dole was freed from having to give me close attention, but gradually he had deeper and deeper misgivings about his choice of a new graduate student.

For the next two years I took the prescribed graduate courses. I worked hard and passed the courses, but I allowed myself little time to accomplish anything in the laboratory to earn my fellowship. During the second year, by department rules, I was obliged to declare a "noncontiguous minor" area of study. The chemistry students usually chose a subject area as close to chemistry as possible. I saw an advantage to the flexible rule, however. I proposed astrophysics, a subject in which I had no previous instruction, but which was related to my childhood interest in astronomy. Dr. Dole surprised me by asking if I might not want to major in astrophysics.

Upon investigation, I found that Northwestern University did not offer the necessary instruction. Some months passed. One day while visiting a college friend who was still at the University of Illinois, I noticed a text on his shelf in which even the title contained a word incomprehensible to me: *Glacial Geology and the Pleistocene Epoch*, by John Foster Flint. I read into the book far enough to learn what Pleistocene meant. Back at Northwestern, I bought a copy. Reading it was pleasurable between experiments in the laboratory. I imagined climbing mountains while measuring the physical properties of glaciers. Here was a new idea for a noncontiguous minor. I proposed geology to Dr. Dole, again a subject in which I had no previous instruction.

Dr. Dole looked at me thoughtfully. "Would you perhaps like to major in geology?" he asked. I said no, I would still major in chemistry. This settled, I started taking undergraduate classes in geology, beginning with engineering aspects, because the course being offered quickly covered the fundamentals. I went on to complete most of the basic undergraduate courses, except for lengthy labs and field excursions that I couldn't find time for.

Twice I interrupted my studies during the summer to hike and climb in the glacier-decked Cascade Mountains of Washington State. I prolonged the second of these excursions by accepting an invitation to join a canoe trip in Ontario, Canada, when one of the scheduled canoeists had to drop out. Of this trip I have lasting memories of great "jack pine" forests and muskeg swamps in the rain. Dr. Dole was more than a little upset by these diversions, but I rationalize now that they were worth the price, not only for my exposure to landscapes but also because I later married a sister of the canoeist who had dropped out.

In 1953, I completed a dissertation on polymers under Dr. Dole, taking what was then the extraordinarily long period of five full years. I had also acquired a working knowledge of geology, weak in laboratory and field work, but adequate for me to consider applying for a postdoctoral fellowship in a geology department.

On the other hand, my thesis in chemistry had received national attention. The cover of an issue of *Chemical and Engineering News* in 1953 showed Dr. Dole standing beside a complicated apparatus built by another graduate student for a different project. Dole was quoted as the discoverer of a new kind of “long-range migration of chemical activity.” This was because my thesis had suggested that double bonds between carbon atoms in polyethylene moved to the ends of polymer chains when subjected to a high-energy beam of neutrons.

Although I hardly grasped it then, the opportunities for new PhDs were at nearly an all-time peak. There had been a shortage of PhD chemists ever since the recent world war. The shortage was still acute. Few chemistry majors at Northwestern investigated low-paying postdoctorals at universities. Dr. Dole’s graduates were going directly to major oil companies and the like. I was offered employment by large chemical manufacturers, most of which were located in the industrialized cities of the eastern United States. Polymer chemistry was important to industry because exciting new plastics were being developed.

There were few new PhDs versed in polymer chemistry. Moreover, I had acquired a modest understanding in another important field, nuclear chemistry, because my thesis, suggested by Dr. Dole, had involved irradiating polymers with neutrons. In more recent times it would have been risky to pass up such good job offers. To Dr. Dole it even then seemed foolhardy to do so. He told me that I shouldn’t shun a job just because it was located in the eastern United States: One could enjoy life wherever one ended up working.

I had trouble seeing the future this way. I wrote letters offering my services as a PhD chemist exclusively to geology departments west of the North American continental divide. In general, I received back polite declining letters, but I got two offers. I accepted an invitation from Professor Harrison Brown of the California Institute of Technology in Pasadena, California, where he had recently started a new department in Geochemistry. I became his first postdoctoral fellow.

## 1953–1956: A PERIOD OF GREAT OPPORTUNITY

### *California*

Southern California was enthralling even though, owing to a peculiar haze in the air, the high mountains immediately behind Pasadena remained steadfastly invisible for weeks after my arrival. I penetrated them by car on winding roads that took me into clear air and pristine pine forests. A visitor from Switzerland roomed with me. He had relatives living nearby who had built a stone cabin high in the San Bernadino mountains, accessible only by foot. We visited it after the first autumnal snowfall. Also, I soon found musicians to play ensemble music with, and I got married.

My only problem was finding a scientific research topic to pursue at Caltech. A graduate student was assigned by Professor Brown to help me. In a dark subbasement of the Caltech geology building was an ear-piercing, dust-belching rock crusher. The student needed to have a lot of rock crushed for an experiment funded by the US Atomic Energy Commission. He asked me to crush rock for two weeks to gain practical experience in geochemistry.

A grant from the Atomic Energy Commission was paying my salary. I had agreed in advance to work for the grant's principal objective—to derive nuclear power from the uranium contained in ordinary granitic rock. It was logical to ask me to learn to crush rocks, but I was very proud of my new PhD. I didn't think two weeks were needed to master the science of rock crushing.

I hesitated to get started, sitting in my new office space each succeeding day reading about geology. I began to audit geology courses. I probably made a terrible impression on some of my associates, who expected me to commence some visibly useful activity; but Professor Brown didn't express concern.

One day, while I was in his office with some others of his group, Brown illustrated the power of applying chemical principles to geology. He suggested that the amount of carbonate in surface water and near-surface ground water might be estimated by assuming the water to be in chemical equilibrium with both limestone and atmospheric carbon dioxide.

To test his idea, I decided to set up a field experiment. This would provide me with the kind of practical experience I really wanted. I could fashion chemical apparatus to function in the real environment. The work could take place outdoors.

### *I Find A Research Project*

With Professor Brown's consent, I postponed the study of uranium in granite and set about building a device to equilibrate water with a closed air supply. I acquired a hand-operated piston pump. Through a nozzle it could spray water from a natural source onto the wall of the glass chamber to bring about a thermodynamic equilibrium between the carbon dioxide ( $\text{CO}_2$ ) dissolved in the stream of water and gaseous  $\text{CO}_2$  in the chamber. The water then exited through a drain, and I stored the equilibrated air in the chamber, which was exchanged for another in the next sampling exercise. Also, I saved a separate portion of the sampled water. This I acidified back at Caltech to convert the dissolved bicarbonate and carbonate salts to  $\text{CO}_2$  gas. I built a vacuum extraction system to isolate, in a cold trap, the  $\text{CO}_2$  gas from each sample of air and acidified water. As a refrigerant I used liquid nitrogen, which had recently become available commercially.

To measure the precise amounts of  $\text{CO}_2$  trapped by the liquid nitrogen some kind of gas manometer was needed. Searching about, I found a 1916 journal

article describing a device that confined a space above a column of mercury, the latter held at a fixed height by gas pressure, which could be adjusted and measured precisely. I modernized the design and engaged a glass blower to construct the instrument from drawings. This constant-volume manometer performed to an imprecision of about one thousandth of the measurement, as well as or better than any other available procedure for measuring CO<sub>2</sub> in air or carbonate in water.

The equilibrator seemed to work when I tested it out on a tiny creek near Pasadena. The setting was not very natural, however, and the measurements didn't appear to be very valuable. Permission was granted for me to go up the California coast to a state park near Monterey, where the Big Sur River flowed out of a mountain wilderness into the Pacific ocean. The area contained calcitic rocks where the ground water would presumably be in good contact with limestone.

Before going to Big Sur I began to worry, however, about assuming a specified concentration for CO<sub>2</sub> in the air based on a search of the scientific literature. This concentration had to be known precisely for comparison with the CO<sub>2</sub> measured in the equilibrator. Published values of atmospheric CO<sub>2</sub> concentration varied widely. After finding little guidance beyond an oft-repeated statement that the concentration was about 0.03% of the content of air, I decided to make direct measurements. To do so I had a dozen 5-liter glass flasks constructed, each closed off with a stopcock to hold a good vacuum. I weighed them empty and filled them with water to determine their volumes. As a rehearsal for field studies, I collected sequences of air samples on the roof of the geology building at Caltech. I extracted the CO<sub>2</sub> with my vacuum line, measured its amount with my new manometer, and calculated its concentration in each sample.

The concentrations that I found varied significantly. It was obvious that Pasadena's air was often affected by CO<sub>2</sub> emissions from industry, car exhaust, and backyard incinerators. Further measurements in the city seemed unproductive. I turned my attention to sampling air and water in Big Sur State Park.

It was a full day's drive to Big Sur. Not being sure that the CO<sub>2</sub> even in pristine air next to the Pacific Ocean would be constant, I decided to take air samples every few hours over a full day and night, as well as a series of water samples.

Why did I devise such an elaborate sampling strategy when my experiment didn't really require it? The reason was simply that I was having fun. I liked designing and assembling equipment. I didn't feel under any pressure to produce a final result in a short time. It didn't occur to me that my activities and progress might soon have to be justified to the sponsoring Atomic Energy Commission. At the age of 27, the prospect of spending more time at Big Sur State Park to

take suites of air and water samples instead of just a few didn't seem objectionable, even if I had to get out of a sleeping bag several times in the night. I saw myself carving out a new career in geochemistry.

I did not anticipate that the procedures established in this first experiment would be the basis for much of the research that I would pursue over the next forty-odd years. Nor did I give much thought to the consequences of following up on a proposal of Professor Brown's close associate Dr. Sam Epstein. Epstein was carrying out new isotopic studies in geochemistry, following the lead of Dr. Harold Urey at the University of Chicago, where Epstein had earned his PhD. Urey's former chief engineer, Mr. Charles McKinney, had been hired at Caltech and had built a mass spectrometer to measure the carbon and oxygen isotopic ratios of CO<sub>2</sub>. Epstein urged me to save the CO<sub>2</sub> samples that I was gathering from air and water and measure their isotopic ratios. I took his suggestion. This turned out to be an important addition to my study, although it had no direct bearing on testing Professor Brown's original hypothesis of a carbonate equilibrium in water.

Contrary to Brown's hypothesis, river and ground waters at Big Sur and several other sites typically bore a higher pressure of CO<sub>2</sub> than the equilibrium value for the air. CO<sub>2</sub> levels were so high in water issuing from a cold spring in limestone near Big Sur that much of it was evidently coming from oxidized organic matter.

I soon focused my attention, however, on obtaining more measurements of CO<sub>2</sub> in air, because these data showed an intriguing diurnal pattern. The air contained more CO<sub>2</sub> at night than during the day. Also, the heavier carbon-13 isotope of the CO<sub>2</sub> at night was depleted with respect to the lighter carbon-12 isotope, as though the CO<sub>2</sub> that caused the nighttime rise had been released by the plants and soil. The degree of depletion of carbon-13 for a given rise in CO<sub>2</sub> concentration varied from site to site in a manner suggesting that the plants during daytime at some sites reabsorbed CO<sub>2</sub> previously released into the air locally the night before. To understand these findings I began to read the literature bearing on plant growth and on the meteorological condition of the air near the ground at night.

The diurnal patterns were similar everywhere I went, from the rain forests of the Olympic peninsula near Canada to the high mountain forests of Arizona near Mexico. (US National Forests at that time had large tracts of land not yet disturbed by logging.) Moreover, the air in the afternoon seemed always to have nearly the same amount of CO<sub>2</sub>, about 310 parts per million (ppm) of air, after correcting for water vapor. The concentrations were highly variable at night and always higher than in the afternoon. Also, the carbon isotopic ratios in the afternoon were all about the same, though systematically variable with concentration at night.

The scientific literature didn't suggest that daytime concentrations should be so similar from place to place. A recently published book on geochemistry (53) indicated that arctic air could contain as little as 150 ppm, tropical air as much as 350 ppm. Moreover, photosynthesis by plants in the area of my sampling should have drawn CO<sub>2</sub> down during the day, making the concentration lower than in air over bare ground. I broadened my study by sampling on a high mountain during strong winds over barren ground. Also, Professor Norris Rakestraw, a marine chemist at the Scripps Institution of Oceanography in San Diego, California, kindly took samples for me over tropical waters of the east Pacific Ocean.

Even at these places, sampled in the free atmosphere, the concentrations and carbon isotopic ratios were nearly the same as in the afternoon near vegetation (30, 32). Why didn't photosynthesis, which takes CO<sub>2</sub> out of the air during the day, cause low and variable concentrations when respiration by plants and soil, which puts CO<sub>2</sub> into the air at night, causes high and variable concentrations? I found an explanation in a book that attracted my attention because of its apt title: *The Climate Near the Ground* (21). All of my forest measurements had been made during fair weather. On such days heating by the Sun typically induces enough turbulence in air near plants to cause thorough mixing of this air with the free atmosphere by early afternoon. Where I had sampled, the free air evidently had been of nearly constant composition with respect to CO<sub>2</sub>. In contrast, during the nighttime the air near the ground cooled, forming a stable layer that allowed CO<sub>2</sub> from respiration to build up within the forest canopy.

The highly variable literature values for CO<sub>2</sub> in the free atmosphere were evidently not correct.<sup>1</sup> Rather, a concentration of 310 ppm of CO<sub>2</sub> appeared to prevail over large regions of the northern hemisphere. I had detected this near-constancy under the implausible circumstances of studying air in old-growth forests where variability was to be expected. By 1956 my broader findings of surprising near-constancy seemed to me secure enough to communicate them to others, including an employee of the US Weather Bureau. Meanwhile, I gave up the study of river and ground water that had first led me to measuring atmospheric CO<sub>2</sub>.

## 1956–1963: IMPLEMENTING A GLOBAL PROGRAM

### *The International Geophysical Year*

In 1956, the US Weather Bureau was a decidedly lean federal agency, dedicated to forecasting the next day's weather. Nevertheless it included a Division of Meteorological Research guided by an energetic director, Dr. Harry Wexler. Wexler had deployed an ozone scientist, Dr. Oliver Wulf, to Caltech. I spoke

<sup>1</sup>I believe now that some of the late nineteenth century data were nearly correct (20).

to Wulf about my measurements of atmospheric CO<sub>2</sub>, and he told his boss about them. Wexler invited me to Washington. There I showed him my data suggesting that the amount of CO<sub>2</sub> in the open atmosphere might be far less variable than was generally believed.

My trip to Washington was by air, my first-ever ride in an airplane. Wexler's haunts were a suite of crowded offices at the old Weather Bureau quarters at 24th and M Streets. Wexler seemed to be in a hurry, with many things on his mind. He quickly accepted my new data as relevant to a major Weather Bureau project recently formulated as part of a worldwide program identified as the International Geophysical Year, IGY for short. The Weather Bureau was already planning to measure atmospheric CO<sub>2</sub> at remote locations during the IGY. Wexler showed a particular interest in my having successfully sampled air on a high mountain and suggested an exciting prospect: sampling at a new meteorological observatory that had been built by the Bureau the year before on the Island of Hawaii. The site was on a slope near the top of a 13,000-foot-high volcano called Mauna Loa. Wexler had taken a personal interest in getting this observatory built, and was keen on its being utilized.

I expressed to him a concern that conventional measuring of CO<sub>2</sub> during the IGY might just lead to more data of the kind that I believed to be unreliable. He very quickly seemed convinced, and we talked about beginning a new kind of program. I had briefly investigated some commercially available instruments that could measure atmospheric CO<sub>2</sub> continuously. These devices detected infrared radiation from a glowing coil of wire after the radiation passed through a cell in which a stream of air flowed. A radiation detector at the other end of the cell determined how much CO<sub>2</sub> was in the air stream. Perhaps several of these infrared gas analyzers could be placed strategically around the world. I proposed that my new manometric technique could be used to calibrate them precisely. Also, samples of air could be collected in 5-liter glass flasks at additional locations and returned to a laboratory to be measured by one of these instruments. Flask samples would furthermore provide much wider coverage, since continuous gas analyzers would be difficult and expensive to operate at more than a few remote locations.

Without hesitation, Wexler supported continuous CO<sub>2</sub> measurements at Mauna Loa and at Little America, the latter Admiral Bird's famous Antarctic station of the 1930s, to be reoccupied during the IGY. After less than an hour had past, he directed me to files of Weather Bureau stations, so that I could tentatively choose additional sites suitable for flask sampling. He also sent me to Dr. Sigmund Fritz of the US Air Force to discuss sampling air on routine reconnaissance flights of the Air Force's Air Weather Service. The next day he saw me again briefly and asked me whether I would like to come to Washington to carry out this program.

I was escorted to where I might work. There was no available space at the crowded 24th and M Streets offices. I was sent to a dim basement of the Naval Observatory where the only activity seemed to be a cloud-seeding study being carried out by a solitary scientist named Ross Gunn. I had misgivings about the prospect for adequate resources to start a project in such a place.

Meanwhile, Norris Rakestraw, who had collected air samples for me over the Pacific Ocean, brought my atmospheric CO<sub>2</sub> data to the attention of Dr. Roger Revelle, Director of the Scripps Institution of Oceanography, situated on the Pacific coast near San Diego, California, a two-hour's drive by automobile south of Caltech. Revelle invited me to visit Scripps with the prospect of a job. I was given lunch in the back yard of the residence of his associate, Professor Hans Suess, in brilliant sunshine wafted by a gentle sea breeze.

Rakestraw could give me a small room at Scripps. After briefly considering the alternative of a basement room in Washington, I agreed to come. Wexler was cordial about this arrangement, agreeing to donate a substantial sum of Weather Bureau IGY money to supplement funds that Revelle said could be provided to pay my salary and a little bit more.

I had taken a gamble in advocating continuous measurements of atmospheric CO<sub>2</sub> to both Revelle and Wexler. For only a couple of days at Caltech had I tested a continuous gas analyzer that might accomplish this. Several companies were marketing such analyzers, developed for military purposes during World War II. None were designed specifically to measure CO<sub>2</sub> in ordinary air. The manufacturers could not say how well they would work at such low CO<sub>2</sub> concentrations. I had tested a model manufactured by the Applied Physics Corporation (APC), the only company in which I was able to get past a salesman and talk directly with an engineer. The APC analyzer, manufactured conveniently close to Caltech, consisted of a thermostated cell, an optical system, and an electronic amplifier, all in a large heavy-metal case. By itself it cost the very large sum of \$6000. It would cost still more to construct a gas handling system for it and to provide it with calibrated reference gases and an electric power supply for use in remote areas with poor electrical generation. Only after one was purchased with these accessories and tested under field conditions could I be sure how well such instruments would work.

The International Geophysical Year, in spite of its name, was scheduled to run for 18 months, beginning in July, 1957. I moved to Scripps from Caltech in August, 1956, less than a year before the program was scheduled to begin. Its chemistry component, within the Weather Bureau, was not closely related to the IGY's primary mission to study the geophysical effects of an anticipated maximum in the solar sunspot cycle. Although I didn't know it when I showed up at 24th and M Streets, no one there really knew yet how to spend all the funds awarded the Weather Bureau for atmospheric chemistry. My proposal to

Wexler to buy expensive new instruments helped him to allocate some of this money.

### *First Results*

As one might have expected, some members of the US IGY steering committee were not immediately persuaded that it was worthwhile to acquire expensive equipment to achieve high accuracy in atmospheric CO<sub>2</sub> measurements, when all the published data pointed to high variability. The distinguished Swedish meteorologist Dr. Karl Gustav Rossby, a strong advocate of CO<sub>2</sub> studies, contrasted my grandiose proposal to the traditional approach of making chemical measurements of CO<sub>2</sub>, adopted in Scandinavia as a prelude to the IGY. Indeed, data already obtained by the traditional method showed a high degree of variability, which Rossby's researchers ascribed to variable origins of the air masses (18). Indeed, Rossby's interest in atmospheric CO<sub>2</sub> lay largely in using it to tag air masses. When he met me during a visit to Scripps, he dubbed me "the man with the machine" (37). Others worried about possible interference from volcanic gases at Mauna Loa Observatory because it was situated on an active volcano. Harry Wexler made a considerable effort to ward off these criticisms (37).

Given the short time before the IGY was to begin, I was allowed right away to purchase four continuous gas analyzer systems. One system each was needed to measure CO<sub>2</sub> continuously at Little America and Mauna Loa. A third was bought because Revelle wanted one installed on a ship. A fourth was needed in our laboratory to cross-calibrate reference gases and replace the slow direct manometric procedures for analyzing flasks that I had used at Caltech.

The first APC analyzers didn't arrive till November. Assembly of the first system in time to be placed on the last ship to go to Antarctica in 1956 was barely possible. Fortunately this ship was berthed in San Diego. A graduate student and I worked into Christmas eve to get all of our equipment for Little America Station on board. The effort to get ready, however, had been too hasty. The air pumps, copied from those used at Little America for ozone measurements, failed to deliver pure air. A year went by before suitable replacements arrived in Antarctica. Flask samples, however, were deployed to the South Pole. In early 1957 these provided the earliest precise time-series measurements of atmospheric CO<sub>2</sub> at a remote location (19). In 1957, because Little America was being shut down, our continuous analyzer system was moved to the South Pole, where it remained until 1963.

Soon after the Antarctic equipment had been sent off in 1956, Wexler, to hasten progress, sent Dr. Gene Wilkens, a full-time Weather Bureau employee, to Scripps to help me. Meanwhile, Revelle was anxious that the analyzer designed to measure CO<sub>2</sub> at sea, be ready in time for an IGY cruise in the fall

of 1957. It was to be used to measure  $\text{CO}_2$  in the water as well as in the air, a logical extension of my equilibrator experiment at Caltech. The equilibrator system, however, now had to be partially automated for continuous operation. Prior to the main expedition at sea we ran test cruises, including a long cruise to Alaska and back.

Revelle was also anxious for the planned aircraft sampling program to start. The complexities of working with military aircraft made progress slow, even with much help from Wilkens. Revelle insisted that this aircraft project take priority over starting up measurements at Mauna Loa, because he believed that the main objective of my program should be to gain a “snapshot” of  $\text{CO}_2$  around the world. He still held to the prevailing belief that the  $\text{CO}_2$  concentration in air was spatially variable and that therefore sampling must be widespread to establish a reliable global average during the IGY. Measurements should be repeated, in say, 20 years to see whether the global concentration had noticeably changed.

Averages of the highly variable published data for the twentieth century had been scrutinized by GS Callendar, an English engineer (12). These data indicated that  $\text{CO}_2$  in air had been increasing. The rate of rise could be accounted for if  $\text{CO}_2$  emitted by burning coal, natural gas, and petroleum had all remained in the air. Revelle distrusted these data because he thought that the oceans must be absorbing somewhere near half of this  $\text{CO}_2$  from “fossil fuel.” Comparing two carefully documented snapshots would provide a reliable estimate of oceanic  $\text{CO}_2$  uptake, given that enough time elapsed to determine precisely the fraction of emissions observed to be staying in the air.

In the fall of 1957, with the IGY under way, I became concerned about the delay in establishing measurements at Mauna Loa Observatory. The equipment was ready for installation, but Revelle was reluctant to sign travel orders for me to go to Hawaii because the aircraft program was not yet operational. Then, in November, Mr. Ben Harlan, who had operated the  $\text{CO}_2$  equipment at Little America, conveniently returned to the United States. To prevent further delay Wexler enlisted him to install the APC analyzer system on Mauna Loa. This he did in March, 1958, with the help of Jack Pales, a recently arrived physicist-in-charge of the observatory. In advance, I gave advice including a suggestion of what  $\text{CO}_2$  concentration to expect if the equipment worked correctly. By assuming that the concentration there would match closely with test measurements of presumably unpolluted air made shortly before at the Scripps pier, I told Harlan to expect 313 ppm.<sup>2</sup> As I recalled (37):

To our great surprise, on the first day of operation it delivered within one ppm the  $\text{CO}_2$  concentration that I had told Harlan to expect on the basis of my earlier manometric data and preliminary test data obtained at Scripps. Of course this

<sup>2</sup>A provisional value later adjusted (35).

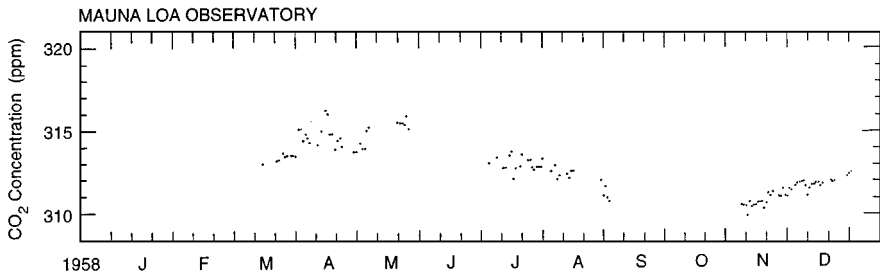


Figure 1 The daily average concentration of atmospheric CO<sub>2</sub> at Mauna Loa Observatory for the calendar year 1958, shown as dots. (Source: Ref. 51)

agreement was an accident. The mean of the daytime manometric and Scripps data just happened to be close to the value typical for the month of March. Indeed, the next month's data did not agree—the concentration rose by over one ppm. The following month's mean concentration was still higher. Electrical power failures then shut down the equipment for several weeks. When measuring resumed in July, the concentration had fallen below the March value. I became anxious that the concentration was going to be hopelessly erratic, especially when the computed concentration fell again in late August. Then there were more power shutdowns.

Finally, in November I was allowed to visit Mauna Loa and restart the analyzer. As new data emerged without further interruption, the concentration rose steadily (Figure 1). Then in May it started to decline (Figure 2). A regular seasonal pattern began to emerge, but it differed markedly from earlier published northern hemisphere data in which the maximum concentration was typically in January, a month when CO<sub>2</sub> from burning is likely to accumulate in the air near the ground because of temperature inversions (9). The maximum concentration at Mauna Loa occurred just before plants in temperate and boreal regions put on new leaves. At Mauna Loa the regular seasonal pattern almost

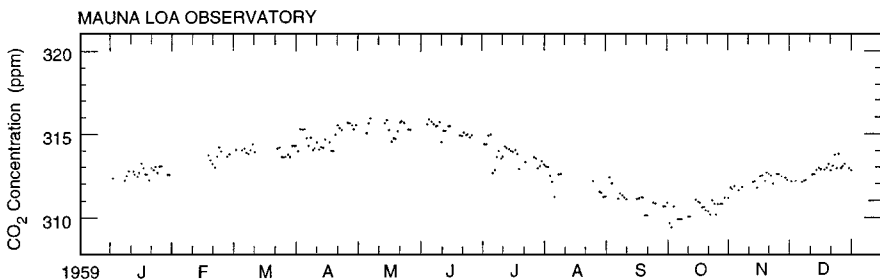


Figure 2 Same as in Figure 1, but for 1959. (Source: Ref. 51)

exactly repeated itself during the second year of measurements (Figure 2). We were witnessing for the first time nature's withdrawing CO<sub>2</sub> from the air for plant growth during the summer and returning it each succeeding winter.

In April, 1958, aircraft sampling came into operation, but I still lacked manometric calibrations. The constant-volume manometer that I had built at Caltech had remained there for possible further use in other projects. I was determined to build a more elaborate manometer which allowed very precise determinations of CO<sub>2</sub> over a much wider range of concentrations by having a larger series of fixed volumes, all controlled by columns of mercury. Assembly work went slowly; it took several months to calibrate the fixed volumes using a precise cathetometer to measure the mercury heights. Not until the spring of 1959, after the IGY was over, was it possible for me to determine the concentrations of CO<sub>2</sub> in the reference gases employed to calibrate the gas analyzers. I made just enough manometric measurements of reference gases to report the field data at a meeting of the International Union of Geodesy and Geophysics (IUGG) in Helsinki in August, 1960. The manometer, designed to yield measurements of CO<sub>2</sub> to an imprecision of one ten-thousandth of the measurement, performed at about one part in four thousand, yielding atmospheric CO<sub>2</sub> measurements precise to 0.1 ppm (22).

At the IUGG meeting there were also presentations of CO<sub>2</sub> data obtained by the chemical methods, and an honorary address by Dr. Kurt Buch, who had championed atmospheric CO<sub>2</sub> measurements in Finland as early as 1920. Atmospheric CO<sub>2</sub> measurements at an array of stations over Scandinavia, reported routinely since 1955 in a new journal, *Tellus*, were presented.

This Scandinavian program, started by Rossby in 1954, had been a major factor in triggering interest in measuring CO<sub>2</sub> during the IGY. Nevertheless it was quietly abandoned after the meeting, when the reported range in concentrations, 150–450 ppm, was seen to reflect large errors.<sup>3</sup> Rejected along with the Scandinavian sampling program was Rossby's hypothesis that CO<sub>2</sub> concentration data could be useful to tag air masses (14).

I don't know how my data were received in Helsinki. The termination of Scandinavian CO<sub>2</sub> data reporting was never explained in *Tellus*. Probably the main reaction was surprise.

The Scripps CO<sub>2</sub> data through March, 1960 appeared in the geophysics journal *Tellus* the following June, prior to the IUGG meeting (31). The article was the shortest I ever wrote. The text, under 1200 words, described the main findings: a distinct seasonal cycle of CO<sub>2</sub> concentration in the northern hemisphere

<sup>3</sup>At two stations in Finland, samples collected by station personnel had been sent to Scripps. These samples yielded nearly the same concentrations as those measured at Mauna Loa Observatory, proving that the errors in the Scandinavian program were mainly analytical rather than due to variable CO<sub>2</sub> in the air being sampled.

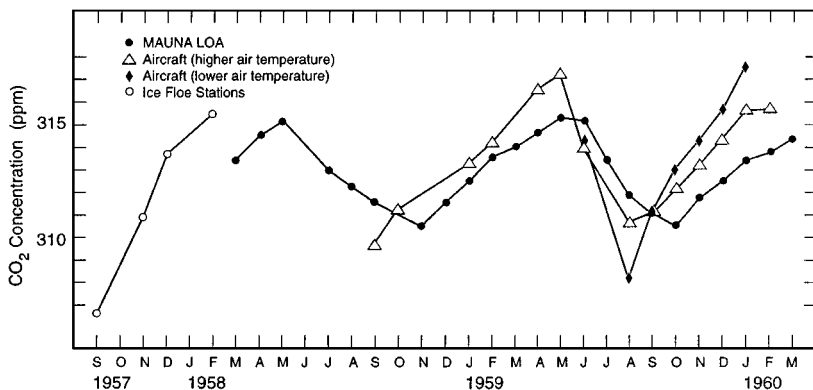


Figure 3 Variation in concentration of atmospheric CO<sub>2</sub> in the Northern Hemisphere. (Source: Ref. 31)

(Figure 3) that diminishes southward, and possibly a worldwide rise in CO<sub>2</sub> from year to year. Because I could by then, in retrospect, see a seasonal variation in the carbon isotopic ratios of CO<sub>2</sub> in my earlier afternoon data from Caltech, I proposed that the activity of plants growing on land was the cause of the seasonal cycle. This activity explained why maximum CO<sub>2</sub> concentrations in both hemispheres were observed in the spring, when most plants begin to grow. The observed year by year rise in concentration was close to that expected if all of the industrial CO<sub>2</sub> from combustion of fossil fuels remained in the air. Aware, however, of Revelle's conviction that the oceans must be absorbing some of that CO<sub>2</sub>, I noted that longer records might cause a revision in the estimated rise. This was a good judgment call. In the 1970s, with much longer records of CO<sub>2</sub>, a coworker, Robert Bacastow, discovered that a transient release of CO<sub>2</sub> from natural sources, associated with a powerful El Niño event in 1958, had exaggerated the average rise in these early data.

### *Mission Accomplished*

The Scripps CO<sub>2</sub> program continued undiminished through 1962, supported by an extension of funding from the National Science Foundation. In addition to continuous measurements at Mauna Loa and at the South Pole, and repetitive flask sampling with aircraft, measurements were made on suites of flask samples collected on ships. The Weather Bureau continued to supply me with a technician and took care of operating the continuous gas analyzers at the South Pole and Mauna Loa Observatory. The CO<sub>2</sub> data, registered on continuous strip charts, were worked up at the sites and on board ship by scaling distances between pen-traces of reference gas and air levels of CO<sub>2</sub> by hand. The distances

were averaged, using adding machines, and then converted to concentration differences by comparison with additional reference gases. The immediate result of my program was to realize the “snapshot” of CO<sub>2</sub> from the arctic to the South Pole that Revelle had wished for as part of the IGY. Ironically, most of the data had been gathered after the IGY was over.

Even before the IGY, Rossby had been warned by doctors that he was working too hard and should take a rest. He paid the ultimate price of not heeding their advice, dying suddenly of a heart attack in 1957 (6). The Institute of Meteorology in Stockholm, which he had founded, was left without an obvious successor. Dr. Bert Bolin, about my age, took over the directorship. He was my host when I chanced to visit the institute after the IUGG meeting in Helsinki, and he invited me to spend a year there. He had been trained in the new profession of numerical weather forecasting but took an interest in the chemistry program, started by Rossby. In the summer of 1961, with permission from NSF to take a year’s leave of absence from Scripps, I went to Stockholm with my wife and three children and began to assess the significance of the “snapshot” data envisioned by Revelle. Mr. Tom Harris, who had replaced Gene Wilkens, on loan to the program from the US Weather Bureau, kept the field program going at Scripps while I was away.

Freed in Stockholm from the daily routine of running a field program, I assembled all of the Scripps CO<sub>2</sub> data. A long-term increase in concentration was present in the data at every location, most clearly seen at Mauna Loa Observatory (Figure 4). The data showed a distinct seasonal cycle in the northern hemisphere (Figure 5), a weaker seasonal cycle in the southern hemisphere. These cycles and spatial gradients in the data seemed to reflect natural sources

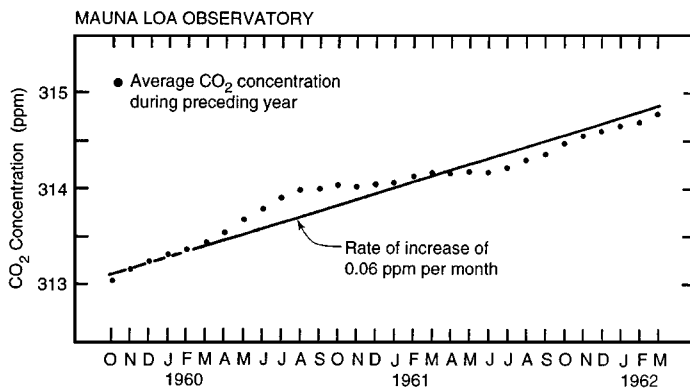


Figure 4 Annual average concentration of atmospheric CO<sub>2</sub> at Mauna Loa Observatory, Hawaii. (Source: Ref. 8)

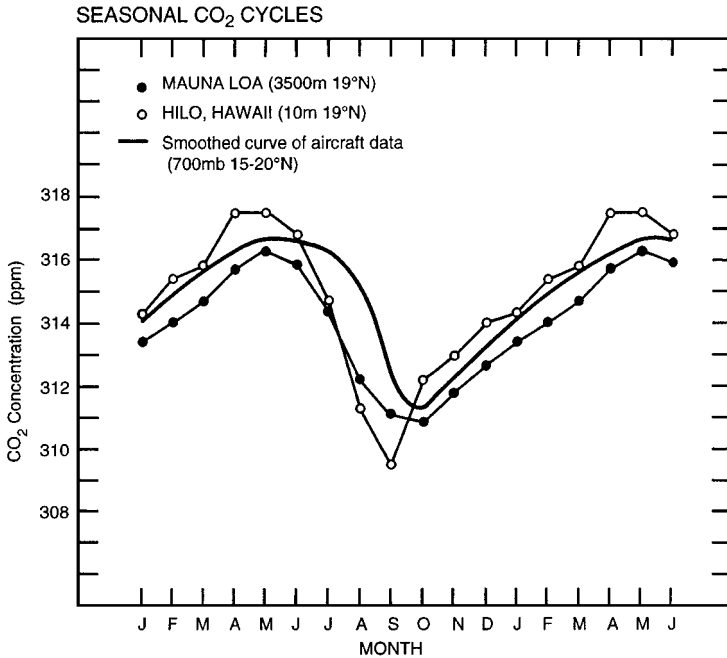


Figure 5 The concentration of atmospheric CO<sub>2</sub> at various altitudes in the northern tropics as a function of the month of the year. January through June (months 1 to 6) are plotted twice to reveal the seasonal pattern more fully. (Source: Ref. 8)

and sinks of atmospheric CO<sub>2</sub>, produced by the oceans and terrestrial vegetation, as well as CO<sub>2</sub> from combustion of fossil fuels.

Bolin estimated these presumed sources and sinks using a set of mathematical functions called Legendre polynomials to characterize the north-south gradients in the mean annual CO<sub>2</sub> concentration and in the amplitudes of the seasonal cycles seen in the data. He applied this technique the next summer while residing on a family farm in central Sweden. It is difficult to imagine in today's environment of fast digital computers how he did this analysis. Relying on his training in numerical forecasting, he copied by hand, to two-figure accuracy, the magnitudes of the first four Legendre polynomials for different latitude bands and then fitted the CO<sub>2</sub> data to these polynomials. Subsequently, also by hand, he computed the second derivatives of these polynomials and the resulting estimates of the inferred sources and sinks of atmospheric CO<sub>2</sub>. We reported our study at a meeting on atmospheric chemistry in Utrecht in August, 1962.

While in Utrecht, I learned that Harry Wexler had suddenly died of a heart attack, probably for reasons similar to those that had contributed to Rossby's

death. It was very sad news, which I found out about from his close friend in the Weather Bureau, Dr. Lester Machta.

My study with Bolin was reported more completely the next summer in Berkeley, California, at an IUGG meeting. By using only the first four Legendre polynomials in our analysis, Bolin and I focused our analysis on estimating average CO<sub>2</sub> fluxes between the northern and southern hemispheres, between each pole and the equator, and between middle latitudes and the rest of each hemisphere. Our analysis was more detailed than an earlier attempt to explain the Scripps data by Christian Junge (28), but our computed fluxes, in retrospect, were too large. We had characterized the global atmospheric circulation as being caused by eddy diffusion. The intensity, assumed to be of the same magnitude everywhere, was estimated indirectly by supposing that a peak in CO<sub>2</sub> in the middle of the northern hemisphere was due solely to the release of industrial CO<sub>2</sub> from fossil fuel. This approach wrongly assumed that the CO<sub>2</sub> concentration would be the same in both hemispheres in the absence of fuel combustion. Although the mechanisms are still not well understood even as I write, some process causes the concentration of CO<sub>2</sub> in the northern hemisphere to be lower than expected in comparison to CO<sub>2</sub> in the southern hemisphere (52). Our results could not be seriously challenged, however, until digital computers, atmospheric circulation models, and considerably more CO<sub>2</sub> data all became available two decades later.

During 1963, the first signs appeared that unfettered support of my studies of atmospheric CO<sub>2</sub> wouldn't be sustained indefinitely by the United States government. The US Congress mandated agency budget cuts for 1964 that seriously affected any Weather Bureau studies not directly related to weather forecasting. Mauna Loa Observatory was threatened with closure. Harry Wexler was not alive to defend it. Lester Machta argued strenuously for its survival, using the CO<sub>2</sub> program as one of the arguments to prevent its closure. It was saved, but the staff was drastically reduced from eight to three, including the transfer, without replacement, of Jack Pales, the physicist-in-charge. Our equipment, at the first glitch in performance, was shut down by the remaining station personnel. Even earlier, in August, 1963, my assistant from the Weather Bureau, Tom Harris, had been reassigned and left my program. I ordered the continuous analyzer from the South Pole returned to Scripps but, without Harris' help, I did not attempt to send flasks to the South Pole for the next field season. The ship and aircraft programs were also ended. Suddenly there were no precise measurements being made of atmospheric CO<sub>2</sub> anywhere.

Furthermore, I had postponed investigating possible systematic errors that might arise from replacing direct manometric assays of flask samples in the manner of my Caltech study with infrared gas analyzer data obtained by comparing air with manometrically calibrated reference gases. I should have paid

more attention to a discussion I had had with a spectroscopist, Dr. Lewis Kaplan, who had viewed our gas analyzer setup early in the program. At the outset I had been afraid that oxidation might affect stored reference gases, and had calibration gases prepared as mixtures of nitrogen gas and CO<sub>2</sub>. Under the rush of the IGY program I had neglected to prepare at least a few special gas mixtures in which nitrogen was partially replaced by oxygen to see what difference this substitution would make. What came to be called the “carrier-gas effect” was not investigated carefully for almost another decade.

I had seen the budget cut coming early in 1963 and had tried to prevent its terminating the CO<sub>2</sub> program at Mauna Loa Observatory. I even went to Washington to plead for supplemental funding. This had no tangible effect, however, until the cessation of measurements actually occurred. The National Science Foundation then found funds to pay for an additional technician at Mauna Loa. In May, 1964, with Mr. John Chin hired for the task, CO<sub>2</sub> measurements were restored there. Through most of 1964 I had the services of a returning South Pole Observer, Mr. Craig Brown, who helped me to work up for publication the eight years’ worth of data acquired from Antarctica. This work-up showed that a flask program at the South Pole would suffice to establish a long-term record now that it was too expensive to maintain a continuous analyzer there. Ironically, the cessation of time-series studies came after Bolin and I had published enough results to show that such data were clearly valuable. I learned a lesson that environmental time-series programs have no particular priority in the funding world, even if their main value lies in maintaining long-term continuity of measurements.

## 1964–1975: THE STRUGGLE BEGINS

### *Rising Interest in Rising Carbon Dioxide*

In 1964 I became a junior faculty member of the Scripps Institution of Oceanography, which had recently become part of a new campus of the University of California. With added responsibilities, I only slowly expanded my atmospheric CO<sub>2</sub> program beyond continuing measurements at Mauna Loa and the South Pole. With the help of former field personnel, I reported the previously collected data (11, 33, 61). I also assisted Mr. John Kelley, who had set up an APC gas analyzer at Point Barrow, Alaska in 1960 and was obtaining continuous CO<sub>2</sub> data there. I was grateful to NSF for continuing my funding in spite of the fact that published results were slow to appear.

In 1966, a physicist, Mr. Arnold Bainbridge, joined my program and assumed responsibility for operating the atmospheric CO<sub>2</sub> field program. His knowledge of the emerging computer technology soon made the processing of our data easier. In 1969 he augmented the program by installing a continuous CO<sub>2</sub> gas

analyzer in his native country, New Zealand. The same year Dr. Chi Shing Wong, formerly my graduate student now working in Canada, began flask sampling for us from a weather ship at  $50^{\circ}$  N in the eastern Pacific Ocean (39). By 1970 my program was gathering  $\text{CO}_2$  data at four stations strategically spaced in both hemispheres.

Meanwhile, Mr. Walter Bischof, an engineer working at Bert Bolin's Institute in Stockholm, set up an aircraft sampling program that led to a better understanding of the vertical mixing of atmospheric  $\text{CO}_2$  (5). Lester Machta, following a meeting in 1966 which I attended, laid plans for the US Weather Bureau to begin a Bureau-run program to measure atmospheric  $\text{CO}_2$  at fixed locations.

In 1969, I spoke on invitation before the American Philosophical Society on the implications of rising atmospheric  $\text{CO}_2$ . This rise was of interest, I said, because if it persisted it was likely to inhibit the escape of heat radiating upward from the Earth's surface and bring about a warmer climate—the so-called “greenhouse effect,” although I didn't use that expression. The Mauna Loa record, as I had stated on previous occasions, showed a “cyclic pattern owing to a seasonal variation in plant activity.” Now we could clearly see that this pattern was superimposed on a rising trend (see Figure 6). A surprising feature in the second half of the record was an “apparent falling off of the slope of the trend during a period when the rate of  $\text{CO}_2$  input [from fossil fuels] was increasing.” No simple mechanism of oceanic uptake could explain this fall-off.

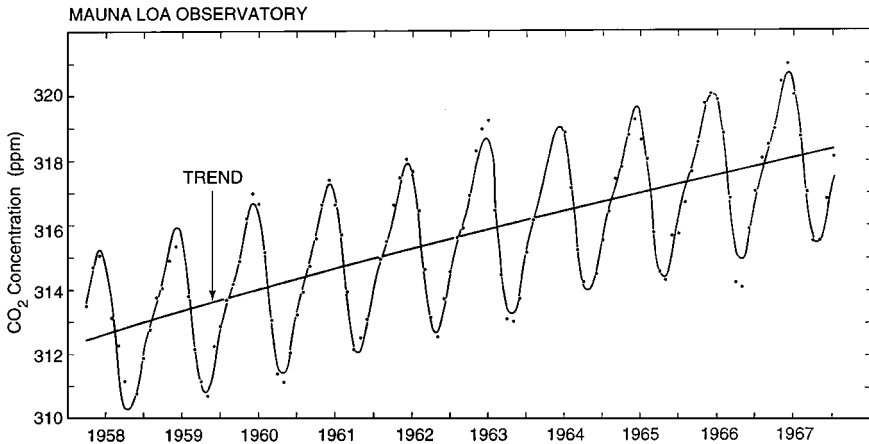


Figure 6 Long-term variations in the concentration of atmospheric  $\text{CO}_2$  at Mauna Loa Observatory, with data through 1967. The oscillatory curve is a least squares fit to monthly average concentrations, shown as dots. The curve is based on an empirical equation containing 6- and 12-month cyclic terms and a secular cubic trend function. The latter is shown separately as a slowly rising curve. (Source: Ref. 34)

Perhaps it was a result of plants on land growing more rapidly in the recent past as a result of “fertilization” caused by higher concentrations of atmospheric CO<sub>2</sub>, an unusual idea at the time.

The remainder of my talk (34) was inspired by my having helped to write a report for the President’s Science Advisory Council. Roger Revelle, the lead author of the report (57), was struck by the fact that the human race was returning to the air a significant part of the carbon that had been slowly extracted by plants and buried in sediments during a half billion years of Earth history. He thought that measurable, perhaps even marked, changes in climate might occur from an increasing greenhouse effect. He believed that careful measurements should be made to check such predictions.

Echoing Revelle’s concern before the American Philosophical Society, I too pondered the significance of returning a half a billion years’ accumulation of carbon to the air (33). I appreciated his concern because of direct personal experience, watching CO<sub>2</sub> rise from near the oft-stated background level of approximately 300 ppm (0.03%) to over 320 ppm. I wondered what the consequences of rising CO<sub>2</sub> would be in, say, 30 years:

The rise in CO<sub>2</sub> is proceeding so slowly that most of us today will, very likely, live out our lives without perceiving that a problem may exist. But CO<sub>2</sub> is just one index of man’s rising activity today. We have rising numbers of college degrees, rising steel production, rising costs of television programming and broadcasting, high rising apartments, rising numbers of marriages, relatively more rapidly rising numbers of divorces, rising employment, and rising unemployment. At the same time we have diminishing natural resources, diminishing distract-free time, diminishing farm land around cities, diminishing virgin lands in the distant country side...

[Viewed over thousands of years] I am struck by the obvious transient nature of the CO<sub>2</sub> rise. The rapid changes in all factors I [have just] mentioned, including the rapid rise in world population, are probably also transient; these changes, so familiar to us today, not only were unknown to all but the most recent of our ancestors but will be unknown to all but the most immediate of our descendants.

I noted in closing my talk that people held widely divergent views concerning a possible peril attending rising CO<sub>2</sub>, but that in 30 years “if present trends are any sign, mankind’s world, I judge, will be in greater immediate danger than it is today.”

As it happened I would have little leisure time to pursue such philosophical thoughts during the next 30 years.

### *First Signs of Trouble*

In the fall of 1969, I and my family spent a pleasant year in Germany as guests of Dr. Karl Otto Münnich of the University of Heidelberg. I found time to travel with my wife and five children, ages 2 to 14, packed together in a Volkswagen

van along with baggage, tent, and an ample supply of German apple juice and American peanut butter, as far south as the Aegean Sea of Greece and north as far as Trondheim, Norway. Although I completed no scientific projects that year, I gained an impression of European scientific research that, in my eyes, fully justified the visit.

Early in my stay, I was asked to be part of a small committee chaired by Professor Christian Junge, of the University of Mainz, to prepare recommendations for an international program to measure atmospheric CO<sub>2</sub>. This request of the World Meteorological Organization (WMO) was an opportunity for me to promote a long-term global-scale CO<sub>2</sub> program of the kind I had started during the IGY. I helped the committee to write a strong recommendation to heed Revelle's admonition that the carbon cycle should be carefully watched.

With my family I returned to the United States in the fall of 1970. The report of the Junge Committee had been accepted by WMO and was already influencing international planning and even leading to some implementation. I was surprised to learn that during my stay in Germany, the US Weather Bureau, now absorbed into a new agency called the National Oceanic and Atmospheric Administration (NOAA), had purchased a CO<sub>2</sub> gas analyzer belonging to my program without my being informed. It had been installed alongside our Applied Physics Corporation (APC) analyzer at Mauna Loa. This seemed disturbing. I had arranged the purchase of this instrument together with two others from a German company, Hartmann and Braun, shortly before the funding cut of 1964. All three analyzers had remained unused until two had been taken to New Zealand by Arnold Bainbridge, leaving one at Scripps for parallel testing with our APC analyzers. No testing had taken place, however. I now protested that the analyzer's installation at Mauna Loa was premature, and it was returned to Scripps for testing.

Bainbridge meanwhile accepted a managerial role in a new oceanographic program named GEOSECS. Mr. David Lowe, hired by Bainbridge in New Zealand, and Peter Guenther, a chemist hired to join my group in 1968, assumed responsibility for running our continuous CO<sub>2</sub> gas analyzer there. Dr. Carl Ekdahl, another physicist, hired to replace Bainbridge, undertook the parallel testing. This task was not straightforward because we had encountered electrical shorts in several of the radiation detectors of our APC analyzers. A new company that had bought the rights to maintain the APC line of commercial analyzers had been unsuccessful in repairing the shorts, and the problem had gradually worsened. Ekdahl arranged for the Scripps machine shop to refurbish all of the faulty detectors. When at length this work was completed, he began a thorough parallel testing of the two kinds of analyzers and even of a third kind, with a still different optical system, called the UNOR.

One day while we were parallel testing all three types of analyzers, Lowe, who happened to be visiting us at the time, glanced at the pen traces and suddenly exclaimed that the UNOR instrument registered atmospheric CO<sub>2</sub> concentrations above the reference gas being used as a comparison, while the other two registered below. This seemed impossible! After some moments of shared astonishment, I felt a sinking feeling as I recalled my conversation 15 years earlier with Lewis Kaplan about a possible carrier-gas effect. Samples of CO<sub>2</sub> diluted with air might register differently from those (like our standards) diluted by nitrogen gas.

To test for this effect we quickly prepared provisional reference gases containing oxygen as well as nitrogen. We found that different analyzers could have substantially different responses to CO<sub>2</sub> depending on the proportion of oxygen. The correction for the Hartmann and Braun analyzer was nearly the same as for the APC but was distinctly different for the UNOR, causing the reversal we had witnessed. Since all of our data, except those from New Zealand, were based on APC analyzers and had been calibrated in the same way, our published scientific findings, which depended on spatial and temporal differences in concentrations, were not seriously affected. Obviously, however, we needed to establish calibrations based on CO<sub>2</sub>-in-air standards to compare with other investigations and to allow for a nearly constant bias caused by the carrier-gas effect.

I was worried, though, that reference gases containing oxygen might be less stable than gases with only inert nitrogen gas as the carrier. I decided that we should continue to rely on primary standards containing CO<sub>2</sub> in nitrogen; at the same time, we should prepare new standards of CO<sub>2</sub>-in-air, thus doubling our calibration effort. The NOAA people, not waiting for us to complete our testing program, purchased a new Hartmann and Braun analyzer, which they installed at Mauna Loa.

### *Real Trouble*

In 1971, shortly before NOAA installed their own analyzer at Mauna Loa, I submitted to the Atmospheric Sciences Section of the National Science Foundation (NSF) a proposal to support eight full-time scientists and technicians to contribute to the emerging global atmospheric CO<sub>2</sub> program under the auspices of the World Meteorological Organization. I felt honored when the program manager for meteorology asked me to expand my funding request to include a laboratory manager, to give me more time for scientific direction of my program. The proposal was then funded with seven full-time and four half-time staff positions to assist me. We soon enlarged our array of CO<sub>2</sub> stations with a flask sampling site on Fanning Island at 4°N in the equatorial Pacific, and we began to make continuous measurements of CO<sub>2</sub> at the end of an ocean

pier near our laboratory. Besides a laboratory manager, a second physicist, Dr. Robert Bacastow was hired to assist Carl Ekdahl in the interpretation of our data.

While this new funding cycle was still in its first year, a friend, Dr. Robert Charlson, who was measuring aerosols with a new “nephelometer” that he had invented (1), sent me copies of letters he had exchanged with the head of the Atmospheric Sciences Section of NSF. He had been informed that his studies were repetitive and therefore no longer suitable for NSF support. He protested that his nephelometer had led to well-regarded data and that he wanted to continue his studies. The NSF section office held fast to its new doctrine. Charlson lost all NSF support for his research.<sup>4</sup>

A few months later I was called by the program manager who had awarded me more funding than I had requested the year before. He now told me that my program probably had strayed from basic science. I should tell him what fraction of my effort was routine. I declared, as had Charlson, that none of my work could be deemed routine. NSF’s response was that the agency would decide for me. Half of my program was declared routine. I learned from Charlson that a NOAA administrator, recently transferred to NSF to become head of the Atmospheric Sciences Division, had introduced this new doctrine.

After some fruitless additional protesting to NSF, I wrote to Christian Junge in Mainz, explaining that evidently I couldn’t be part of the new WMO program unless NSF changed its mind. He wrote to NSF questioning the funding cut.

Here, in part, is the reply from NSF, dated May 17, 1973:

Dear Professor Junge:

I have delayed replying to your letter of March 20 until I had an opportunity to visit Dr. Keeling and discuss his work with him. I can assure you that we have no intention of unilaterally discontinuing support for Dr. Keeling and his work on CO<sub>2</sub> measurements. However, I believe that we in this program must tread a narrow line between that work which constitutes basic research and that work which constitutes fairly routine monitoring... I believe that Dr. Keeling’s work is of sufficient status that the scientific community will urge us to continue his support of *basic research* [italics added].

At first I thought that this last remark might mean that I could continue my work if I claimed that my measurements were nonroutine. The letter went

<sup>4</sup>Charlson continued limited research with funding from the US Environmental Protection Agency until 1976, when the agency insisted that he address urban aerosols. For the next five years he managed to investigate natural aerosols, his main interest, during extended visits to Sweden, where Bert Bolin arranged European funding. He has survived as a distinguished experimental scientist thanks to this European connection and reinstatement of aerosol funding by the National Science Foundation in the 1980s.

on to say, however, that, as an “interested scientist,” I would “presumably be willing” to work on “technical problems” of a “WMO network” of CO<sub>2</sub> stations. This evidently meant that I was expected to turn over all actual CO<sub>2</sub> measurements under my supervision to agencies dedicated to “monitoring.” This latter term, new to me in the context of my program, stuck in my mind. Soon it would be in use in the Atmospheric Sciences Section of NSF as a basis for separating all repetitive observations of nature from the scope of basic science.

A few weeks after I received this letter, it became clear on which side of the “narrow line” trodden by NSF I was judged to be. NSF cut my previously awarded second-year budget from nine to five staff positions. I now appealed to Dr. Junge for help, stating that I would not be able to solve the carrier-gas problem without supplemental funding. This was a persuasive argument for restoration of some of the lost funding, because my program had been the sole supplier of calibrated reference gases to WMO programs just starting up in other countries, as well as by NOAA, so that their measurements could all be reliably intercompared. No one else so far had prepared their own standards, although NOAA personnel were attempting to do so.<sup>5</sup>

Junge, in response, called for an international meeting of prospective participants in the new WMO program. At this meeting, which took place at Scripps in 1975, it was agreed to designate our laboratory a “Central CO<sub>2</sub> Laboratory of WMO” to supervise calibrating worldwide and to assist with evaluating instruments and procedures for this new program (59). After protracted procedural difficulties dealing with the United Nations bureaucracy, Scripps was awarded start-up funds for two years to carry out calibrating via the recently inaugurated United Nations Environment Program (UNEP).

## 1976–1980: THE RISE OF MISSION-ORIENTED RESEARCH

### *The Department of Energy Embraces Carbon Dioxide*

In 1972, before the funding cut by NSF had taken effect, Dr. George Woodwell of the Brookhaven National Laboratory convened a symposium of biologists and ecologists to contemplate how increasing atmospheric carbon dioxide might affect the environment of the Earth’s plants and animals. Invited to participate, I set about, with my new associates Ekdahl and Bacastow, to explain the significance of this rise in the context of geochemical models of the global carbon

<sup>5</sup>NOAA personnel met with me twice, as I recall, to discuss calibrating. They were not impressed by the manometric method, declaring that they had devised a volumetric method that was more precise. It wasn’t working yet, however.

cycle (3, 15). Imitating some earlier modeling attempts, we represented this cycle by interconnected reservoirs of carbon residing on land, in the oceans, and in the atmosphere. We asserted that carbon was exchanged among these reservoirs according to identified physical processes (such as turbulent gas exchange at the sea surface) and biological processes (such as photosynthesis and respiration of land plants). Aided by new measurements of the radioactive isotope carbon-14, many of which had been made recently by Professor Hans Suess at Scripps, we could estimate realistically the amount of carbon passing among the identified reservoirs. Indeed, Revelle and Suess (56) had already estimated in this way that about half of the  $\text{CO}_2$  from fossil fuel production was being absorbed by the oceans. We now showed that their estimate of oceanic uptake was close to what we observed in our measurements of rising atmospheric  $\text{CO}_2$ .

We noticed, however, that the measured rate of rise of  $\text{CO}_2$  didn't seem steady. When I had spoken to the American Philosophic Society in 1969 the rate had recently slowed down, although fossil fuel emissions were increasing. Now in 1971 the rate was speeding up. This wasn't particularly obvious when looking at the actual Mauna Loa record (Figure 7) but could be clearly seen when a cubic function, such as I had used in 1969, was fit to the data after subtracting the average seasonal cycle (Figure 8). This approximately decadal feature, seen also in the South Pole record, did not correspond to variations in fossil fuel use, nor could the speeding up of the rate of rise recently be caused by an increase

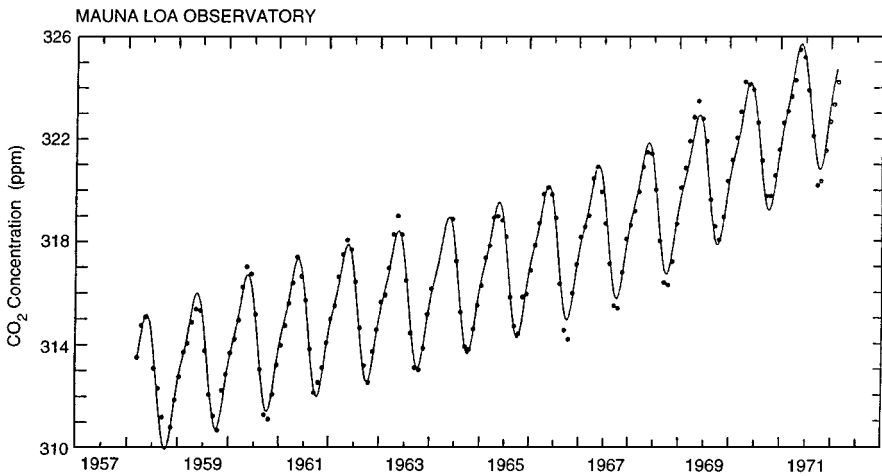


Figure 7 Long-term variation in the concentration of atmospheric  $\text{CO}_2$  at Mauna Loa Observatory, with data through 1971. Monthly data are shown as dots. The oscillatory curve was obtained using the same function as in Figure 6. (Source: Ref. 15)

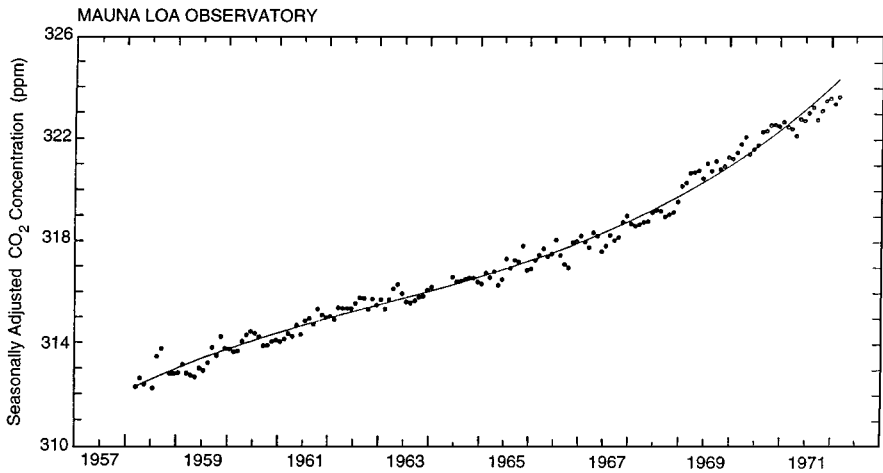


Figure 8 Secular trend of atmospheric CO<sub>2</sub> concentration at Mauna Loa. Circles represent monthly averages, seasonally adjusted by subtracting the 6- and 12-month cyclic terms used in deriving the curve of Figure 7. The *rising curve* is a cubic secular trend function as in Figure 6. (Source: Ref. 15)

in plant growth by CO<sub>2</sub> fertilization. Thus we didn't really understand what was going on.

A year later, in 1973, still another feature of the CO<sub>2</sub> rise was discovered. Bacastow, whose employment had survived the NSF funding cut, noticed that if he removed the influence of fossil fuel combustion from the South Pole and Mauna Loa records, which he could do approximately by subtracting a constant fraction of the computed fuel emissions, residual fluctuations showed up, shorter in duration than the decadal pattern reported at the Woodwell symposium.

Looking for clues in an extensive treatise on climate that had just been published (46), Bacastow<sup>6</sup> found a reference to the "southern oscillation," a feature of tropical climate that neither of us had heard of, consisting of a quasi-periodic strengthening and weakening of the air pressure difference across the equatorial Pacific Ocean. He saw that time variations in this gradient matched closely with

<sup>6</sup>This occurred, one day, when he was standing in the check-out line at the Scripps Library. The line was long, but for the diversion of waiting customers there was an inviting display of new books on a nearby table. To pass the time, he began examining the table of contents of one of these books and saw an entry labeled "cyclic and quasi-periodic phenomena." Turning to the text, he was surprised to find a description of a feature called "The Southern Oscillation." He had no prior idea that any interannual variation like this existed. He checked out the book, by HH Lamb (46), and soon saw that a plot of the Southern Oscillation (on page 246) tended to match the feature that he had seen in our CO<sub>2</sub> records.

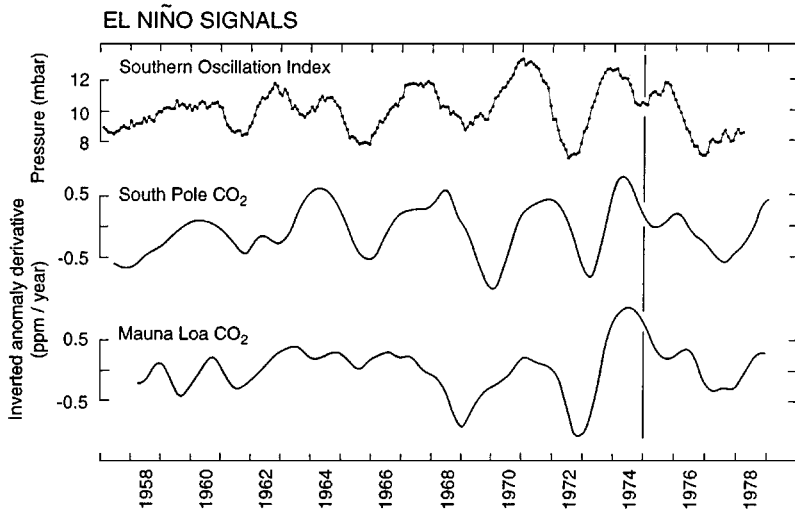


Figure 9 Southern Oscillation, expressed as an index, and residual rates of change of atmospheric CO<sub>2</sub> at the South Pole and Mauna Loa. The CO<sub>2</sub> data were first seasonally adjusted and the influence of fossil fuel combustion removed. The resulting rates are inverted in the plots for easier comparison with the Southern Oscillation index. Fully developed El Niño events occurred in 1965, 1969, 1972, and 1976, and a weak El Niño occurred in 1975. (Source: Ref. 4)

the rate of change in the residual fluctuations for both the South Pole and Mauna Loa Observatory (see Figure 9). He published his discovery, even though the phenomenon's cause was elusive (2). The correlation continued to appear in our data (4), as did decadal irregularities in the rate of rise of CO<sub>2</sub>.

In 1976 we published 14 years' worth of measurements for Mauna Loa and the South Pole (35, 36) showing the unmistakable rise in CO<sub>2</sub> concentration and its irregular pattern. The long wait to publish after the Brookhaven symposium was due to my hesitation to present our data to the public formally until the data could be properly adjusted for the carrier-gas effect. Because of the loss of almost half of our staff in 1973, it took us several years to establish this adjustment for our laboratory.<sup>7</sup>

This same year the issue of rising atmospheric CO<sub>2</sub> attracted the attention of a new US administrative division called the Energy Research and Development Agency (ERDA). It had recently been carved out of the US government's Atomic Energy Commission (AEC) along with a new Nuclear Regulatory Commission, following accusations that the AEC had not adequately addressed risks

<sup>7</sup>It took substantially longer to establish carrier-gas corrections for participants of the WMO CO<sub>2</sub> program, who depended on our laboratory for reference gases.

attending the use of nuclear energy. ERDA continued the AEC task of overseeing national laboratories devoted to nuclear studies but was mandated to consider energy issues more broadly than the AEC.

Pursuant to this new mandate, the director of ERDA's Oak Ridge National Laboratory, Dr. Alvin Weinberg, appointed a Study Group on the Global Environmental Effects of Carbon Dioxide (58). He did not hide his motive. The application of nuclear energy to electrical power generation was regarded as unsafe by a vocal group of objectors to nuclear energy. Owing to the CO<sub>2</sub> greenhouse effect, the burning of fossil fuels might be more dangerous to mankind than any perceived side effects of nuclear energy. It was time to find out.

After brief deliberations the Study Group, chaired by Weinberg, recommended that ERDA sponsor a Carbon Dioxide Effects Research Program. In July, 1977, as a first step, a group of scientists was asked to prepare a development paper (58). NOAA's leading proponent of CO<sub>2</sub> research was asked to be "interim director." A "broadly based group of experts" was called on to prepare papers recommending "what should be done." I was asked to participate. My assigned subject area was "the exchange of CO<sub>2</sub> gas at the interface of air and sea." Assigned to set up a worldwide program of "atmospheric monitoring of CO<sub>2</sub>" was Dr. Kirby Hanson, a solar energy scientist from NOAA.

The papers we prepared were reviewed at a meeting in February, 1978, shortly after ERDA had been restructured as the Department of Energy (DOE). As a courtesy, because I was suffering from back pain and didn't want to travel, the meeting was held at Scripps. The participants were specifically asked in advance of the meeting to provide advice "on the most constructive way to start spending DOE CO<sub>2</sub> funds to help resolve the uncertainties in the CO<sub>2</sub> problem." This advice was sought relative to each participant's assigned area of expertise. Dr. Hanson, now identified as a CO<sub>2</sub> monitoring expert, recommended an extensive NOAA-funded CO<sub>2</sub> program. I stated the case for pursuing the possibility, highly speculative at the time,<sup>8</sup> that the CO<sub>2</sub> gas exchange between the air and the oceans could be established quantitatively by direct eddy flux measurements. The consequences of these combined recommendations were soon to have a profoundly negative impact on my ability to pursue CO<sub>2</sub> studies.

The first direct signs of this appeared in a hand-delivered copy of an internal "diary note," which I received in April, 1977. Prepared by the manager of a new "climate dynamics program" of NSF, it memorialized an agency meeting. NSF staff at this meeting disclosed that funding of "routine monitoring" activities had continued in my program despite NSF's doctrine to the contrary. However, 1977 would be "the final year" of this support. NOAA personnel at the meeting then stated that they weren't ready to assume full responsibility for monitoring

<sup>8</sup>And still so, as I write.

yet. They would assist my program with temporary funding through 1979. After that, however, “of course they intend to phase out Dr. Keeling’s work including the providing of standard gases.” WMO personnel, also present at the meeting, declared that the UNEP funding to Scripps would be phased out in two more years, since it was only granted to “initiate monitoring activities throughout the world.” I now realized that there was a coordinated plan to phase out my entire atmospheric CO<sub>2</sub> measuring program.

### *Negotiating with Three Agencies*

At about the same time, I was visited by Willem Mook of Groningen University and Karl Otto Münnich, my host eight years earlier in Germany. They arrived together with a mission in mind. Why had I given up measurements of the <sup>13</sup>C/<sup>12</sup>C ratio of atmospheric CO<sub>2</sub>? Isotopic data of the kind I had collected at Caltech would obviously furnish valuable additional insight on the working of the global carbon cycle.

I explained that I had planned to make isotopic measurements when I first came to Scripps in 1956. I had even set up and tested an extraction vacuum line to prepare samples for isotopic analysis back in 1957. Ample air for isotopic analysis remained in the flasks of our collection program after concentration analysis. Difficulties with getting isotopic analysis carried out at Scripps, followed by mounting demands on my time, had thwarted my plans till now.

Professor Mook proposed a remedy. A student of his, Pieter Tans, would soon complete a PhD thesis. He could spend a year at Scripps as a paid-for postdoctoral fellow. If we prepared isotopes of CO<sub>2</sub> from our flask samples, Mook’s laboratory would run the samples on a mass-spectrometer in Groningen.

The plan was implemented. Dr. Pieter Tans arrived in April, 1978, and within a few weeks activated the extraction line that I had set up 21 years earlier. We soon had a year’s worth of isotopic measurements completed for La Jolla, Mauna Loa Observatory, and the South Pole. We published a paper showing a distinct shift in <sup>13</sup>C/<sup>12</sup>C ratio since 1955 related to fossil fuel combustion (38).

In June, 1978, it was formally announced that DOE had agreed to fund NOAA to measure atmospheric CO<sub>2</sub> in flasks at 20 stations. Soon after this I was told by the interim director of the DOE CO<sub>2</sub> program that, regrettably, the US government couldn’t continue funding my studies of atmospheric CO<sub>2</sub>.

The NSF program manager, who in 1972 had said that at least the “basic research” component of my atmospheric CO<sub>2</sub> studies was fundable by his agency, had left NSF. His replacement, in the new Climate Dynamics Research Section of NSF, declared to me that I should not apply for any further funding. I protested, stating that I had just begun a cooperative program with Groningen University to gather valuable CO<sub>2</sub> isotopic ratio data. Soon thereafter I learned

that NOAA had just arranged to have the US Geological Survey make isotopic measurements for their CO<sub>2</sub> program, disposing of that argument.

In late 1979, after I pressed the issue, the same manager at NSF agreed to consider funding my program for two more years, if I agreed in advance not to apply again. To reinforce his determination, and evidently mindful that NSF relied on peer review to judge proposals, he warned me that he could find “calibrated reviewers” should I be unwise enough to apply again.<sup>9</sup> Funding was then provided for two years, for eight full-time salaries. NOAA and DOE together contributed 80% of the funding, assuring that managers in these two agencies were in control of funding, not NSF. A fixed ending date of July, 1982, was set.

Now I risked taking a sabbatical leave from Scripps. Overcoming my concerns about future funding were the urging of my family, a desire to broaden my perspective by a second stay in Europe, and an opportunity to interact with Professor Hans Oeschger and his associates at the University of Bern, who were developing measurements of CO<sub>2</sub> trapped in glacial ice in hopes of extending atmospheric CO<sub>2</sub> data over thousands of years.

One of my sons, Ralph, who had just begun graduate studies in environmental science, was also invited to come to Bern to assist a graduate student working in the ice core program. When my family and I arrived in Switzerland, Oeschger’s student had not been able to achieve a clean separation of CO<sub>2</sub> from water in the ice samples. He had been told by the overseeing ice crystallographer to thaw the samples before extraction of the CO<sub>2</sub>. The student had felt that he couldn’t defy authority, but with Ralph’s assistance in running experiments and my backing he had an excuse to try a dry extraction. It led to successful collection of CO<sub>2</sub> data from ice cores. Ralph, who had planned to be theoretician, changed advisors the next year at Harvard University and became an experimentalist. Watching the ice core program become a success was one of the motivations for his change of mind.

Professor Oeschger very graciously provided a highly talented graduate student, Martin Heimann, to work with me on interpreting the Scripps CO<sub>2</sub> data. With a geochemical model portraying sources and sinks of CO<sub>2</sub> with north-south resolution (23, 40), we refined the regional representation of the carbon cycle that I had worked on earlier with Bert Bolin in Sweden (8). Still more important as groundwork for a later study, we worked to identify all of the most important components of the global carbon cycle, and to characterize them using up-to-date geochemical data.

<sup>9</sup>While writing this biographical chapter I learned from Dr. Robert Charlson that during the same year he was advised by the program manager of the atmospheric chemistry program of NSF not to seek funding for his nephelometer studies. Exactly the same term, “calibrated reviewers,” was used to reinforce this advice.

Halfway through my year's stay in Bern, I was abruptly reminded that my overall program was supposed to terminate. From the former interim director of the DOE CO<sub>2</sub> program, now in his capacity as director of the new DOE-funded NOAA program, I received a polite letter, asking me to prepare a "protocol" to transfer responsibility of my CO<sub>2</sub> measurements to NOAA. This letter dated February 8, 1980, read in part:

Dear Dave: You may recall that as part of the arrangement for funding Scripps for two years by NSF, DOE and NOAA, it was agreed that you and I (or our staffs) ought to almost immediately agree on some protocol whereby, if we jointly agree, responsibility for preparing CO<sub>2</sub> standards and measuring atmospheric CO<sub>2</sub> at Scripps stations would be transferred to NBS and NOAA respectively. Such a transfer from Scripps would not reduce funds from the NSF to Scripps during the two-year period of the most recent contract renewal but it likely would affect the kinds of work which the three supporting agencies would grant funds for any further renewal.... Do you have suggestions on how to start our preparation of such a protocol?... I hope that we can get past the preliminary exchanges well before your return to the States, which I presume will be during the summer of 1980... I hope you and your wife are enjoying Bern.

I wrote back that I didn't agree to discontinue my program, citing that the NOAA program and the USGS isotopic data hadn't been demonstrated to be of a quality to make my studies redundant.

## MORE OBSTACLES

### *A Short Reprieve*

I returned from Switzerland with my family early in the fall of 1980. I renewed my protest of the imminent termination of my program. NOAA personnel at length agreed to a meeting at Scripps where I would have an opportunity to plead that my studies were not redundant with theirs. During the morning on the meeting day in January, 1981, I gave my arguments to the NOAA people with little feedback. The director of the DOE-funded NOAA CO<sub>2</sub> program complimented my program but once again expressed regrets that the US government couldn't any longer afford to fund it.

After lunch, the director of Scripps, Dr. William Nierenberg, presided over a wrap-up session in his outer office. We sat down together around a big table. Dr. David Slade, now the permanent director of the DOE CO<sub>2</sub> program, had arrived. I regretted that it was he who must ultimately decide the funding issue, and that he hadn't heard any part of my morning pleadings. Nierenberg briefly outlined the problem, and a general discussion ensued. At some point, without any special lead-in, Dr. Slade in a very quiet voice said:

"I think that DOE can pick up the tab for Keeling's program."

By accident I happened at that moment to be looking at the director of the NOAA CO<sub>2</sub> program. For a fraction of a second his facial muscles tensed involuntarily. It was quickly over, but I was sure that he was as much surprised as I was.<sup>10</sup> The meeting soon concluded, with little more said by the NOAA people.

After the meeting Nierenberg revealed to me that he had talked with Deputy Secretary Edward Frieman of DOE, who felt that killing my program abruptly would be unjustified. Slade had followed the secretary's advice. After peer review I was granted two years of DOE funding, in an amount equal to the total previously supplied jointly by DOE, NOAA, and NSF.

My good fortune was shortlived. Ronald Reagan had become President of the United States. His new administration almost immediately shook up the DOE environmental program. Dr. Frieman was replaced. Slade soon after was transferred to an inconsequential job. Slade's replacement, Dr. Fred Koomanoff, almost surely under instructions from above in DOE, substantially reduced the overall funding for CO<sub>2</sub> research and promptly informed me that, except for the 1982 funds already granted, I would receive no more DOE funding. He made clear his authority to make this decision, because DOE, with the support of NOAA and NSF, had become the US government's "lead agency" in CO<sub>2</sub> research.

My atmospheric CO<sub>2</sub> observing program was again in turmoil. I reapplied to NSF for funding, this time to their atmospheric chemistry program, where there was a director who did not heed the earlier "agreement" that I should no longer seek any funds from NSF. He was nevertheless constrained by the now well-established doctrine that routine measurements shouldn't be funded by the Atmospheric Sciences Division of NSF and was probably further constrained because of DOE's status as "lead agency" in CO<sub>2</sub> research. Only about one third of the previously anticipated annual DOE funding was awarded to me by NSF for 1983.

To gain additional funding I had two arguments. First, our data and NOAA's did not agree closely. This wasn't surprising: NOAA had adopted measuring procedures substantially different from ours. I argued that our measurements, especially at Mauna Loa Observatory, should therefore not be discontinued. Second, the responsibility for primary calibrating of the WMO CO<sub>2</sub> program was still assigned to Scripps. NOAA kept urging that this responsibility be transferred to the US National Bureau of Standards (NBS), to provide a desirably "institutional setting," but the CO<sub>2</sub> experts of WMO were refusing to support NOAA's request.

<sup>10</sup>For many years prior to DOE's setting up a CO<sub>2</sub> program he and Slade had worked closely together on joint AEC-Weather Bureau projects.

Late in 1982, NOAA agreed to fund my Mauna Loa research for one more year. This funding, however, was for the expressed purpose of coming promptly into mutual agreement on measurements, so that there would be no further need for my program.

### *A Mandated "Convergence" Effort*

Measurement agreement would not have been a problem for NOAA if they had closely imitated the Scripps procedures. From the start, however, they had planned a program that would produce more data at lower unit cost. To collect air they used small flasks more likely to cause contamination on storage than our 5-liter flasks. At four principle stations extending from Alaska to the South Pole they had adopted less laborious procedures for sampling the air and for calibrating continuous measurements there. The isotopic analyses, farmed out to a laboratory of the US Geological Survey, appeared to be far from satisfactory. NOAA's project to prepare their own standards by a volumetric technique had not worked out. The problem of the carrier-gas effect was still not settled with respect to the WMO worldwide program, and the willingness of NOAA from time to time to swap one type of analyzer with another, without demonstrably adequate overlap, supported my argument that, for a while at least, my program should be allowed to continue measuring CO<sub>2</sub>.

I was now in the uncomfortable position that my program's survival depended, paradoxically, on a continued disagreement between our data and NOAA's. I felt insecure and evaded requests coming from NOAA to tell them our results informally before they told us theirs.

Nevertheless, NOAA and Scripps set about formally to bring about the "convergence" of results. The field exercise was carefully planned. Success was achieved in December, 1983, but only after both instruments had been adjusted with unusual care, and we had sequenced our reference gases and matched our other procedures very closely. The exercise didn't solve the convergence problem, however, because we couldn't agree on whose procedures were best for future "routine" work. I could still claim that our procedures were not redundant with NOAA's, and might be better.

I argued that Scripps should be allowed to continue measurements at Mauna Loa simply for the long-term continuity provided. There was no guarantee that NOAA's program might not have problems in the future. The CO<sub>2</sub> program, I argued, was important enough that at one spot on the Earth two parallel sets of measurements were justified.

In 1984, NOAA and DOE each agreed to pay part of the cost for Scripps to continue our measurements at Mauna Loa for one more year to complete a report on the convergence, but Koomanoff refused to grant any more funds from DOE after that. The crisis therefore continued. I objected to everyone I could

about the ending of my observing program. There were still further discussions brokered by the former NSF program manager, now working for NOAA, who in 1979 had asked me to cease seeking support from NSF. Then the Oak Ridge National Laboratory, operated for DOE, agreed to supervise funding for Mauna Loa studies for two more years, relieving Koomanoff of having to change his mind about direct DOE funding.

A new program manager from this laboratory in Tennessee appeared on the scene and proclaimed to me new rules of engagement. I was to write a proposal in which I anticipated two discoveries per year arising out of the data. The discoveries could not be associated with the results from any other atmospheric CO<sub>2</sub> study or location and were to be programmed with stated “milestones” anticipating progressive stages in the discovery process. After objecting over the telephone that such a procedure was impracticable, if not impossible, I philosophically attempted to comply. Funding for two years was forthcoming but was not renewed after that. I had evidently failed in this discovery process, or there was no further perceived need for discoveries from Scripps.

Again I faced a budget crisis. A complaint reached Admiral James Watkins, secretary of DOE during President Bush’s administration, who soon after visited my laboratory. I heard from a new program manager, who had replaced Fred Koomanoff, that “my predicament had sensitized all levels of DOE.” My DOE program was saved for the second time, and remained secure until 1994.

In June of 1994 I received a letter from DOE stating that “research presently being supported on the topics of the global carbon cycle, and on the response of vegetation to CO<sub>2</sub> will be redirected towards focused efforts related to terrestrial carbon processes (TCP).”<sup>11</sup> Existing grants related to CO<sub>2</sub>, carbon, and vegetation topics were not going to be renewed, specifically including my grant. This action of DOE, however, was not directed at my research alone; DOE had decided to cease being the lead US agency for CO<sub>2</sub> research and was giving grantees a year’s warning of the fact. DOE had thus come full turn since Alvin Weinberg had started the CO<sub>2</sub> effects program in DOE to demonstrate that CO<sub>2</sub> released by the burning of fossil fuel might have adverse consequences. DOE was now seeking evidence that this release might have favorable consequences. The agency didn’t need to support broadly based carbon cycle research anymore.

Among those who received similar letters I was particularly fortunate because I could claim with some justification that my research would benefit the new TCP program. Although the latter was conceived mainly to carry out field experiments to test whether rising CO<sub>2</sub> will result in greater uptake of CO<sub>2</sub>

<sup>11</sup>The letter explained this unexpected change of focus by stating that “it is the practice of the research program offices [of DOE] to periodically change program direction...through the normal course of identifying research needs related to the Department of Energy (DOE) mission....”

by plants and soil and thereby reduce the prospect of greenhouse warming, it could benefit from global data gathered in my program, simultaneously with data from the TCP field studies. My program has thus survived to the present.

### *Struggling with the Standardization Issue*

Still greater than my frustration with the attempt of US agencies to restrict my atmospheric CO<sub>2</sub> research for the sake of a doctrine was their determination to institutionalize international calibration of standards, even if to do so might compromise the entire worldwide atmospheric CO<sub>2</sub> program of WMO. In March, 1977, less than two years after Scripps was approved by WMO as a "central laboratory" to maintain worldwide standards, DOE sponsored a conference on global effects of CO<sub>2</sub> from fossil fuels, including a special panel to address future needs of CO<sub>2</sub> standards (12). I was not informed. The panel found that the Scripps standards were "unsatisfactory" and proposed that the National Bureau of Standards (NBS) issue new standards in which the "total uncertainty" in any sample "will not exceed 0.1% relative."<sup>12</sup>

I was dismayed by this administrative decree because our Scripps manometric standards were demonstrably more precise than that, and their absolute uncertainty should be hardly greater than the imprecision as soon as the carrier-gas effect was fully resolved for the WMO CO<sub>2</sub> program. For a while the calibrating matter remained quiet, however, as NBS responded to DOE's request. Our UNEP funding ceased, however, and we proceeded only very slowly to carry out our calibrational obligations to WMO.

Four years went by. Then in 1981, at a WMO CO<sub>2</sub> Experts meeting in Geneva, Switzerland, an NBS representative claimed that the requested standards goal of 0.1% "uncertainty" had been met. As I reminded the CO<sub>2</sub> Experts, they had previously set a goal of 0.1 ppm or better, whereas the NBS goal was equivalent to relaxing their goal to 0.3 ppm. The director of the NOAA CO<sub>2</sub> program, who had chaired the DOE standards panel, and was now chairing this meeting, thereupon sought their concurrence to so relax the goal. I argued strongly to retain the previously agreed upon goal. The director was unsuccessful (60). After the meeting, NOAA nevertheless pressed NBS to make CO<sub>2</sub> standards, without stressing the need for higher precision.

DOE now asked NBS to predict when their standards would be ready. Sensing WMO's need for higher precision, NBS pleaded to be given three more years.

DOE responded by providing funding for a convergence of NBS standards with our standards, constrained by a timetable to assure compliance in three years. Adequate funds, under NBS supervision, were passed on to Scripps to carry out our part of the convergence effort. Early on, NBS had decided not to

<sup>12</sup>This wording cleverly left ambiguous the distinction between imprecision and absolute inaccuracy.

attempt to develop manometric calibrating, in favor of two other approaches. One was simply to improve a gravimetric technique they had already used to achieve 0.1% uncertainty. The other was an untried isotopic dilution method. Near the end of the three years, it became evident that they would probably fail in both attempts. I learned from dedicated personnel at the working level in NBS that considerable pressure was then placed on them to meet the original contract schedule, irrespective of problems being encountered and a funding cut imposed by DOE in the third year.

In November, 1985, the WMO Experts met at NBS headquarters to decide whether to accept the NBS standards. Shortly before the meeting, the DOE office of Koomanoff, calling attention to the 1977 DOE conference, sent a telegram to all relevant US agencies asking them to concur in a DOE decision that, because NBS had achieved the uncertainty of 0.1% recommended in 1977, the agency should now assume responsibility for providing standards for the international "CO<sub>2</sub> measurement community." Astoundingly, the agencies were requested to concur with the DOE statement *prior* to the WMO meeting.<sup>13</sup>

The CO<sub>2</sub> Experts, notwithstanding this telegram, rejected the NBS standards. NBS, without prospects of further DOE funding, dug into their own resources to remedy the situation. When they failed again at their next meeting in 1987, the embarrassment caused the principal staff personnel involved to be transferred to other duties or to leave the agency. NBS gave up their attempt to replace the Scripps standards.

The NBS failed to provide reliable standards because they could not find a satisfactory substitute for manometric calibrations. Ironically this occurred mainly because of their "institutional setting." They consistently rejected "manometry," as they called it, because this technique was useful only for trace gases that could be trapped from air at low temperature. The manometric technique was not generally applicable to their agency mission.<sup>14</sup>

Who should prepare international CO<sub>2</sub> standards was finally resolved by a special panel of the National Academy of Sciences, set up after the 1987 WMO

<sup>13</sup>The telegram dated 28 October, 1603Z, stated in part: THE 1977 CONFERENCE ON GLOBAL EFFECTS OF CO<sub>2</sub> FROM FOSSIL FUELS...RECOMMENDED THAT THE NATIONAL BUREAU OF STANDARDS DEVELOP STABLE STANDARD REFERENCE MATERIALS FOR MEASURING ATMOSPHERIC CO<sub>2</sub> [WITH RELATIVE UNCERTAINTY OF 0.1% OR BETTER]. THIS TASK HAS NOW BEEN ACCOMPLISHED. I AM THEREFORE PROVIDING YOU WITH A STATEMENT OF THE USA CO<sub>2</sub> PROGRAM WHICH ESTABLISHED WITH NBS THE INSTITUTIONAL RESPONSIBILITY FOR PROVIDING CO<sub>2</sub> STANDARD REFERENCE MATERIALS TO THE CO<sub>2</sub> MEASUREMENT COMMUNITY. YOUR CONCURRENCE IS REQUESTED BY 31 OCTOBER, PRIOR TO WMO EXPERTS MEETING IN CALIFORNIA, NOV. 4-8, 1985.

<sup>14</sup>I had previously met with NBS personnel on my own initiative to urge their use of our manometric procedures. As I learned later, a formal report was written by NBS personnel rejecting manometry.

meeting at the urgent request of Roger Revelle. With NBS out of the picture, Dr. Pieter Tans of NOAA proposed to prepare manometric standards. The CO<sub>2</sub> Experts agreed, and NOAA set about developing procedures. In 1995 WMO granted NOAA full responsibility for preparing standards and operating the Central CO<sub>2</sub> Laboratory. As I write, the prospect is favorable that NOAA will be able to maintain precise, stable standards for the WMO program, provided that decision-makers in the US government don't someday find standardization an easy target for funding cuts, an ever present threat to a government agency carrying out an activity with almost invisible benefits.

## SCIENCE IN SPITE OF POLITICS

### *Why Go On?*

Through the long years of my disagreement with government agencies on whether or not I should be measuring atmospheric CO<sub>2</sub>, many people wondered why I tried so hard to stay involved. I will now attempt to explain, although in some respects I'm not really sure.

First, my enthusiasm to study atmospheric CO<sub>2</sub> never slackened; it depended, however, on acquiring data that truly reflected nature. The international CO<sub>2</sub> monitoring program of WMO, although originally organized by scientists, was soon mainly under the control of meteorological agencies. After agency managers began to assert that the acquisition of CO<sub>2</sub> data, like the acquisition of weather data, was to be regarded as routine rather than a pursuit of basic science, I wondered what might happen to data quality over time. I hadn't forgotten that the CO<sub>2</sub> measurements published in *Tellus* before I began my studies had been terribly wrong but were generally regarded as valid until new data proved them wrong. I wanted to remain directly involved in CO<sub>2</sub> data gathering to be able to judge the quality of such data on my own terms.

Moreover, those recruited to inaugurate NOAA's CO<sub>2</sub> program set about removing what I felt to be safeguards in the procedures that I had adopted to assure valid data. Then these newcomers gave out an impression that measuring atmospheric CO<sub>2</sub> was relatively problem-free, whereas I had had difficulty sustaining high-quality measurements over long periods. To add to my apprehensions, the official in NOAA most responsible for opposing my program had a reputation for after-the-fact apologies when data under his supervision had turned out to be less than satisfactory.

The most compelling reason for my wishing to stay involved was that the data gathered in my program became more and more fascinating as the records lengthened. They didn't appear to be subject to the law of diminishing returns. To complete this biographical essay I will touch on some of the scientific findings that led me to feel this way.

### *More Discoveries from Time-Series Data*

Following Bert Bolin's and my analysis of a global "snapshot" of atmospheric CO<sub>2</sub> in 1963 (8), my interest turned to the ever lengthening time-series of CO<sub>2</sub> observations at Mauna Loa Observatory and the South Pole. As I have already explained, these records by 1972 were long enough to see evidence that CO<sub>2</sub> varied on a decadal time scale in a manner that couldn't be explained by emissions from fossil fuel combustion. I wanted to acquire the most trustworthy data possible, for as long as possible, to study such subtle effects. I was dismayed when advised that NSF funding for my CO<sub>2</sub> measuring activities would be cut drastically because they had become routine.

In 1982, during the darkest days of my funding problems, William Nierenberg, the director of the Scripps Institution of Oceanography, found me a new, nongovernmental source of funding: the Electric Power Research Institute (EPRI). The original intention was to support combined CO<sub>2</sub> and climate studies by three Scripps investigators, but on the basis of peer review, EPRI awarded the entire agreed-upon sum to my program. With this incredible windfall to pay costs, I invited Dr. Martin Heimann, who had just completed his doctoral thesis in Switzerland, to work with me. He was hired for two years with a programmer to assist him. Also, a data analyst, Mr. Timothy Whorf, and a modeler, Dr. Stephen Piper, didn't have to be laid off.

With the generous collaboration of Dr. Inez Fung of the Goddard Institute of Space Sciences, who had pioneered three-dimensional modeling of atmospheric CO<sub>2</sub>, we were able, in less than a year, to simulate how prescribed oceanic and terrestrial processes specifically affected atmospheric CO<sub>2</sub> at each of our observing stations (24). By the fall of 1985, we had identified the major sources and sinks of atmospheric CO<sub>2</sub> likely to be causing the variations seen in our data (29). Four years later, this work, which without EPRI support would probably never have been completed, resulted in four articles totalling nearly 200 pages, setting forth virtually all that we knew at the time from measurements of atmospheric CO<sub>2</sub> (25, 26, 41, 42).

Meanwhile, in 1983 we experienced a partial breakthrough in understanding the relationship of El Niño events to atmospheric CO<sub>2</sub>, discovered nearly ten years earlier by Bacastow. While in Europe attending a meeting, I learned that Drs. Chris Folland and David Parker of the British Meteorological Office in Bracknell, England had assembled a global time-series of sea-surface temperature. I visited Bracknell the next week to compare our respective data sets. We saw closely matching interannual patterns in CO<sub>2</sub> and temperature, as though both were influenced by El Niño events.

The El Niño phenomenon was just beginning to attract widespread attention. Indeed, a major event was in progress. Back at Scripps, Bacastow and I soon saw an unmistakable response to that new event, first in temperature,

and then in atmospheric CO<sub>2</sub>, as both rose anomalously. We couldn't, however, account for the anomalous CO<sub>2</sub> rise by any simple mechanism such as changes in the temperature-dependent solubility of CO<sub>2</sub> in surface ocean water, because the observed CO<sub>2</sub> rise was too large. Soon, however, we had a tentative answer.

In spite of funding constraints, my program had gradually added more CO<sub>2</sub> sampling sites. This expansion was possible mainly because of the generosity of technical personnel in NOAA, who collected air samples in flasks on our behalf at stations in their growing network. Also, by 1985, with Mook in the Netherlands providing isotopic data at almost no cost to us, we were obtaining isotopic data for all of our sites.

We now discerned patterns related to El Niño events in the <sup>13</sup>C/<sup>12</sup>C isotopic ratio of atmospheric CO<sub>2</sub>, as well as in its concentration (41). This was a surprise because these isotopic variations must mainly reflect CO<sub>2</sub> exchanges with vegetation on land. Oceanic exchange of CO<sub>2</sub> with the air does not cause significant isotopic fractionation, whereas, as I had learned at first hand during my years at Caltech, vegetation on land does. The dominant cause of an anomalous rise in CO<sub>2</sub> concentration during El Niño events appeared to be a release of CO<sub>2</sub> to the air by vegetation and soils.<sup>15</sup>

Ironically, these discoveries were coming at about the same time that the program manager for our Mauna Loa studies was demanding that I anticipate two discoveries per year in advance of receiving funding. What if we could temporarily keep our isotopic discoveries confidential, and later propose to discover them, complete with milestones? Then I remembered that the anticipated discoveries to get funding must arise exclusively from Mauna Loa data. These broader discoveries didn't count.

It was time to take a further look at variations in the atmospheric CO<sub>2</sub> records on the decadal time scale. The gradual slowdown in the rate of rise of CO<sub>2</sub> at Mauna Loa and the South Pole in the mid-1960s, and the subsequent more rapid rise in the early 1970s, had been followed by two more such slowdowns and rises. With our records now 30 years long, these fluctuations looked like a repeating decadal oscillation. Was the cause oceanic or terrestrial? Did El Niño events in some way contribute? Our quest to find out led us, however, well beyond our original focus, because once again we found a surprising relationship between CO<sub>2</sub> and temperature.

As the first step in this quest, my associate Mr. Tim Whorf and I removed from consideration, as Bacastow had done earlier, the pervasive upward curvature in our CO<sub>2</sub> records. This feature, best seen in our long CO<sub>2</sub> records at Mauna Loa

<sup>15</sup>We couldn't be sure that our data were correct, however. The US Geological Survey had not provided any useful isotopic data. The only other data besides ours, from Australia, showed no El Niño signal.

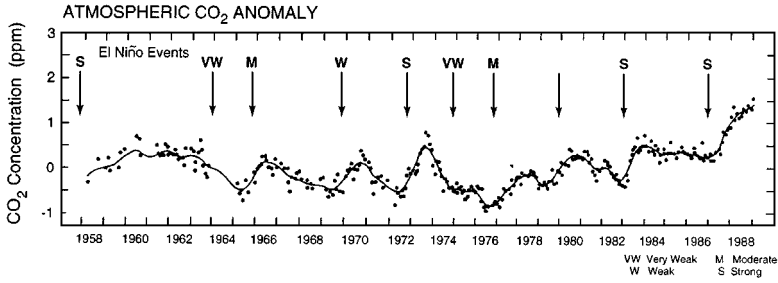


Figure 10 Anomaly in the concentration of atmospheric CO<sub>2</sub> for data of Mauna Loa Observatory and the South Pole combined. The solid curve is a spline fit to monthly averages of the data (the latter plotted as dots) to show El Niño patterns. The arrows denote the time of El Niño events, ranked S, strong; M, moderate; W, weak; and VW, very weak. The event in 1979 is doubtful, and is not ranked. (Source: Ref. 41)

and the South Pole, captures the worldwide rate of rise in CO<sub>2</sub>, a rise reflecting the relentless increase in rate of emissions of CO<sub>2</sub> from the combustion of fossil fuels. We estimated that 57% of these emissions accounted for the global rise in atmospheric CO<sub>2</sub> from 1958 to 1989 (41). By subtracting this fraction month by month from the average of the Mauna Loa and South Pole CO<sub>2</sub> records after seasonal adjustment, we obtained an “anomaly” plot. Expressed by a spline curve, this plot beautifully revealed El Niño patterns, except for a very weak event in 1964, a year when we had no data (Figure 10). A stiffer spline curve fit to the same data showed a decadal pattern (solid curve in Figure 11). An even stiffer spline curve showed a hint of a yet longer-term trend, evidenced by

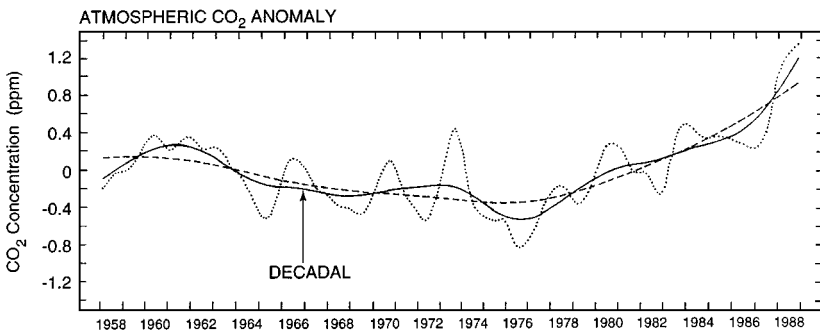


Figure 11 Interannual variability in the anomaly in atmospheric CO<sub>2</sub>, shown by curves of varying stiffness. The solid curve is a spline fit to the monthly data of Figure 10, to show decadal variability. The dashed curve is stiffer. The dotted curve is looser; it is the same as the solid curve in Figure 10 but plotted on an expanded scale. (Source: Ref. 41)

a downward bowing near the middle of the anomaly record (dashed curve in Figure 11). Amazingly, global temperatures showed all of the same patterns: El Niño and decadal signals, and a bowed shape (Figure 12).

More recently, with our records extended through 1994, we further noticed that the amplitude of the seasonal cycle of atmospheric CO<sub>2</sub> at Mauna Loa showed a similarly timed decadal signal, especially after 1975 (44). A longer-term increase, possibly related to fertilization of plants caused by rising CO<sub>2</sub> (41, 50), overlaid this signal, but the signal itself showed greatest amplitudes during times of unusual warmth near 1980 and 1990 (Figure 13), suggesting a positive response of plant growth to warming (44). Also, the seasonal decline in CO<sub>2</sub> concentration in the spring came earlier than usual during these warm years, indicating an earlier growing season by as much as 7 days, confirmed by satellite data on the Earth's greenness (49). We saw even greater amplitude increases (up to 40%) and similar evidence of earlier plant growth at our two more northern sites.

An increase evidently also occurred in the north-south gradients in atmospheric CO<sub>2</sub> and its carbon-13 isotopic ratio. These increases in gradient, especially evident in the late 1980s, demonstrated that a net loss of carbon to the atmosphere had occurred in the northern regions in spite of increased plant growth, presumably because the ground and soil also warmed, releasing more than the usual amount of CO<sub>2</sub> from decaying plant detritus residing in litter and soils (54). Thus both uptake and release of CO<sub>2</sub> on land at higher latitudes appear to have responded strongly to warming, the releases more so than uptakes. Although we originally looked to the oceans as the likely leading cause of temperature-driven signals observed in CO<sub>2</sub>, our data now pointed to terrestrial vegetation and soils as more important.

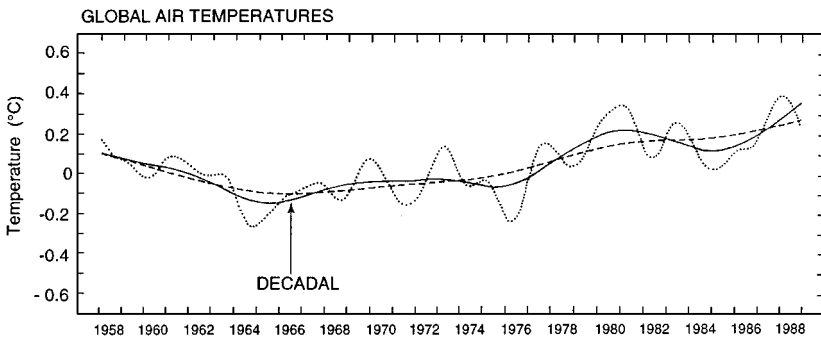


Figure 12 Interannual variability in the global anomaly in air temperature, in °C, shown as in Figure 11, by spline curves of varying stiffness fit to monthly data (not shown). (Source: Ref. 41)

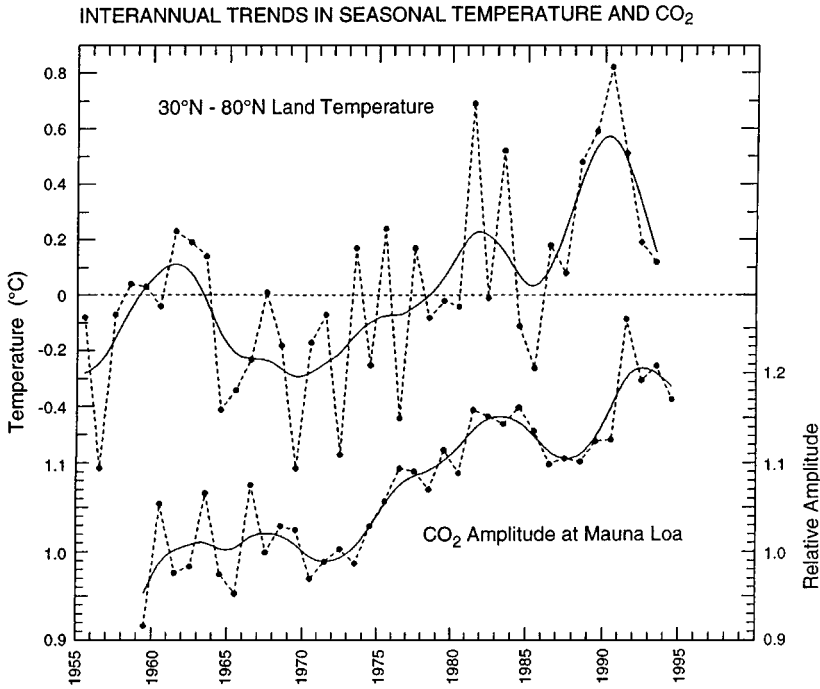


Figure 13 Comparison of trends in the amplitude of the seasonal cycle of atmospheric CO<sub>2</sub> and in temperature over land north of the tropics. Dots connected by dashed lines indicate the data. The smooth curves are spline fits to show decadal variability. The upper plot shows annual average temperatures north of 30° N. The lower plot shows annual amplitudes for Mauna Loa Observatory, relative to 1964. (Source: Ref. 44)

### Linking CO<sub>2</sub> Data to Global Climate

Our curiosity was now drawn towards what could be causing approximately 10-year fluctuations in temperature. Finding no clear explanation in the scientific literature, we began to search for a cause ourselves. Global temperature data had recently become available from A.D. 1855 onward, giving us a much longer record for studying decadal variability than afforded by atmospheric CO<sub>2</sub> data.

To this long global temperature record we fit spline curves of three different stiffnesses, as we had in our analysis of temperature variability over the 40-year period of our CO<sub>2</sub> record (Figure 14, upper plot, from Refs. 43 and 45). We then subtracted the spline curve of intermediate stiffness from the most flexible spline curve to create an approximately decadal “band-pass” curve (Figure 14, lower panel). Supporting our belief that the recent decadal fluctuations in CO<sub>2</sub> and temperature might have some underlying cause, this curve

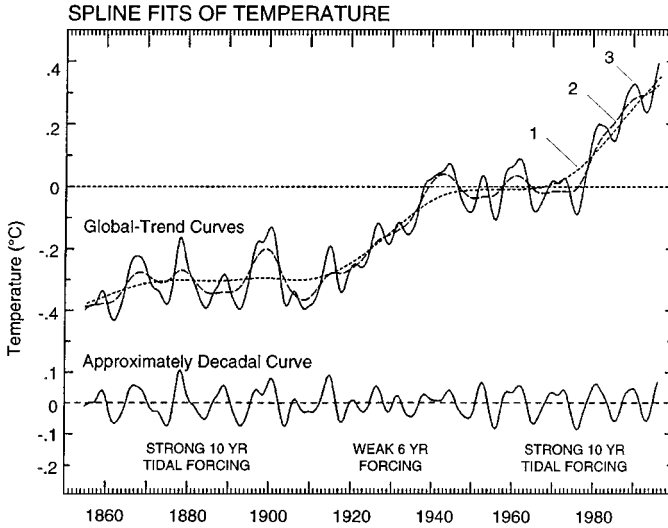


Figure 14 Trends in global temperature shown by spline curves fit to anomaly data. *Upper plot:* curves (1 to 3) of decreasing stiffness to match as well as possible the curves of Figure 12, but extended back to A.D. 1855. *Lower plot:* approximately 10-year fluctuations revealed by subtracting Curve 2 from Curve 3. Note the replacement of such fluctuations with 6-year fluctuations from 1920 to 1940 during a time interval of global warming. Below the plot are shown time intervals of strong and weak tidal forcing on a centennial time scale. (Source: Ref. 43 and 45)

showed approximately 10-year temperature fluctuations during the nineteenth century, as well as recently. But strangely, the curve showed approximately 6-year fluctuations in the 1920s and 1930s. A spectral analysis of the whole record yielded oscillations in the band-pass region with periods of 6.0, 9.3, and 10.3 years, phased such that when combined, they created nearly the same pattern as the band-pass curve, supporting the latter as a reasonable portrayal of both the 6- and 10-year fluctuations (43).

There is a school of thought in meteorology (47, p. 72) that the circulation of the oceans and the atmosphere, and hence temperature, can change on near-decadal time scales solely as a result of their interacting with each other as a dynamically coupled system. This view does not, however, preclude the possibility that external forces also influence temperature on similar time scales. We found suggestions in the scientific literature of astronomical causes of decadal variability in temperature linked to variations in the number of spots on the Sun and the strength of the oceanic tides. We decided to examine these claims.

The interruption that we saw in decadal fluctuations in temperature, beginning in the 1920s, had already been noted by others (27) and was almost surely real.

This was not an encouragement to finding a decadal connection of temperature with the sunspot cycle, which showed no such interruption and anyway has an average periodicity slightly longer than the 10-year periodicity that we found in the temperature data. We turned our attention to exploring a possible tidal connection with temperature, encouraged by a relevant discussion in the same treatise on climate where Bacastow had discovered the Southern Oscillation (46). Also, in an article by Loder & Garrett (48) we found mention of a plausible mechanism: that strong tides may cause vertical mixing of stratified surface ocean water with cooler deeper water, sufficiently to cause appreciable transient cooling at the sea surface.

Periodicities abound in the astronomical forcing of oceanic tides by the Sun and the Moon, but to our surprise the only nearly decadal periodicities in tidal forcing that we found were at 9.3 and 10.3 years, very close to the spectral periods that we had found for temperature. Moreover, these two periodicities reinforced each other near 1880 and 1970 but cancelled each other out near 1920, as did the spectral oscillations in temperature. Most surprising, a 6.0 year tidal periodicity replaced the cancelled out decadal periodicity in the 1920s and 1930s; it was phased such that, by causing periodic cooling, it might explain the 6-year fluctuations in temperature seen in our decadal spline curve from about 1920 to 1940. We had perhaps found a plausible tidal mechanism that could explain all of the main features of our band-pass temperature curve. Encouraged by this success, we began to look for additional features of global temperature that might be explained by an hypothesis that strong tides cause cooling of surface sea water.

We were now attracted to the El Niño time scale by work of a graduate student at Columbia University, Ms. Ami Ffield (16). She gave us a preprint of an article that she and her supervisor had submitted to a journal proposing that tidal action in the west Pacific Ocean might affect the timing of El Niño events. The paper, although afterwards rejected by referees, seemed to us to have merit.<sup>16</sup> We could, however, not find any convincing tidal relationship related to global temperature to support her thesis, although we found hints of one.

Another feature to investigate as a test of our tidal hypothesis was a step-like upward trend in temperature seen in the stiffest spline of Figure 14 (an extension back to the 1850s of the bowshaped spline curve in Figure 12). Could this step-like feature be related to the appearance and disappearance of decadal tidal forcing? During times of this strong decadal forcing, from about 1870 to 1910, and again from about 1950 to 1980, the stiffest temperature spline showed no overall warming. In contrast, from about 1920 to 1940, when this strong forcing gave way to weaker 6-year forcing, the stiffest spline shows pronounced

<sup>16</sup>The idea is discussed in Ffield's doctoral thesis (17).

warming. The coincidence of this warming trend with 6-year fluctuations in temperature was striking, as though both features were related to the weaker tidal forcing (see Figure 14). Beginning in the late 1970s decadal fluctuations have been accompanied by overall warming, in violation of our tidal hypothesis, but this warming could reflect an enhanced greenhouse effect beginning measurably to affect global temperature.

Two additional temperature features that we thought it worthwhile to investigate were the cool period in the Middle Ages called “the Little Ice Age” and the preceding warm period extending back to the Roman era. Could these alternations of warm and cold climatic conditions be partially a result of tidal cooling on centennial and millennial time scales?

To explore this possibility we have extended the time frame of our analysis over several tens of thousands of years, an investigation still in progress. Looking both forward and backward, we have verified the existence of a pervasive tidal periodicity of approximately 1800 years that we had tentatively identified earlier (45). We have also found pervasive, approximately 90 and 180 year periodicities related to alternations between stronger decadal and weaker, shorter period tidal forcing in which the maximal decadal forcing varied from century to century. The maximal decadal forcing in the 1970s, though stronger than that of the 1880s, was not as strong as that in the 1790s. Still stronger forcing, with peaks near 1610 and 1430, was the greatest during the past two thousand years. This maximal forcing on the millennial time scale, in support of our tidal hypothesis, occurred during the Little Ice Age, while the lesser forcing in the previous thousand years occurred during the warm Roman era.

What may happen in the future if our tidal hypothesis is correct? Our analysis suggests that if tidal forcing actually contributes to the global heat balance, the world faces the prospect of substantial and increasing natural warming added to greenhouse warming for the next three centuries followed by only slight natural cooling for three more centuries. Only after A.D. 2600 will tidal forcing as strong as that of the 1970s recur.

As these speculations indicate, we have been led in unexpected directions by our pursuit of time-series data on atmospheric CO<sub>2</sub>. The CO<sub>2</sub> signals that we measured were slight but persistent. We dared to believe that they might be real, because we deemed our data to be precise enough, and sufficiently well calibrated, to show weak patterns, if they existed. Then we risked proposing that temperature also varied decadal because it correlated with our CO<sub>2</sub> data, and we looked for and found a possible cause.

It was a fortunate circumstance that we received a substantial part of our funding from the National Science Foundation while engaged in this exploratory study. NSF, as we knew, encourages exploratory research, because its mission is “basic science,” a mission of which we have, paradoxically, had great difficulty

proving our monitoring of atmospheric CO<sub>2</sub> to be a part. Of course, not all activities ascribable to basic science turn out to be successful. If, however, our tidal hypothesis of climate change should turn out to be correct, we would owe our discovery to having repetitively measured atmospheric CO<sub>2</sub>.

### *Where Do We Stand?*

It has been over 30 years since I speculated before the American Philosophical Society that the world by the end of the 20th century might be in greater danger from rising CO<sub>2</sub> than it was in 1969. Where do we stand on this issue today? My friend Bert Bolin, joined by others around the world, a few years ago started a political process drawing attention to possible dangers of rising CO<sub>2</sub> and urging that the use of fossil fuels be scaled back, or at least stabilized (6). Governments worldwide have recently tried to initiate this stabilization process, meeting in Kyoto, Japan, to agree on the wording of an international treaty to restrict fossil-fuel use. I have not been a part of this political process, but I would like to add here a few thoughts on whether rising CO<sub>2</sub> and attending climate change, especially possible global warming, should be viewed with concern.

Not everyone agrees that there is a global-warming problem. There are probably even some who doubt that atmospheric CO<sub>2</sub> concentrations are rising. Not long ago, while attending a scientific conference on climate, I encountered a meteorologist employed by a utility company who was examining a poster showing a curve of rising atmospheric CO<sub>2</sub> labeled "Mauna Loa Observatory." He was pointing out to the author of the poster that one should be cautious about interpreting this curve because of a steady increase in local automobile traffic near the observatory. I could not fault him for raising this concern, because I was at the time protesting to NOAA a lack of control over this increasing traffic, but he should have acknowledged that CO<sub>2</sub> measurements at other sites, with no possibility of local contamination, corroborated that the rate of rise seen in the Mauna Loa record was global.

There is greater justification to doubt that air temperatures are rising globally, because many more reliable measurements than for CO<sub>2</sub> must be averaged to prove that the apparent upward trend in the data is significant. I am convinced that temperatures are rising significantly, not just from viewing temperature data, such as that plotted in Figure 14, but also because the atmospheric CO<sub>2</sub> record makes any other interpretation difficult. The increase in amplitude of the seasonal cycle from the 1960s to a peak in 1990, about 20% for Mauna Loa, as shown in Figure 13, and nearly 40% in our longest northern polar record (44), is almost surely too large and too well correlated with decadal varying temperature, to be accounted for solely by plant growth gradually stimulated by higher CO<sub>2</sub> concentrations, or by other chemicals introduced into the environment by human activities, such as nitrogen compounds. The advance of

the growing season during recent unusually warm years also points to rising temperature as being significant.

These arguments, and others that address the significance of recent temperature trends, are not easily explained to a nonscientific audience, however. For example, my own attempts to explain my research to interviewers working for the public media hardly ever produce in-depth reports. Through the press, radio, and television, the public receives little more than snips of information on which to judge the significance of rising CO<sub>2</sub> or temperature changes. The tendency of the news media to seek out alternative sides to every issue without evaluating pros and cons makes almost every aspect of the global-warming issue appear controversial. People in general are poorly informed, and many are probably confused.

In Montana, for example, on land once owned by my grandfather, I have been sampling forest air with flasks in the manner of my early studies at Caltech. One day last year a neighbor saw some of these 5-liter glass spheres lying about, curiously wrapped in adhesive tape for security against breakage, and asked my wife what they were for. She said, trying to keep the explanation simple, that I was carrying out a scientific study having to do with global warming. He replied with some surprise, "Oh, I thought that problem had gone away."

A recent two-page presentation of environmental issues, in this neighbor's local newspaper in commemoration of an annual national event called "Earth Day," would give him little reason to change his mind. On half of the first page, above advertisements urging readers to buy compost, irrigation pipe, vitamins, power mowers, and the like, was an article noting that the day was an anniversary of the birth of the great communist V. I. Lenin, and that an 80-year-old elm tree had recently been saved by pruning its roots out from under an endangering sidewalk. The article was necessarily short because of the ample space given over to a photograph of the elm tree. On the second page was a more substantial article on "Myths and Facts about the Environment." Along with "myths" regarding wetlands, ozone depletion, and pesticides, was the "myth" that "the temperature of the planet has been rising at such an alarming rate that the United States and other nations must act immediately to reduce greenhouse gas emissions." Refuting the "myth" was the "fact" that reliable data show either cooling or only insignificant warming. The article had been provided to the local paper by a "national center" in Washington, DC, and probably appeared widely in newspapers across the United States.

Should scientists attempt to help the public to understand better the significance of rising CO<sub>2</sub> and the global warming issue? Understandably, many do not wish to take a position regarding a possible peril associated with these issues. Even to publish scientific findings that suggest a peril in rising CO<sub>2</sub> or temperature can be construed as taking a prejudicial position.

A safe approach is just to remain an interested observer of the unfolding scientific evidence of man-made global change and its possible significance to future human welfare. Without risk one can comment dispassionately on sociological, political, and religious perspectives of the global warming issue, for example, as an historian might, beginning with the first hints of man-made global change and progressing toward the time, not yet arrived, when there may be convincing proof of global warming. (Perhaps convincing proof will be acknowledged to have arrived when a substantial number of US Congressman are discovered to have secretly purchased real estate in northern Canada.)

I believe, however, that a more prudent attitude would be to heed the rise in atmospheric CO<sub>2</sub> concentration as serious unless proven to be benign. If scientists would make clear to the public the wisdom of this cautious approach, people would demand to be better informed about what scientists already know. The collective talent and wisdom of a species self-named *Homo sapiens* might then be better directed toward the issue of global warming.

The consumption of fossil fuel has increased globally nearly three-fold since I began measuring CO<sub>2</sub> and almost six-fold over my lifetime. In Southern California it isn't necessary to look at statistics to sense this enormous increase. When my family and I first moved here over 40 years ago, we could stand on a vantage point above the Pacific Ocean a few miles north of the city center of San Diego and look eastward over an expanse of hills and distant mountains accessed by only a few country roads and inhabited mainly by farmers and wildlife. Revisiting this vantage point at night many times since, I have watched the number of lights steadily increase; lights from new homes, lights from commercial enterprises, lights from vehicles after an eight-lane highway was built just to the east 30 years ago. I have repeatedly asked myself how long these increases can go on.

Almost all of these lights, and the activities they support, depend on fossil fuel energy. It is to protect such activities that people oppose restrictions on the use of fossil fuel and hope, and even assert, that greenhouse warming is a myth.

Realistically, the greatest myth is that natural resources and the ability of the Earth's habitable regions to absorb the impacts of human activities are limitless. Harrison Brown, who wrote about *The Challenge of Man's Future* (10) while I was a postdoctoral fellow at Caltech, was quite sure that fossil fuel could not be used at an ever faster rate indefinitely. He contemplated the fossil fuel era from its early stages to its peak and thence to its inevitable decline (10, p. 169), showing this progression graphically with two plots (Figure 15) adapted from an article in 1947 by the farsighted petroleum geologist King Hubbert. The first of these plots shows the fossil fuel era predicted to last about a thousand years;

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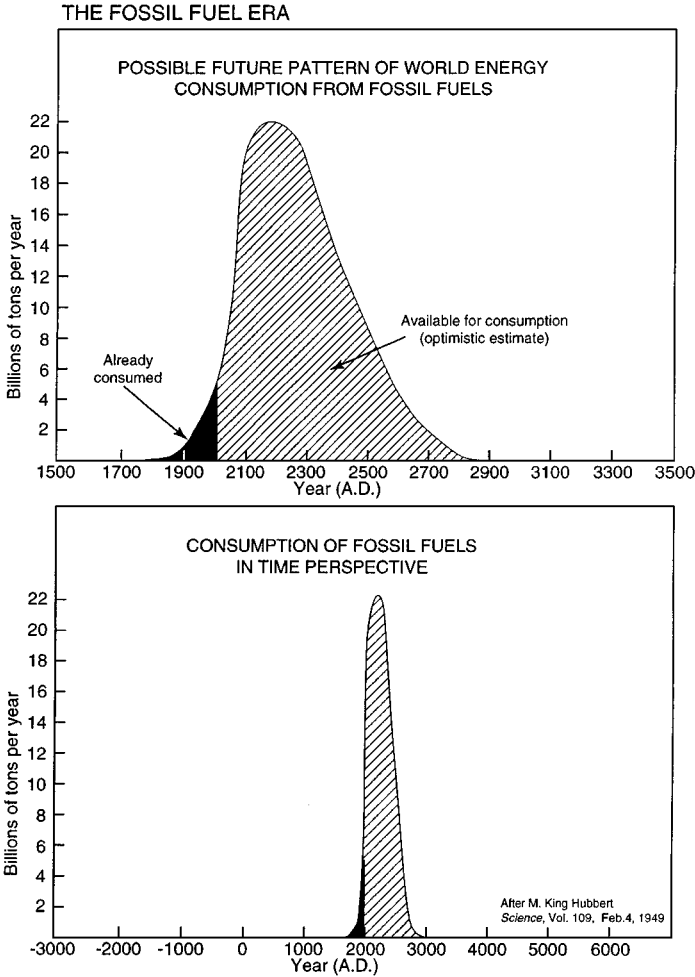


Figure 15 Rise and fall in the global rate of consumption of fossil fuel estimated for the entire era of its use. (Source: Ref. 10)

the second shows how brief this era appears when viewed over ten thousand years.

In historic perspective, the fossil fuel era will probably span less time than elapsed from the beginning of the Roman Empire in the 1st century after Christ to the fall of Constantinople to the Turks that removed the Empire’s last vestige in the 15th century. Good times for fuel users will be over in less than half that time. But we live today still in an early stage of this era, shown on the plots by

the termination of black areas under the curves of fossil fuel usage (the timing updated from Brown's graph).<sup>17</sup> The curve is rising today almost as rapidly as it ever will or can. In essence, those who oppose the restriction of fossil fuel use want this curve to continue to rise as depicted in these plots, checked only by the inevitable depletion of fuel reserves. Brown asserted, and I agree, that fossil fuel use should be restricted as much as possible simply so that it lasts as long as possible, whether or not adverse environmental consequences result from using it rapidly. The likely danger of man-made global warming would then be significantly reduced as well.

Meanwhile, what about at least monitoring what is happening to our environment to prepare for possible change? It has been over 40 years since Roger Revelle and Hans Suess (56) pointed out that the burning of fossil fuels was a large-scale geophysical experiment that "if adequately documented may yield a far-reaching insight into the processes determining weather and climate." There was no sense of peril then, just a keen interest in gaining knowledge. Now, four decades later, there is a hint, perhaps more than a hint, of peril. Nevertheless, and despite the heightened political awareness of the greenhouse problem indicated by the Kyoto meeting last winter, most governments have shown little heightened interest in environmental monitoring.

Even now my own studies, especially those involving carbon in the oceans, which I have not had space to write about here, are again in jeopardy. This time the cutting back of support is not directed at a few academic persons competing with government-based science, but reflects a generally declining support for such time-series studies.

Examples, such as my effort to maintain repetitive observations of carbon dioxide, have evidently done little to increase enthusiasm for time-series measurements in general. I cite one startling instance: measurements of temperature itself. One might rationally expect that a major effort would now be under way to improve "routine" measurements of air temperature worldwide. Such is not the case. Until now, temperatures have been measured worldwide mainly to achieve short-term benefits, e.g. to forecast weather and aid agriculture. Management of these measurements is institutionalized within agencies that do not adapt easily to a new need for long-range climate studies. Most shocking is that even the existing weather-observational programs have recently been seriously degraded over a wide area owing to budget cuts and unstable, indifferent, or financially distressed governments.

<sup>17</sup>As Brown stated: Consumption of the earth's stores of fossil fuels has barely started; yet already we can see the end. In a length of time which is extremely short when compared with the span of human history, and insignificant when compared with the length of time during which man has inhabited the earth, fossil fuels will have been discovered, utilized, and completely consumed. The "age of fossil fuels" will be over, not to be repeated for perhaps another 100 million years (because it will take that long for nature to create a new fossil fuel resource).

Perhaps my success in sustaining time-series measurements will eventually raise the general scientific regard for making repetitive but important environmental measurements. Also, I hope that there will always be opportunity for individual scientists to pursue scientific leads not anticipated by committees and agencies.

## EPILOGUE

I have tried to be as objective as I could be in this narrative, but I apologize if, in my recollections, I have unfairly recalled, or never properly understood, activities recounted here. I hope that my text makes clear that I had a great deal of help in trying to stay in carbon dioxide research. Much of this help came from personnel below the management level in the agencies opposing my research. I also received crucial assistance from interveners, not all of whom I have had space to mention. On a more personal note, I express my appreciation to John Harte, who after suggesting that I write this narrative, helped me in this understandably difficult task, given the necessity to criticize the actions of others, however well meaning their intentions may have been. I also appreciate reviews of my text by Robert Socolow, Devendra Lal, and Ralph Keeling. Contributing to my research at the Scripps Institution of Oceanography, besides those mentioned in the text, are David Moss, who as the project engineer for 32 years, assured that my program had the best possible instruments within budget, and Alane Bollenbacher and Peter Guenther, who carried out many of the measurements in the laboratory for 20 and 30 years, respectively. Also, John Chin of NOAA, recruited for work at Mauna Loa, who has taken care of our equipment at Mauna Loa Observatory ever since the budget crisis of 1963, and is probably more directly responsible than anyone else for creating the Mauna Loa CO<sub>2</sub> curve that has been credited to me. I would like to close by expressing my deep gratitude to Louise Keeling, my wife for almost my entire career, whose patience, sympathy, love, and understanding gave me much needed support in my pursuit of the carbon dioxide molecule in all of its ramifications.

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