EARTH SYSTEM SCIENCE AT 20 ORAL HISTORY PROJECT
ORAL HISTORY TRANSCRIPT

SHELBY G. TILFORD
INTERVIEWED BY REBECCA WRIGHT
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The questions in this transcript were asked during an oral history session conducted at the Earth System Science at 20 Symposium, held at the National Academy of Sciences, Washington, DC. Dr. Shelby G. Tilford has amended the answers for clarification purposes; therefore, this transcript does not exactly match the audio recording.

WRIGHT: Today is June 23, 2009. This oral history is being conducted with Dr. Shelby Tilford at the National Academy of Sciences in Washington, D.C. This interview is part of the Earth System Science at 20 Oral History Project, being conducted to gather experiences from those who significantly were involved in various efforts of the launch and evolution of Earth System Science. Interviewer is Rebecca Wright, assisted by Jennifer Ross-Nazzal. Also present is Dr. Ming-Ying Wei, NASA Office of Earth Science.

We thank you for taking a break in the scheduled activities this morning to be with us. We know that you are considered to be one of the key founders of the Earth System Science concept. How did you first get interested in this field?

TILFORD: It was purely by accident. I was always interested in science and originally started out to be a chemical engineer, I thought, in high school. But when I got to college, I really didn’t know what I wanted to do, so I majored in chemistry, math, and physics at Western Kentucky [University, Bowling Green, Kentucky], and then went on to graduate school at Vanderbilt [University, Nashville, Tennessee].

Instead of doing what I expected I would do, pursing a graduate degree in chemical engineering, I became interested in physical chemistry. That area looked most attractive at that particular time. There were several physical chemists at Vanderbilt at that time. The one that I chose for a director, Dr. K. Keith Innes, had been there a couple of years. He was fairly young. He was a spectroscopist. I barely knew what spectroscopy was, but, it sounded interesting to me. The University had just obtained a brand new high-resolution vacuum ultraviolet spectrograph that he had been able to support by a grant from NSF [National Science Foundation], so I thought this would be a great opportunity. He was very interested in very complex molecules, which are more difficult than atoms, or diatomic molecules or tri-atomic molecules.

I wound up doing my PhD thesis on the spectra of dinitrogen substituted benzene rings. When you substitute two nitrogen atoms in a benzene ring, you wind up with three different molecules because you can put them next to each other; you can separate them by one carbon or you separate them with two carbons. You get three different kinds of spectral signatures for the three different molecules.

I spent the first couple of years really learning the ropes. Then my graduate director came in and told me that he was going on sabbatical at the end of next year, and I could either finish up my thesis before he left or wait until he got back. There was not much question of what to do, so I finished my PhD requirements and thesis in one year.

I applied for and was awarded a post-doctoral research position at the [US] Naval Research Laboratory [NRL, Washington DC], where they too, had just obtained a similar new high-resolution vacuum UV spectrograph. Very rapidly, of course, I decided that polyatomic molecules were much more difficult than simpler molecules, so I started working on diatomic molecules and atoms, which were much more important in understanding Earth's atmosphere and the sun. Fortunately, when I went to the Naval Research Lab they had two of the people who essentially started space science. One was my boss, Richard [Dick] Tousey, a solar physicist, who fired the first ultraviolet spectrograph into space on a captured V-2 German rocket in, I think, 1947 or '48. His boss was [Herbert] Herb Friedman, who had done similar experiments, but looking at much higher energy processes in space. He was an x-ray astronomer. Later in his career, he was one of the principals in the National Academy of Science. It was a great start.

We started working on molecules of atmospheric interest. Also, at the same time, NRL had an old grazing-incidence spectrograph that could look at photograph spectra all the way down into the soft x-ray region of the spectrum. I got involved in looking at the spectra from all kinds of highly excited atoms. The main reason for this was that in the spectra that Dick Tousey and his colleagues had taken several years earlier, (it was the first complete echelle spectra of the ultra-violet energy output from the sun), there were a lot of unknown spectral lines that no one understood. We spent a lot of time trying to find out what the sources were for a lot of these lines. It turned out that molecules like carbon monoxide and many atomic components were present.

In the atmosphere, no one had ever photographed a high-resolution detailed spectrum of molecular nitrogen, carbon monoxide, molecular oxygen (to some extent) but not completely, and a number of other molecules and atoms. There are a lot of metals in the sun, so we did some very high-temperature studies of the absorption spectrum of various metals. We obtained and analyzed a lot of other spectra, but it was the molecules in the atmosphere that really interested us. I think we did a pretty good job of categorizing, cataloging, and identifying most of the lines of atmospheric molecules in the spectral region above 1000 angstroms.

By now, I'm in my late 30s, and I've been a scientist my entire career, and if I ever want to do something different, this seemed like the right time to try it. The solar physics program managers at NASA wanted a detailee to come down for a year. They didn't have any permanent positions, but they needed help in solar physics. They were just getting ready to select the instruments for the Solar Max [Solar Maximum Satellite] Mission. I didn't want to go for an entire year, so I talked another scientist at NRL into sharing the detaileeship with me. He went for the first six months, then I arrived at NASA Headquarters in January of 1976.

During that six months, the NASA officials talked me into staying in the solar physics program at NASA, but that was interrupted a few months after I had agreed to accept the position. That's when NASA was assigned the responsibility for trying to understand the ozone issue, and the Upper Atmospheric Research Program was established. They wanted someone who knew a little bit about the atmosphere.

Well, I didn't know much of anything about how the atmosphere behaved on a global basis, but I knew quite a bit about what was in it, so they said, "Why don't you come over to this new program?" I did, and it was fascinating.

All of the controversy at that time was about ozone depletion, and were CFCs [chlorofluorocarbons] responsible. The reason that NASA ultimately got the responsibility was because NASA was working on the potential environmental effects of the Space Shuttle whose exhausts contained chlorine and depleted ozone. They wanted to understand what the affect of these missions would be on the atmosphere. How detrimental was it? Is it a big problem? Is it permanent? Is it this? Is it that?

Before NASA took the lead in the ozone question, the FAA [Federal Aviation Administration] was the lead agency [through the Climate Impact Assessment Program (CIAP)] because there was a similar question about the nitric oxides in jet engine exhaust. We spent several years studying ozone. We funded a lot of university researchers, a lot of scientists in NASA, NOAA [National Oceanic and Atmospheric Administration], and many other agencies. We had numerous advisory groups and many NASA, national and international scientific meetings, which eventually culminated in the banning of CFCs on a global scale. That was really the first time that any science input had ever made a significant contribution in any global political decision, so it was fun.

At this time, within Headquarters, NASA had its Earth Science and observations programs distributed in various offices within Headquarters. There was an applications program, which essentially flew new instruments as demonstrations of new technologies, and for the most part other people performed the data analysis. Some research in atmospheric, ocean, and solid Earth Science projects were carried out at Goddard [Space Flight Center], JPL [Jet Propulsion Laboratory], Langley Research Center, JSC [Johnson Space Center], Ames Research Center and smaller efforts at each of the other NASA Centers. Most of these efforts were performed without the benefit of scientific peer reviews. There was a piece in space science, which did the upper atmosphere research program. The aircraft program flew airplanes to make observations, test new instrumentation, and do calibrations of satellite observations.

In late 1978 NASA consolidated all of these Earth Science components, except some dealing with life sciences, into one office. At that time, I accepted the responsibility for all atmospheric research in total. Additional, responsibility for the Oceans Program came a couple of years later, along with the Aircraft Program and Solar-Terrestrial Physics except the Solar Physics Program I had been involved with earlier. A short time later, they decided to consolidate the land programs into my area, so the integrated Earth Science Program just evolved over several years as the interest of Earth Science increased within NASA.

WRIGHT: Share with us a little more about that time period when CFCs were banned, and what that meant, not just on the impact on society, but also on the impact on the scientific field. As you mentioned, it was the first time that that had happened.

TILFORD: It was a whole change in attitude, because this was the first time they had ever banned anything on a global basis, based upon scientific evidence that it would be damaging to the global environment. It was wonderful. I think what we did at that time, is that we brought in a lot of external scientists. We ran a program office, but at the same time we sometimes tried to provide the scientists a little guidance, regarding the directions that we and the advisory groups thought were important.

We had a full-up peer review program. If you wanted to be funded, you had to write a decent proposal that had to be reviewed by your colleagues, and it had to be genuine. We took what the National Science Foundation was doing, and said, “That seems like a good approach.” We got the scientists involved to a great degree.

The reports that came out were fairly uniform. There were always a few dissentions. It’s amazing. The same individuals who lobbied against the fact that they didn’t think the ozone was being destroyed with CFCs are the same individuals that testified before Congress that cigarette smoke doesn’t give you cancer. This includes many of those same individuals who proclaim that CO_2 [carbon dioxide] increase in the atmosphere has nothing to do with global warming or the monumental melting of arctic ice. There is a group of scientific lobbyists that are paid by certain industrial organizations, those who pay people to come up with the "right" answer.

We were employed to come up with the right answer, but we didn't have any axe to grind, and most of our program managers tried to take the approach of "give us the facts of your investigation and then let the scientific community integrate those with other results until we can reach a consensus." This approach eventually led to the Intergovernmental Panel on Climate Change [IPCC] approach to climate change assessment.

Essentially, what we tried to do, was give an assessment of the ozone issue. This involved a lot of scientists. Some of them worked in NASA. Most of them worked outside of NASA. We came up with approaches using balloons, *in situ* measurements, rockets, aircraft, satellites, laboratory measurements, and continuously improved model simulations. Fortunately, a few years earlier, NASA had put a solar backscatter ultraviolet instrument on the Nimbus satellite. After many years, and after many, many iterations of data analysis, we came up with a pretty good measurement of global ozone variability.

Then, the ozone hole came along, and we said, "This is really serious." Beginning in the late 1970s, we proposed the Upper Atmospheric Research Satellite [UARS], which essentially was a set of instruments to measure many stratospheric components, by several different instrument approaches, IR [Infra-Red], microwave, backscatter, etc., to measure global stratospheric winds, to measure and completely analyze all of the aspects of incoming solar radiation. What impact does it have? Is it constant? Finally, in 1991 we were able to launch it.

We supported studies of many of the chemical species, the free radicals, ozone, OH [Hydroxyl radical, the neutral form of the hydroxide ion (OH^{-}) and are highly reactive and consequently short-lived], both chlorine and nitrogen compounds, et cetera, that we thought were important. We had a fairly large *in situ* laboratory program, which included not only studying reaction rate constants, but all kinds of kinetics, temperature effects on these reactions, and so on. It also involved spectroscopy. Our laboratory program was about a quarter of the total research program for the Upper Atmospheric Research Program.

Another quarter was in modeling. We thought that if you can’t model it, you don’t understand it, thus you need to study it further or it doesn’t make much sense to study it. We funded a large number of modeling groups, both in the university community, in NASA, NOAA, and numerous other Federal agencies, as well as some industry efforts. Then the final part was a field measurements program, which included, as I said before, balloons, aircraft, *in situ*, etc. Anything that we could utilize in order to understand if ozone was varying, and if it is, why.

In fact, before I left NRL, my colleagues and I tried one experiment when there was a small program in the Department of Transportation. We designed what we called a multiple path cell. We were going to titrate *in situ* ozone in this multiple path cell, by releasing a small measured amount of nitric oxide through a complex mechanism, and measure, we thought, the decomposition rate of ozone in the upper atmosphere. Well, the balloon started drifting. We lost the control signal; ballast was being released from the bottom of the gondola and was simultaneously being released from the top of the balloon, and so on. Therefore, we didn’t get a lot of information from this experiment. It was an interesting experiment and experience flying a balloon from Palestine, Texas [National Scientific Balloon Facility].

I tried to get involved a little bit with the ozone issue before I came to NASA. The first thing I did at NASA really was in solar physics. Dr. Adrienne [F.] Timothy, who was the program manager at that time, said, “I don’t have time to do the solar constant selection for the SMM [Solar Maximum Mission] which was to investigate various aspects of the sun’s variability. We want to put a solar constant measurement on SMM." So I carried out that part of the evaluation and selection process.

I was involved in the selection of, really, the first solar constant measurement on a continued basis, which we still have continuous observations after about six or seven overlapping flights of a solar constant measurement, but we may not have much longer. We have had continuity. We've gone through, I think, four different principal investigators at four different institutions in all of these years. It really is a solar issue, but it's extremely important to Earth. If we don't know whether the sun is varying or not, how are we going to determine if anything else is varying? And what are the impacts of such variations?

WRIGHT: Do you recall what year that was that you started that?

TILFORD: 1976.

WRIGHT: Right when you got here.

TILFORD: I started the next week, essentially. Then we completed that selection before I transferred over to the Upper Atmospheric Research Office.

WRIGHT: Were you given a lot of room to establish some of the areas that you wanted, or were you pretty much directed?

TILFORD: In the Upper Atmospheric Research Program, yes. This was a program mandated by Congress. There was a lot of competition between the Department of Transportation, the Department of Energy, NOAA, NSF, and NASA. But because NASA had potentially the biggest impact, through the rockets and the Shuttle, it was assigned to NASA.

There was a lot of lobbying for, and against, among the several agencies that wanted to head the program. At that time, for the first time, NASA was directed to do a specific program relating to Earth Science. It is part of the NASA charter that was modified in 1976, [Article 4 of the NASA Organic Act was passed into law in 1976.]. The Congress actually changed the NASA charter to include the Upper Atmospheric Research Program and the Earth.

There are about three and a half pages [of report language] telling us exactly what they wanted—not how to do it—but what they wanted. They wanted to find out, is it? What is it? How bad is it? And what are corrective procedures? Yes, it was pretty neat.

Actually, NASA had had another scientist in from JPL [Pasadena, California], Dr. James King [Jr.], who came in on a temporary basis initially to organize the program, but they didn't have a charter at that time. He only came in for a few months as a detailee. He was going to leave, and Headquarters detailed Ron Greenwood from Langley [Hampton, Virginia]. Ron didn't have a lot of scientific experience, and he talked me into joining the new Upper Atmosphere Research Program [UARP], which was in the Office of Space Science. When Ron left in late 1981, I was asked to head up the UARP program.

With respect to the Upper Atmosphere, we had complete free reign within scientific constraints. This was also true as we moved from demonstrating space techniques into doing research on weather and climate and all of the other areas of Earth Science. At that time, the ocean program was under Dr. Stan Wilson, who was one of the other members of the panel today

[*NASA Earth System Science at 20: Accomplishments, Plans and Challenges*, National Academy of Sciences, Washington, DC]. He was responsible for the oceans, and I was responsible for the atmospheres, and so on. Once Landsat 4 was launched, NASA combined everything into one single Division with several branches: Atmospheric Dynamics and Radiation Branch, Upper Atmosphere/Research/Tropsopheric Chemistry Branch, Oceanic Processes Branch, Flight Programs Branch, Land Processes Branch, Geodynamics Branch, and Space Plasma Physics Branch.

Later the Space Plasma Physics Branch was transferred to the Space Science Program which already included the Solar Physics Program. That program was moved over to the Earth Science Program and then it moved back to Space Science. But at the interface, it is difficult to separate the stratosphere/mesosphere from the ionosphere. They do interact at the upper levels of the stratosphere/mesosphere, because it's a diffuse transition region.

I actually did a few experiments in airglow observations, interpretation-wise. When I was at NRL, we fired rockets into the atmosphere and tried to measure airglow as a function of altitude and determine how much atomic oxygen was at different altitudes under different conditions and how much atomic nitrogen was present, and so on. I wasn't involved very much in the experimental part of the measurements; I was involved in the analysis part so I did have a little experience in this area.

WRIGHT: As the head of these programs, were you able to do what you had grown to love through these years, or were you having to do more of the program side?

TILFORD: Very quickly it got all almost completely into the program side. I mean, we made science decisions and programmatic decisions at the program level. As far as hands-on, no, I didn't do anything, but I wasn't too unhappy about that. I was enjoying what I was doing.

Finally, things were rearranged again, and again, and again, in terms of NASA's organization. In 1992 the Earth Science Division was made into a separate office, the Office of Mission to Planet Earth, and then after I retired, it was recombined with Space Science into a single office again. I think they have to do it every few years or they're not happy, but that's my personal opinion.

Anyway, how we really got involved in this program big-time, I think, were discussions with John McElroy, a former NASA employee, who had gone over to head up NOAA's NESDIS [National Environmental Satellite, Data, and Information Service] program. NOAA was discussing at that time what they were going to do for the next generation of geostationary satellites. Originally, NASA pioneered in the Office of Applications the geostationary observations, and Professor Verner E. Suomi from University of Wisconsin, granddaddy of geostationary observations, had designed this sensor (camera) to put in geostationary orbit. There were a couple of talks that mentioned it in the last few days. He was quite a talented and unique individual who made so many contributions for the advancement of Earth Science, from the observational, data processing and storage, and interpretational areas.

Anyway, they were now in the stage at NOAA where they were going to the next generation geostationary observatory. John wanted to do something better. Originally NOAA had a rotating camera, which actually was a fixed camera on a rotating satellite 22,500 miles above the Earth, which during every revolution, looked at a slightly different position on the Earth (so that it covered a north to south area every 30 minutes). He wanted to set up a geostationary fixed satellite camera pointing toward the Earth. It was going to be big and complex. He and I talked a lot, and I felt we needed a more robust satellite program for Earth Science to do a lot more things.

At this time NASA appointed a new Associate Administrator, an energetic individual, Dr. Burt [Burton I.] Edelson, who was really a communication expert, but had had very little knowledge about Earth Science. But he was enthused about Earth Science and the potential of satellites to improve our understanding of how the Earth System works. He was a close friend of Jim [James M.] Beggs, who was currently the Administrator of NASA. Dr. Hans Mark, a former director of [NASA] Ames [Research Center, Moffett Field, California], was his deputy. At this time, the Space Shuttle was a big question as to whether it would ever fly or not, and especially how it would fly and where it would fly, when it would fly, and so on. They were having trouble keeping it sold because it was such a big program.

This was in 1981 and '82 when all of this took place. I had been at the agency now for five years. We had flown, or were soon to fly a number of instruments, several satellites, and had several things, including an ocean altimeter, SAGE [Stratospheric Aerosol and Gas Experiment] and UARS, the Upper Atmospheric Research Satellite, was planned to be launched. But we thought we should do more. So between myself and Dr. Dixon [M.] Butler and a few other people and Burt, we went up and talked to Jim Beggs about this new Earth Science initiative, which we couldn't sell under any circumstances. NASA had previously not been very interested in anything except technology demonstrations, as far as Earth Science. After the Upper Atmospheric Research Program, there was a little more interest.

This was the same year that they were going to do UNISPACE '82 [United Nations Conference on the Exploration and Peaceful Uses of Outer Space]. This was a big deal for space. All of the space agencies and all of the people related to that from every country in the world that had a space program were going to meet in Vienna [Austria] in the middle of 1982. Beggs reply was, "I don't know. We have the Shuttle issue."

At this time NASA was still wanting to fly the Shuttle in polar orbits. So John and I got together and discussed it in a little detail, and he said, "Now, we could even launch our polar orbiting satellites from the Shuttle, and we could make them serviceable so that we don't have to replace a satellite. We just replace the instrument." Much like what eventually has happened with the Hubble Space Telescope, but this was going to be in polar orbit.

I said, "Well, we could complement that with an integrated Earth Science Program, which would demonstrate new instruments for NOAA, and maybe replace Landsat [Land Remote Sensing Satellite], and do a few other things. If they're going to be serviceable, this will be a very economic approach."

But this was all wrong—well, we were figuring out how to do this. We could sell a program. He [Beggs] could benefit. It would all be nice. We discussed this, and he backed it, and I backed it as Dixon and Burt had done for some time.

Meantime, all of the Space Science people didn't want any part of the Shuttle because if they were going to put a telescope on the Shuttle and men are moving around, they're not going to be able to point the thing precisely and hold it. They had all of these arguments. Professor Tom [M.] Donahue, who was Chairman of the Space Science Board of the National Research Council of the National Academy of Sciences, opposed it very much. Of course, all of the astronomers didn't like it. Earth Science liked it.

But at the time, we didn't know yet that just because of mass and propulsion capability, that you would never be able to put the Shuttle in a polar orbit and service something, because there was no weight left. It's a whole lot harder to put spacecraft in a polar orbit than it is in a lower Earth orbit, because of all that momentum change you have to do, whereas, you can use the Earth's rotation to help you, when you fly in a low latitude orbit. And if you're going into polar orbit, it takes a much bigger rocket to get the same payload into orbit. The reusable Shuttle makes this problem much worse

Burt went up and talked to his old roommate from the Naval Academy. It turns out that Jim Beggs and Burt Edelson roomed together when they were at the [US] Naval Academy, so Burt went and talked to Jim. At that time, the Shuttle flying in polar orbit was still a good argument, so the Shuttle was going to fly in polar orbit. Beggs said, "Well, let's go do this. This is a good thing to do."

We got a group together and had a couple of meetings. We had about twenty scientists of various kinds: atmospheric, oceanographic, land people, and so on. Professor Richard Goody, who was at Harvard [University, Cambridge, Massachusetts], was involved because he had a lot of prestige with the astrophysics community and Burt thought much of him. Then Beggs talked the author of *Space*, James Michener, into going with us.

We all went to Vienna, and Michener and Goody presented our program, with all its benefits of a better understanding of the Earth System to the international community. We were shot down! It was really not very exciting. No one accepted it. They would not buy into it. They thought it was the United States trying to take over the world, and that we were going to keep all of the data, and we were going to have all of the information on their countries, and they didn't want that. Sounds a lot like India, or China, or North Korea today, but that was the attitude.

We sort of came home with our tails tucked between our legs. We said, "This isn't going to work." Beggs said, "Well, I'd still like to do it." So we all got together and agreed that we would set up this huge group of scientists from every aspect of Earth Science, get them together, and put together a plan for what this could and would do.

Now, at this time, it was also a real problem between the different disciplines in Earth Science—Dr. Ming-Ying [Wei] will tell you it still exists to a great degree, but not to the extent that existed in 1982 —that is oceanographers would barely talk to atmospheric scientists, and neither one of them talk to land scientists. In addition there was the geodynamics/solid Earth community which looked at things on a completely different time scale. There was simply very little interdisciplinary communications. It was three, or four, different, separate areas completely.

We then contacted Dr. Francis [P.] Bretherton, asked him if he would chair this new Earth Science Committee. We told him it would take about a year probably to get it completed. He was at that time, I think, head of NCAR [National Center for Atmospheric Research], and he had been closely associated with UCAR [University Corporation for Atmospheric Research]. His training was in applied mathematics, and he had published significant papers in both oceans related and atmospheric related topics, so we thought he was the right kind of individual to do this. Again, now we brought in a larger group. After thinking about it for a while, he agreed that it would be a difficult job, but he thought it would be a very worthwhile thing to do.

We also brought in people from all of the other agencies that would attend. NOAA, National Science Foundation, Department of Energy, FAA, USGS [United States Geological Survey], USDA [United States Department of Agriculture], all of the agencies that we thought might help us or complain about us. Then we set out to put together a science rationale for doing

Mission to Planet Earth. We didn't have a name for it. At that time it was the Earth Science Program. With Francis working with NASA, the science community, the National Academy of Sciences, we finally put together a very impressive group of scientists and Agency representatives and began the process. [Refer to page 51 for complete list of participants.]

Francis, the Committee, assisted by Ray Arnold, Dixon Butler, Stan Wilson, Bob Watson, me, and other members of our Division, along with various representatives from the Centers, worked and we worked and we worked. We formulated the Committee in 1982.

The first meeting was early in 1983. What we figured to be a one-year task wasn't. We didn't finish in 1983. We didn't finish in 1984. We had still not finished in 1985. But we said, we've got to get a report out.

At that time, 1986 the Bretherton Committee [Earth System Science Committee of the NASA Advisory Council] came out with an interim report; it was an overview of the Earth System Science Program. It was called, "A Program for Global Change: Earth System Science Program." It's a beautiful brochure, and it's got a lot of information in it. It tells what needs to be done, but we still had some real problems. Now, the whole thing was put together in what is now called The Bretherton Chart. [Refer to page 52 and page 53.]

It couples everything, almost every element of the Earth system, going from the sun to the center of the Earth. That includes the solar input, the effect of the upper atmosphere, lower atmosphere, the troposphere, et cetera. It covers weather, it covers oceans, it covers land, it covers the solid Earth. We tried to put together something that would integrate the Earth as an integrated science program. That's what the chart does.

But we still weren't finished, because we still had a lot of dissension between oceanographers and atmospheric scientists and solid Earth scientists in terms of priorities.

Everybody wanted to be first. Well, we didn't want everybody to be first. We wanted everybody to work together. This was hard.

So three years later, this came out [referring to document] that was called an overview. It was called, "A Closer View." This was primarily Francis Bretherton with lots of help from a number of individuals. Let's see, who else were the big players? We had many of them. I think all of the people are listed here. Let's see. There was oceanographer Dr. Jim [D. James] Baker, Joint Oceanographic Institutions, Inc.; meteorologist Professor John [A.] Dutton, Pennsylvania State University; biogeochemical cycles Professor Berrien Moore (III), University of New Hampshire; solid Earth scientist Dr. Kevin [C.] Burke, NASA Lunar and Planetary Institute; and remote sensing and atmospheric scientist Dr. Moustafa Chahine, Jet Propulsion Laboratory.

And we had expert people like Professor Jim [James J.] McCarthy, biologist; Professor Ron [Ronald G.] Prinn, a modeler; Dr. Willy [Wilford W.] Weeks, ice; Professor Paul [J.] Zinke, trees and living things; Professor [Lennard A.] Len Fisk, space plasma physicist; Professor [Daniel B.] Botkin, a medical doctor who got interested in ecology research, and Professor David A. Langrebe, land remote sensing. We had a whole group of other people from various NASA centers, numerous members of my NASA Headquarters staff and several support institutions and they all worked together. We spent many, many meetings and many, many hours together. Finally, this other draft came out. It's a much more complex, integrated chart than the one that I showed you.

All of this came together in 1988-89. This was a formulation by this group of scientists and about five or six [NASA] Headquarters people, including four of whom you saw at the table the other day—Dixon Butler, Stan Wilson, Bill Townsend, and myself. Dixon played a huge role in the EOS [Earth Observing System] program, before it became EOS, and with developing the data system after it became EOS. There are also people like Dr. Bob [Robert T.] Watson who had been extremely involved in the ozone issue and went on to chair many of the international assessment programs.

We were intimately involved at this time, too, with their International Programs Office, because, as part of this, we agreed very early on that we could not again do it alone in the United States. We had to engage the people that disagreed with us in Vienna. We set out to fly other countries' instruments, joint instruments, fly our instruments on their satellites, or their rockets or whatever. This took a tremendous amount of time. First, we had to deal with the different disciplines, because they all didn't agree or didn't think the other science was that important. Then, at home, we had to get together.

Fortunately, we had a great group of people in Washington at that time. We had representatives from NOAA. First it was John [H.] McElroy, and then after that it was William [P.] Bishop, [J. Michael] Mike Hall, and [Russell] Russ Koffler. We had the National Science Foundation. First was the director of geosciences, William [J.] Merrell, Jr. and he left shortly thereafter. At that time, [Robert W.] Bob Corell came in, and he was extremely involved, as was Nancy Ann Brewster Budden. We had Dr. Ari Patrinos of the Department of Energy involved. We had representatives of the FAA and USGS. The main thing, we also had OSTP [Office of Science & Technology Policy] in the person of Richard G. Johnson and the Office of Management and Budget [OMB], in the person of Dr. Jack [D.] Fellows who were a tremendous help at all stages of this process. We had some Congressional staffers there. We set up this integrated approach which included many international participants.

Let's talk about international for a minute. We had had discussions with the English, and they actually had an instrument on UARS, a radiometer to measure certain minor species with a state-of-the-art instrument called a pressure modulated radiometer. I won't go into technical details, but it was unique. And, it had worked in the laboratory, and we thought we could adapt it to work in space. They funded it, which was wonderful.

We had talked with the French, and we had talked with the Germans. We had already agreed to fly some Germany provided Earth Science instruments on the Shuttle. Then we talked to the French, who had agreed to jointly fund TOPEX/Poseidon [Ocean Topography Experiment]. In fact, we built the instrument, and they launched it on their satellite, but with some of their software. We talked to a number of scientists, which we signed up as part of the science team, in other countries. We have also had a very good working relationship with Japan in so many ways. Japan provided the ADEOS [Advanced Earth Observing Satellite] spacecraft for the US NSCAT scatterometer and the joint Japan/US TRMM [Tropical Rainfall Measuring Mission] mission has been a fantastic success.

In addition, we got to one point in the early 1990s where, if you remember after the Shuttle [*Challenger*, STS 51-L, 1986] blew up, we were sort of hurting for any way to launch a satellite. Our SBUVs [Solar Backscatter Ultraviolet Spectrometers] were about to fail, and we were going to lose a continuous trend in the ozone data set, which at that time, we needed to prove that the ozone hole is real. However, we couldn't fly it. We simply had no way in the US to fly it.

One of the Russians, who had been at one of our meetings said, "Maybe you could fly it on one of our rockets." They had a satellite, and they had some space on it. We said, "That's a good idea." So these negotiations were extremely interesting.

I visited Russia about five times, and they visited over here five or six times. We finally got all of the details worked out. But the [US] State Department wouldn't let us turn this instrument over to the Russians; it had to be in our possession at all times. Well, none of us wanted to spend that much time in Russia, because it was going to have to be there for two or three months before they could integrate it onto their satellite. We wound up having this instrument stored in the living room of the science attaché in Moscow.

Finally, we did launch it. The Russians have a unique launch vehicle system. They roll this door up—this is all in one day—they roll the rocket out, they attach the satellite to it, they lift it up, and they fire it in a few hours. I wasn't there for the launch, but we had a delegation of about 26 people in Russia that day. It turned out that on the afternoon of the day of the launch is when they overthrew [President of the Soviet Union Mikhail] Gorbachev. We had 26 people in Russia, and we couldn't communicate with them. It was sort of a mess. But shortly thereafter is when [Boris] Yeltsin got up on the tank, made a fantastic speech, and everything settled down. Communism was just about gone at this time, but we had a lot of guys there watching. They all got home safely.

WRIGHT: And you had a satellite up.

TILFORD: We had a satellite up. It was working. So it was a little bit different, but it was good. It filled a data gap that could have been interrupted. But still, we had our problems in launching UARS because after the Shuttle blew up, we had on this satellite a solid hydrogen tank, because one of our instruments had to be cooled down to essentially liquid helium temperature. The most energy-efficient way of cooling this instrument was with solid hydrogen, because up there it's cold. We could put it in a double tank in a big vacuum bottle. We were going to launch that on the Shuttle. Well, no way after the accident was NASA going to put a piece of solid hydrogen on the Shuttle. We had a delay in launching UARS until we could replace the solid hydrogen tank, with a large liquid He tank. UARS was launched on the Shuttle in September 1991.

This is a side story, but it's interesting. It makes you wonder. We were having the tenth anniversary of the launch of UARS at Goddard [Space Flight Center, Greenbelt, Maryland] in September of 2001. About 10:15 am one of the astronauts who launched UARS from the Shuttle ten years earlier was describing the launch with viewgraphs and a movie. At 10:30 am that morning [September 11, 2001], the screen went blank, and they showed this plane hitting the [World Trade Center] in New York. There was no commentary just the video. They didn't tell us anything. They just showed this on the screen. So UARS had an interesting but harrowing tenth anniversary celebration. Everybody left and went home, and a few people drove to California because you couldn't get on an airplane. I've never seen such chaos in Washington [DC] in my life. That was a bad day. Just a sideline.

On the international scene, we really had good participation by a number of foreign countries. I think that has continued until today with Canada, England, France, Germany, Japan and with a lot of minor countries. We even tried to help out Brazil and Uruguay and countries like that by signing joint data agreements.

Another thing that we did as we were developing the Earth Science Program is make a significant change. For most of NASA's history, the data that is obtained from any particular instrument, essentially all priorities and rights were given to the Principal Investigator [PI] and his team. Well, all of us, I think, or most of us, except for the PIs, felt that that's <u>not</u> the way it should be. We figured if this was Earth Science, and we were spending all this taxpayers' money on this program, that we had to change the data policy of NASA with respect to Earth Science.

So from day one when we let out the announcement of opportunity for the first mission—well, integrated missions, at that time; we wanted to see what kinds of instruments we received, how we could put them together in an integrated, efficient fashion so that it made sense. Some of them took longer to develop at times than others. But anyway, essentially from day one, we said we've got to change the data policy. The PI is responsible for developing the algorithm. However, if there is someone else with a competing idea to develop an algorithm, he has access to the primary data, also. This was quite a shock to all of the PIs in Earth Science.

WRIGHT: That was truly a monumental change of direction.

TILFORD: Yes, but it worked. Eventually. Not completely. But it did work. I think it was a good thing for many, many reasons. Especially the one to get the most information out of any data that might be obtained.

WRIGHT: Did this help sell the entire concept to the international partners?

TILFORD: Yes. This made a big impression on them. We required a similar thing from everyone who signed up to use it, that they would have to turn—not instantaneously—but that they would turn their data over to a data pool which would be assessable to the general public.

This part of EOS—the Earth Observing Data System—was of a scope no one had ever attempted to put together , not this kind of complicated data system in the non-classified world. This was a big thing, because it was going to run the satellites, it was going to get the data down, it was going to use the algorithms that the PIs developed to put these things in distributive data centers. We had data facilities strung all over NASA and USGS and a few other places. These data centers were going to communicate with each other, and they were going to run the algorithms. They were going to do all of these functions, and then they were going to archive the data. This is a lot of data. This is more data than you can think about. I don’t even know how many petabytes we’re talking about.

At that time, no one had a system that would even come close to making all of this work the way we thought we wanted. But we tried. After we got through the initial process, I gave Dixon Butler the job of being the EOS data czar. He brought a lot of data people in, a lot of industry people in, a lot of other people in. We had to start on this before we could start the program, essentially. We wouldn’t have time to do it after everything else got built, so we had to do them in parallel. This was, of course, because of budget limitations and because of the recent NASA Administrators, it was reduced, and it was reduced, and it was reduced.

It’s still a unique system. I think it’s probably one of the best data systems in the world. It has its faults, because we changed, or had to change, it several times in midstream, because what we started wasn’t capable of being completed at that time. We didn’t know that in the beginning. We took the manufacturers’ word for it, but it has changed hands two or three times. I think without a doubt it is still the most comprehensive data system that exists for non-classified data.

I hope they use it for the new Climate Data Center where all of this information should go into one place that has accessibility to most all Earth Science data that is available. It has to. I don’t think it has to go in one physical place, but it has to go some place where anyone who needs climate data can get it. Right now, NOAA has a facility in Asheville, North Carolina, that’s called the National Climatic Data Center [NCDC]. They’ve been given responsibility to set up a climate data system, as well as a weather data system. Dr. Tom [Thomas R.] Karl, who was director of that facility, is now, I think, acting czar for the new initiative on data. I'm going to try to persuade NASA and USGS to sign up without difficulty and to be an integrated part of this approach, because it makes so much sense from my point of view, at least, which doesn't carry much weight anymore. I think that would be a wonderful thing to do, first on a national basis, but ultimately on an international basis.

Let's talk about interagency cooperation. Cooperation between the agencies, because of the people involved, was actually much easier than I had anticipated based on previous experiences. I believe that happened because there were some new people involved. We had very good participation with the other agencies, having the cooperation of the Office of Management and Budget, and essentially, the Chief Scientist for the United States, the OSTP at that time, Office of Science and Technology Policy. We had his support, and the people that worked for him, we had their support. They were active participants.

This approach evolved from essentially 1982 when we started talking about an integrated Earth Science Program. Originally, there was a tremendous fight over who was going to be in charge of this committee that we were putting together with participants from all of these agencies. Because of our previous experience with all of these, "who's in charge issues," we, NASA, said, "We really don't want to be in charge," because we'd been in charge once and blew it. NASA was going to put in most of the money, or request most of the money from Congress, but we felt if we were in charge of the committee, or chaired the committee even, that it would be difficult not seeing that NASA was going to take all of the money and other agencies would get upset.

So we first agreed that Tony [Anthony J.] Calio, who had been an associate administrator [Office of Applications] at NASA, who I worked under for several years but now the director of NOAA, we agreed that Tony Calio would be a good person with interagency experience for this position, at the first meeting of the CES, the Committee on Earth Sciences, I think it was called. I forget all of these acronyms. There's been so many of them in the past, but all agencies had agreed he'd be in charge.

Well, the first meeting we had with all of these people at the agency level—at that time, Dale [D.] Myers, the Deputy Administrator of NASA, was our representative, at Beggs's selection. Tony wanted to take over the whole program and let NOAA do it. Well, NOAA doesn't have much engineering experience. They don't do too well in data. There are a few minor things that were missing. No one thought that NOAA, an operational agency, although they have done a good job in weather, a little bit poorer job in oceans, was capable of being in charge of an integrated, agency wide, research program. Nevertheless, many of the participants thought NOAA does a good job in its operational function. They do have to be integral to anything that's done in Earth observations, analysis, and prediction, as does USGS.

After this first meeting, there was a terrible eruption. I think we held the meeting in the Executive Office Building adjacent to the White House. Everybody just blew up. The next two weeks, OSTP removed Tony as the head of the Committee on Earth Sciences (CES) and Dr. Dallas [L.] Peck of USGS was appointed to be the new chairman of CES. Dallas was a good selection; he worked out fine, and got along well with almost all of the participating agencies. After that, we had sub-groups that met frequently; however, the CES committee met only periodically.

The little small committee met every week. At least once, maybe twice. It included all of the principal agencies. Dr. Jack Fellows at that time was the OMB examiner. He was there. NSF and all of the other subgroup participants met and worked issues out together. It all worked out well. Everybody was in step.

As we got a little bit further along, and someone, I don't remember whether it was OMB or OSTP, decided that what we really needed was an integrated budget across all of the agencies where we would define our programs as Earth System Science programs or climate programs, anything that fit in this particular realm. This would eventually be a budget that was outside of the individual agencies, so that the heads of the agencies couldn't veto it. Well, oh boy! We did this. The first year, everybody is scared to death to put their programs in as an integral part of this new Earth System Science, Global Climate (it's had numerous names over the years) program crosscut budget settling eventually as the U.S. Global Change Research Program..

But we did a cross-cut budget the first year. NASA didn't even put EOS in it that year, because the agency, with good reason, thought that if we did, it would just get cut out completely. So, everybody played it cautiously until the next year. It worked fine the first year, but still not a separate budget. We're not independent of the agency head making decisions on it. Second year, everybody got in step with the program. I think we published, and they may still do, an Integrated Earth Science Program crosscut budget. At this time, it may be the Climate Program. But every year, we put together a little booklet that we forwarded to OMB, which OMB forwards on to the Congress. It was an integrated budget for Earth Science.

It worked fine for about two or three years, until they started trying to isolate this as an integrated budget. That blew up after a while. Everybody got mad. This was not the way to proceed. NASA was not too uncomfortable with it at first, but that changed with people. The

Department of Agriculture said, “No way is that going to happen. The head of the Department of Agriculture is going to make a final decision on any budget that USGS has.” The Department of Commerce said the same thing about NOAA. So on, and so on, and so on. Well, that disappeared.

So it was a great idea. It’s still a great idea. There have been a number of papers written by people like Charlie [Charles F.] Kennel, who took over after I left NASA, and by Jack Fellows, and a number of other people, who would propose that all of the Earth Science agencies except NASA be integrated into a single climate area and would have its own separate budget. It would be a line item budget, so that it would be visible and understandable.

Now, I would personally lobby for NASA to be part of that, with one exception, and it’s a big exception—that is, the technology required to put instruments in space. There is no other agency, with the exception of DoD [Department of Defense], and you certainly can’t have it there. The Department of Energy has actually done some space work, but there’s no agency that could really provide, I think, the engineering technology besides NASA. So I would exclude NASA from that, but would include them as an integral part of the program.

NASA’s space technology is unique. I don’t think anyone disagrees with that. I don’t think you can separate the technology because of the integrated way it works. You can’t take out technology for Earth science, because you have the launch and propulsion capabilities, you have the integrations capabilities, you have the tracking capability. That makes it more complicated.

But anyway, they have made these proposals, which I think would be a good idea for climate-related processes and understanding Earth and to do the required modeling that is going to be associated with trying to do real assessments of how serious the climate problem is, how serious the CO_2 problem is, what's the impact on future energy uses, and everything associated with these questions.

Dixon was a jewel. He really did jump in and overdo things, but you got to. Dixon and I worked very closely together. The fun part really was that Dixon and I and a fellow named [D.] Brent Smith, who used to be at NASA—he's now at NOAA, and he was in the International Affairs Office—we used to travel together. A lot. In Europe and every place else. The funniest thing was that I would go in and have a beer or something, but Dixon is a Christian Scientist, so he didn't drink. Brent Smith is extremely religious so he didn't drink. So here are these two non-drinkers who I'm always sitting in a bar with. I felt bad about it, but it was also fun.

WEI: They always would leave a large tip!

TILFORD: Yes. They would have water and/or a soda.

WRIGHT: You mentioned you had this very large group of scientists that you got together. If you can, share with us some of the initial reaction from some of these people when you first invited them to be part of this group for discussion. Were they ready for this type of movement?

TILFORD: A lot of them were really ready for it. Several of them had known about the ozone issue and how that was done. We sort of used that concept for going on into a bigger Earth Science approach. I think most of them were pleased.

The university people, I think, perceived this to be an opportunity to get more graduate students in the program. In fact, that was one of our objectives, to get more students involved through grants, so that the next generation would be better than this one. I think most of the university people wanted to participate, but a lot of them were a little bit afraid that their discipline would be left out, so they wanted to make sure that they got a word in. They had a motive, whereas the agencies didn't. So in a sense, we got the support of the scientists. It was easier to get them to participate than it was to getting really active participation from some of the other agencies.

But the other agencies, for the most part, really had an interest in what we were going to do, because it would affect them. NSF has a fairly big atmospheric program, in a sense. From a ground-based point of view, from an aircraft point of view, they did a lot. They did a lot of modeling. In fact, in many areas, they did, at that time, more modeling than NASA did. So they were willing to participate. The head of that office shortly after the program was conceived was Bob Corell, who was as enthusiastic as I was about this program. He was really helpful. The fact that he was an oceanographer—I've always been thought of as an atmospheric scientist, but that's not what I am, but that's what all of the oceanographers think. That helped, having an oceanographer working with or against this atmospheric scientist, balanced it off a little bit.

The biggest problem was with Solid Earth people, because NASA had been carrying out for a number of years an experiment called LAGEOS [Laser Geodynamic Satellite], which measured the gravity field of the Earth, and it's still continuing today. What this is, is NASA made these great big golf balls, real heavy, and put little tiny mirrors, about this big [gestures to indicate size], all around it. Then they launched it into an orbit that circles around Earth at a very high altitude. What they did then is they would shoot lasers at this ball, and it would reflect down. As the Earth rotated around from several different places, they would shoot these lasers at it, and precisely, within about a few millimeters, they could see the difference in the gravity pull, as the Earth rotated and the satellites circulated.

That’s how we know so much about the gravity field and how it changes, because anything you fly, unless you fly it at a very high altitude, is going to have a big gravity pull. Newton’s Theory [of Universal Gravitation]. It’s going to pull the two objects together, so they were a little bit concerned that maybe it wouldn’t include Solid Earth.

The timescales, too, were a real big problem, because we were talking about timescales in terms of the ozone, in terms of weeks to days to years, because the rate at which ozone depletion was taking place at that time. Most of the phenomenon we’re talking about in terms of weather was within days. All of our models in weather, for the most part, are good for days.

Now, when you get to the ocean, at this time, we didn’t have a lot of information on the ocean in the early 1970s. There had been a few measurements in terms of very large-scale altimeters and such, but there was no small area, a few kilometers measurements. You did it by ships, but you didn’t know how high the ship was. The first time we ever understood what the height of the ocean variability was, was after we flew an altimeter on TOPEX/Poseidon, to any precision whatsoever. Now, that’s the session that I just went to listen to, the results of some of those measurements and what we have learned. We can now measure to within one and a half centimeters, anyplace that this satellite flies, which is marvelous, from my point of view.

But there are things that do make a difference here. With some of the laser altimeters and the synthetic aperture radars, we can measure major earthquakes within a centimeter. There are a number of faults in California, especially after some of the Alaska earthquakes, where all you need is a yardstick, or I should say a ruler, because most of them are not more than a foot, but those are big displacements. Well now, when flying some of the altimeters, we can detect movements that we could never see before. In terms of those kinds of things, the plate tectonics, they're fascinating and interesting, but they move at the same rate your fingernail grows about an eighth of an inch a month, or something like that. Now we can do those things from space. Many of these we could not do before. A lot of things just happened, without being noticed while the event is in progress, now we can obtain extra information from them.

The Solid Earth people, especially those interested in plate tectonics, were really concerned that they would be left out. It's harder to integrate what's happening there. Most of those are episodic, in the sense that they happen big and infrequently, like a volcano, that messes up everything. Atmosphere, ocean, everything changes because of it. Heat budget changes so rapidly because aerosols absorb so much sunlight that the whole heating system and cooling system of Earth is changed. So you need to know as much information as possible about extent, concentration, attenuation, etc. to evaluate the probable perturbations that may occur, and how the surface of Earth will be affected.

Fortunately, for a couple of those large volcanic eruptions over the past two decades we had aerosol monitors flying on satellites. They measured both aerosols and clouds, but with a big earthquake aerosols dominate, and they really change the climate significantly, probably the biggest impacts that we've ever been able to see results of, except for results of super volcanoes like the one we have in Yellowstone [National Park, Wyoming], which is scary. If you remember a couple of years ago, the lake tilted a little bit. The lake water started running out of the wrong end of the lake. Everybody thought, and I did too, that this might be the precursor of another super volcano, because the last time that happened was several thousand years ago and it dumped several inches of ash on Kansas. That's pretty far away.

Another geologist predicts that magma would be flung fifty kilometers into the atmosphere. Within a thousand kilometers virtually all life would be killed by falling ash, lava flows, and the sheer explosive force of the eruption. One thousand cubic kilometers of lava would pour out of the volcano, enough to coat the whole USA with a layer five inches thick. But the problem is, with something like that, the Earth is going to cool off right quick, because the sunlight can't get through the atmosphere and the whole dynamics change. There's nothing we can do except watch, at that point.

Anyway, we included some of that. Everybody eventually was happy, I think. Almost everyone was happy with the Bretherton Report when it was finally published. A lot of people accepted it as a road map for things to follow.

WRIGHT: The report has Francis Bretherton's name, but how much were you intimately involved in putting that together?

TILFORD: I sort of helped it along a little bit here and there. I tried to guide them without guiding them. I found, if you're very active and pushy, you don't get much done. But if you can help them a little bit here and there, it's a lot easier to do. That's the way I worked the whole program, because if you get too much involved, it becomes yours instead of his, or theirs, and I didn't want that. It was his report, and the committee's report. It wasn't my report.

But, yes, I played a supporting role. I was at nearly every meeting, and I was right in the middle of discussions. We'd talk about pros and cons of various proposal and suggestions, and I'd of course give my opinion. I tried not to dominate it, because I have learned that doesn't work.

WRIGHT: Sounds like a good lesson learned.

TILFORD: Yes. We tried to play the same role in the interagency thing, because we had learned if we tried to do it, we just would lose. We wanted the other agencies to accept it. As Peter [W.] Backlund, one of the super staff individuals that used to work for me, said, "What I think you realized is that if you can make it a national program, rather than an agency program, you'd have a whole lot better chance of getting it to work." Which is true. Once something becomes a national priority or has a national visibility, the probability that it can be sold and implemented is much greater. I didn't fully realize that at the time, but that's the way it is. If you can make it where it really doesn't belong to one agency, but it belongs to the United States, then yes, that's a lot easier to get across—not only to the public, but to the Congress and everybody else. That's essentially the role that NASA tried to play, with some exceptions.

That's why we had other people in charge of components of the program. For the working group, Bob Corell of NSF chaired the group, and then there was Dallas Peck from USGS for the big CES committee. We had the dominant budget, and probably we had 80 or 90 percent of what was going into Earth Science. That's the reason we didn't want to say that it was a NASA program, because we would never have been able to get it approved.

WRIGHT: You mentioned earlier about it then growing into the next step, which would be an international or a global endeavor.

TILFORD: Well, we did all this in parallel. We slowly worked it up, so we would cooperate with this organization, or that country, or that agency and so on. That's still going on. The Japanese have been wonderful partners in this whole thing. We fly together, we exchange data. The same way with France. The same with England, although they don't have the launch capability. We've tried to work with country after country after country in a cooperative manner. We fly their experiments on our satellites. Germany, they've built numerous instruments, a few to fly on our satellites, but mostly to fly on the Shuttle. They were very interested in utilizing the Shuttle and having a man fly in space, which Japan and France were too. That was initially part of the trade-offs, that they would get an astronaut, if they would cooperate in other aspects of the space program.

WRIGHT: So it expanded into the human space flight as well.

TILFORD: Exactly. We did fly a number of instruments on the Shuttle, as I mentioned. We flew an infrared interferometer, which measured at extremely high resolution the spectra of the sun on two different occasions. One when it was a little bit active, one when it was pretty quiet, because that gave us the highest resolution spectrum we had ever had of the sun over the whole infrared wavelength region. That atlas is about this thick [gestures to indicate size], with all of the observed solar lines in it. If anyone ever needs to utilize the solar spectrum for something, they'll know where lines are, what intensity, how they fluctuate, and so on.

So you don't need to fly that all of the time. You need to know what it is. What you need to know is how much the sun is varying, because a one percent change in the output of the sun is going to make a big impact on climate and us. Up or down.

WRIGHT: You have talked off and on about the DoD. Did you know what their thoughts were on this initiative to bring all of these agencies and disciplines together, as well as international?

TILFORD: They didn't interfere with us too much about that. What we were concerned with didn't require looking at the ground at a certain resolution point in a time frame, because that potentially had defense implications. So it got better and better, and today, I don't really know anymore. I know they've released a lot of their classified data.

WRIGHT: You also talked about the disciplines and people's priorities. What was your process to decrease the negative output of the why-it-won't-works, and how were you able to overcome that and get people more focused on the overall vision?

TILFORD: Well, as this program grew, and as that first document came out showing all of these interactions, people started thinking, "There's something in there for us. Maybe we can use this."

As the process went on, it was the scientists who were doing this. It wasn't me, and it wasn't NASA. We really tried to give the scientists the free will. We tried to guide. We tried to give constraints on what we might and might not do. If they ask us something, we say, "Well, that's outside of the range of what we can provide a budget for," but we didn't do enough of that, apparently. Then, they made a number of recommendations for measurements, and we tried to incorporate most of those.

Our original plan, which, unfortunately never made it, was that we would have three series of two or three large satellites, which would give us a long enough data set, we hope—15 years, 5 years apiece—such that it would give us a data set that we could actually verify that those particular measurements were important or were not important. Now, some of them, like solar constant, we knew were important and should be done forever. On the others, we did things like altimeters and scatterometers and backscatter experiments and active microwave experiments and LIDAR [Light Detection and Ranging] experiments. These all were new and state-of-the-art, in terms of flying in space.

We tried to do a lot of things that we just couldn't do. We wanted to fly an active LIDAR to measure winds in the troposphere. Now, we did measure winds in the stratosphere on UARS, but not with an active laser. Dr. Paul Hays developed an innovative etalon interferometer which worked well to measure global stratospheric winds. Before issuing the Announcement of Opportunity [AO] for EOS, the project tried to do a careful cost/weight/pointing assessment based on similar class experiments on previous satellites, then we sat down and tried to price these things, and in the early or mid-1980s, there wasn't a laser that worked. You could look at a Gatlin gun [rapid fire, multiple lasers] approach, which we did, in flying a bunch of lasers and use them one-by-one until they failed. Still, the lifetime of lasers 25 years ago was not capable, and I don't know whether it is yet, to be perfectly honest. But anyway, we tried to do it.

After I retired, I went to Orbital Sciences [Corporation, Dulles, Virginia], and I talked Orbital Sciences into trying to do it on a commercial basis, which back then was a great, big nice thing. Everybody was trying to commercialize everything. We tried several things that all failed in terms of commercialization. I guess the ocean color experiment was probably one that worked for a while, but it was not economically viable or feasible. If the government doesn't do most of these things, they don't get done commercially like communications satellites.

Look at Landsat. Congress dictated in the late 1960s, early 70s that we should give all of the data from Landsat to a commercial company to distribute. Well, the commercial company never made any money at it. The data never was distributed to the people that paid for it. From my point of view, it was a horror story. We tried to keep an open data policy, where we give the data to organizations, and individuals that will use it for many purposes, not get the commercial sector involved in it. It was a US research program, or US operational aspect. One of these days, I'm sure the commercial sector will take it over, and they should. When it gets to be operational, if they can do it cheaper than the government, it should be done that way. Right now it's not feasible, I don't think. A lot of people do. From my personal experiences, I don't believe we are there yet.

One place it did work, and it's worked extremely well, is in communication satellites. Now almost all communication satellites, including the TV and most other communications satellites are commercialized, and that's fine. The government shouldn't be doing that. But for these measurements which will provide information on how we develop energy, natural resources, etc, which, for the most part, only have interest to the long-term survival, if you want to put it that way, or at least the benefit of society as a whole, I don't see that the commercial sector will ever do that.

In fact, one of the biggest problems this agency and NOAA and everybody else has right now is that because of the limited number of satellites that have been flown and the limited number of instruments that have been flown, we're slowly but surely losing industry ability to build complicated state-of-the-art instruments. That's because most of the people that built most of these for the last 50 years are retiring.

It's a sad state of affairs, but we've exported so many things we took for granted before, that our skills are beginning to deteriorate. I think it's going to be a huge problem for NASA and DoD in the future. Before, large instruments builders could obtain components from companies that were building big hardware. A lot of planes, a lot of auto components, etc., but we're not doing as much of that as we did. We're exporting a lot of jobs and we're not training the younger generation in many of the skills required for building state-of-the-art advanced instrumentation. It's going to be a factor in the future. There's still talent around there, but many people who used to work as machinists and so on now work doing data analysis or something similar. In data analysis, we're doing good. In manufacturing, we're not doing good.

WRIGHT: Was that one of the factors of setting your goals and objectives during the initial startup of Earth System Science, knowing that these expectations could be filled with the talent that was here?

TILFORD: Some of them.

WRIGHT: You mentioned the large satellites in polar orbit.

TILFORD: We knew we couldn't fly them on the Shuttle. The weights, size, and power were the principal problems; you can't get that much power out of the Shuttle. You might be able to get them in orbit, but you can't put anything on them. We knew that.

But no, we didn't recommend those. There were three instruments that we couldn't build, technology-wise. We had carried them on as studies just before I left NASA. We had carried them on after the AO process. One of them was a laser wind sounder. Just before I left NASA, I had to terminate all three of those, because I didn't want whoever came in to have to deal with this issue of terminating those instruments, because he or she would have a heck of a thing to deal with, because they didn't know the history of any of those instruments.

So there were three that we just couldn't build. We could not afford to build or the technology was just not available to make it work for several years in space. We could have built it. It probably wouldn't have worked very well; it wasn't feasible, let's just put it that way. We had picked the instruments, and I'm sorry I don't remember how many we came in with, but we probably picked about 20 to 30 percent of them, something like that. There were a lot of people that were very unhappy, which I don't blame them, but they all went through a peer review process, and after they went through an engineering review process, we also had the project to look at them for feasibility and how they could be integrated on a large platform.

We made some cutbacks in some of the instruments because of size or because of power or because of pointing requirements. Some of these we couldn't accommodate, and some of them we could modify and that was fine, and some of them we couldn't afford. The ones we couldn't support, we just had to terminate. We carried them on hoping that, for instance, lasers would improve in the future to the point that we could fly them. Someday I hope that's true. But it wasn't feasible then, and some of them still wouldn't be feasible now.

I do want to go back and talk about System Z. I forgot to do that. That was what we put together, essentially before we went to Vienna. Dixon was highly involved in this, and we were trying to look at a system of a series of measurements that we could make a reasonable rationale for flying. Dixon and another group of scientists before this, without having the concept of full up science input, got together, and after many meetings they came up with a hypothetical concept, which we called System Z for years. After the Bretherton Report came out, we changed it. There's a lot of controversy over where Mission to Planet Earth title came from. I didn't particularly care for it, but a lot of people did.

WRIGHT: You're talking about the name?

TILFORD: The name. It was originally Mission to Planet Earth with a System Z satellite set, and I thought both of those were pretty bad. A lot of people discussed the name Mission to Planet Earth, which it carried for a long time until it got to be Earth System Science and EOS. EOS is fine. Earth Observing System, that makes a lot of sense. It was either some combination of Mous [Moustafa] Chahine, Dixon Butler, and Burt Edelson. One of them, and I have no idea which one originally came up with it. Or it could have been Richard [M.] Goody, I don't know. At that time, there was Mission to Mars, and Mission to Venus, and all of those missions to other planets, so they said, "Why not a Mission to Planet Earth?" But it's a name that's drawn a lot of controversy. I'm not responsible for that one.

This concept was something we had planned in order to do it on the Shuttle, that we would have these three large platforms that we could put in polar orbit and then service. These were serviceable instruments, not just one flight, period. That's where the System Z thing came in. Ming-Ying mentioned that I wanted to cover that, and that goes back in the beginning of these proposed concepts.

WRIGHT: You met for years, you got priorities set, and then how were you able to launch this program?

TILFORD: At this time we had involved the other agencies to a great extent. They knew exactly what we were doing. We had to tell them why and how it would benefit them. Everybody supported the concept. It was essentially new money for Earth Science, so we didn’t have a lot of problem from that. We weren’t taking from the other agencies. We were building something new.

It was a new start, and we had set aside certain things that would benefit all of the agencies. The EOSDIS [Data Information System] provided a way for everybody to get the data. We had set it up so there would be a good R&A, that’s Research and Analysis Program, essentially, so that we’d have a lot of scientists involved. Each instrument had a science team. We funded a science team long before the instrument flew so that they could work on the algorithms, they could work on the trade offs. I don’t know. We probably had, what, Ming-Ying, approximately 10-15 members on each team or something like that?

We had a lobby, essentially. What we did is try to get most people to support the program. We really wanted people to get involved. We really wanted this program to work. If that was going to happen, when a question was asked on the [Capitol] Hill, they had to say, “Yes, we support this,” whether it was a scientist, or whether it was another agency. Because we weren’t taking anything from the other agencies, that worked all right. They were part of it. They got to talk to about it and make suggestions and everything you should be doing.

We really didn’t have a big problem, because the NASA administration really wanted their science effort to succeed. I think Jim Beggs really wanted it to succeed because it was his idea. Then [James C.] Jim Fletcher, who was a Mormon, and he really believed that what the agency did should benefit humanity. He was a big supporter of it. After Beggs unfortunately had to leave the agency, or left of his own accord, because of false, whatever it was, charges—and I never learned all the details, I may have known some of them, but I've forgotten them—but anyway, he decided he had to leave. I don't think he did, but he didn't want to pull the agency through a terrible situation with [Capitol] Hill.

But when Fletcher came back, he was very supportive. Somewhere in there—what was the name of the former Bell Telephone executive who ran the agency for a while? He was an oceanographer. Hold on.

WEI: [Robert A.] Frosch?

TILFORD: Frosch. Yes, Bob Frosch. He was a big supporter. I mean, Bob Frosch started out as an oceanographer, so he was a real supporter.

WRIGHT: Were you having support as well from the Presidential administration at this time?

TILFORD: Yes, we did. We couldn't have done it without them. When you say Presidential support, we had OSTP support. OSTP and OMB, to me, that's presidential support. Actually, the first [President George H. W.] Bush was a big supporter of this program because—and this is Shelby's opinion—but from my point of view, senior Bush thought this was a great way whereas he didn't have to worry about global warming. That he was going to do research and find out if it was real. It was a whole lot easier and cheaper for him to do the research than it was to make a decision. That decision. I don't blame him. But anyway, that's my opinion.

So we had a lot of support from the White House. We had support from Congress. People like [George E.] Brown [Jr.]. Of course, [Senator] Barbara [A.] Mikulski supported us big, because of Goddard. Of course we had support in Texas, we had support in Alabama, we had support in California. Yes, all of the senators got a part of this.

Nobody really objected too much. Some of the Texas crowd was a little bit concerned about us, and Florida too, for budget reasons, but we were going to fly satellites on their Shuttle. That was all part of the program. In that sense, we were pretty lucky. There were some dissenters, as I mentioned, the same people that were objecting to cigarette smoke and ozone, now became anti-global warming characters. We had the same problem as those programs endured. Most of these individuals were paid to do research, in this case by the oil companies, to put out their results that there is no such thing as the CO_2 build-up and global warming, that the instruments weren't good, etc. But that still existed, and it probably does today. I haven't been around for a while, but I'm sure the same people are saying the same thing. Almost always without scientific evidence, one way or the other. Of course, none of them believe in modeling. I must add that there are some very few legitimate scientists who do not subscribe to the CO_2 atmospheric heating.

WEI: You reconvened again, then from the three large platforms story. When did Dan [Daniel S.] Goldin [Former NASA Administrator] come in? What year did he start?

TILFORD: He came in 1992.

WEI: So that's much later.

WRIGHT: So the Earth System Science had been actually up and going.

TILFORD: We had to work hard until 1990 to get a new start. Once we got a new start and we issued an AO, actually, I think we issued the AO when we got it put in the budget, when we knew it was put into the next year's budget and had a good chance of passing. We got that information from sources. We had to reduce it some, but okay. We could do that. So anyway, we issued the AO then. Then we got the new start in 1990 (FY 1991), but that meant we didn't really get any hard money. We had been putting a little bit of money in it, a few million.

WRIGHT: Was this part of the Global Change Research Program?

TILFORD: Well, the Global Change Research Program is a bigger program than Earth Systems Science, or than EOS. We're talking about EOS here for the most part. Global Change we'll go into next time. There's a CEOS [Committee on Earth Observing Satellites] that we'll go into next time.

This is the International Committee on Earth Observing Satellites. What we tried to do was set up, what was mandated by Congress and by the President, that we set up an international Earth observation group to discuss internationally how we could cooperate, which we'd been doing for a few years anyway. That was a group that met twice a year, most of the time, sometimes more frequently, to discuss the international cooperation aspects of the program.

Global change got to be a great big thing involving a lot of countries and UNEP [United Nations Environment Programme]. All of these things going on at once required a lot of time and travel. Dixon Butler, [Robert T.] Bob Watson, Stan Wilson, Lisa Shaffer, Peter Backlund, John Theon, other members of my staff and Brent Smith and other members of the International Affairs [Office] were an integral part of these international discussions and agreements.

WRIGHT: When 1990 came, it was almost a new beginning for you in a lot of ways.

TILFORD: It was a new beginning, which came to an abrupt end when Goldin arrived at NASA, because as I said earlier, the first thing he did was to separate Earth System Science out as a separate office. We had an Associate Administrator [AA] just like all of the other offices did at that time. I was acting AA, but he was damn sure he would never give me the job, because "quote" I had refused to give TRW [Incorporated] a contract for a previous satellite selection.

Before Dan was appointed as the NASA Administrator, while he was still a TRW employee, TRW had written a rebuttal paper to an earlier satellite selection by the Earth Science Project Office. They informed me that they were going to challenge the selection. I called them up and told them that the details that they had stated in their paper were not true, and if they went forward with it, I would write a letter to whoever they submitted it to and explain all the details of how and why the previous selection was made. TRW made a decision not to send the rebuttal for not being selected.

The first time I walked into his office, he accused me of personally undermining TRW in a selection process. We never got over that!

Dan did not like big instruments. Dan did not like big satellites. Dan wanted to fly cheaper, better, faster. I didn't mind the cheaper part, I didn't mind the faster part, but he didn't know what better meant. He apparently did not care, nor understand much about what the ultimate goal of a measurement might be, just make a successful flight, make sure the system worked, period. That's my interpretation of Dan's approach to NASA.

Do it faster and cheaper and demonstrate that it works, never mind whether it can contribute to a better understanding of the overall goal of the program or the observations. He and I never got along from day one; we just didn't care much for each other. He knew I didn't, and I knew he didn't.

WRIGHT: Were you able to the program that you invested in and believed in?

TILFORD: Well, the program I had was funded by the Congress. Now, they only do one year funding, but you get a run-out budget. He couldn't kill the run-out portion in one year. He tried to kill as much of it as he could, but most of it survived. What happened is that we lost the second set of satellites, and we lost the third set of satellites. That was really unfortunate for the Earth Science community and for the United States as a whole.

WRIGHT: Tell me what the significance is of being able to look at 15, 20 years of data. What does that tell the scientist?

TILFORD: If you look at most of the parameters we're talking about, we don't know how they change, or vary on a decadal, or longer time frame. We do know that the ocean changes very rapidly. It also has long term changes that are not associated with short term changes. The same thing is true with droughts and rainfall. Until the last few years, we did not know what the rainfall had been, or how it varies, especially over the ocean which covers about 2/3 of Earth. We had no way of even estimating what the cloud cover was in terms of solar attenuation or reflection over the ocean. We had no idea how the ocean varied in height until ten years ago. If you have a ship out there in the ocean, it goes up and down. Without a GPS [Global Positioning System], how in the world are you going to tell how high the ocean surface is? You're sitting on top of the ocean, and the ocean bottom changes as you move. There was no way to know what the ocean height was doing.

Unless you have a long-term data set of at least 15 years—22 is ideal, because the solar cycle is 11 years, and you need two solar cycles to really understand what the various outputs and various wavelengths from the infrared to the ultraviolet are, actually to the x-rays—but unless you have that kind of data, how do you do understand or predict climate? You've got to have a long-term data set. Now, 20 years or 15 years—we expected the proposed EOS would provide about 20 years, because most satellites now live 10 years, but you can only assign a 5 year lifetime on any satellite, unless you're crazy. I'm teasing—no, I'm not teasing. That's about what a normal satellite lives, is 5 years, but many of them operate for 7 or 8 or 9, some 10.

That's the purpose of a long term data set. It was climate we were interested in then. It is climate we are interested in today. Climate change is something we are just beginning to understand. I just saw the first ten year data set from TOPEX/Poseidon, which is the altimeter, a few minutes ago. That's why I wanted to go back and hear about all that had happened. They saw some truly unusual anomalies in the ocean. The biggest one that ever happened, they recorded altimetry data from it, watching the warm water rush up against the coast of South America and then turn back.

You remember the El Niño they talked about so much? And then La Niña? One of them is when you get cold water in the Eastern Pacific [Ocean], and one of them is when you get warm water in the Eastern Pacific. Because it affects the whole ocean circulation, it in turn affects the total rainfall pattern over the whole world. These are things that we did not know twenty years ago. We know a little bit, now, but what we need is enough of a data set to say, "Okay, where are the drivers?" We know the sun is a driver. It's the biggest driver. But what are the other drivers in the climate system? CO_2, that's a driver, because we're changing its concentration, and it does trap Infrared radiation into the Earth's atmosphere, thus heating up the Earth's temperature.

We don't know what the ocean circulation is. Ice melt in the last three years has been phenomenal. We've melted more ice in the ocean, which is a lot of water, in the last three years than we probably have in the last one hundred years. These are all things we don't know about. These are all things we can measure now. That was our objective, to go measure it, and then let people analyze it. That was the whole objective. Go measure things we don't know about on a global scale and determine what's important and what isn't. When we find out what's important, we'll try to measure it on a continuous basis, but we won't continue measuring some of the other things which aren't important.

It's trying to learn what's important and what isn't. Then we want to incorporate these findings and changes into improved model predictions that will help us predict and plan for the future—water resources, food production, ocean level changes, deforestation, energy production, flood protection, transportation improvements, etc., etc., etc. These goals are what Bretherton and his colleagues proposed in the Bretherton Report, and this is what we set out to achieve with EOS and EOSDIS.

I think on the 20th anniversary of EOS (from my personal point of view—the 25th anniversary), from what I have heard in terms of accomplishments, the EOS program has made great progress.

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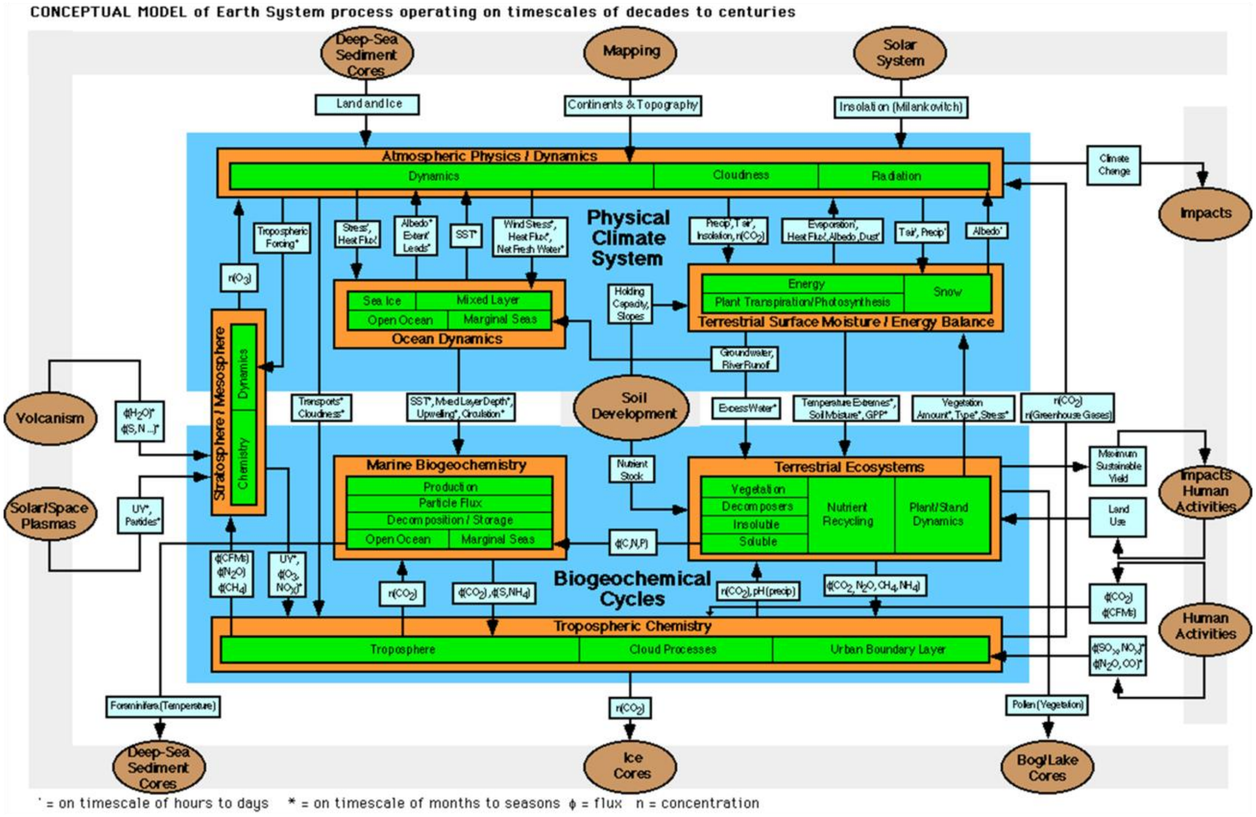
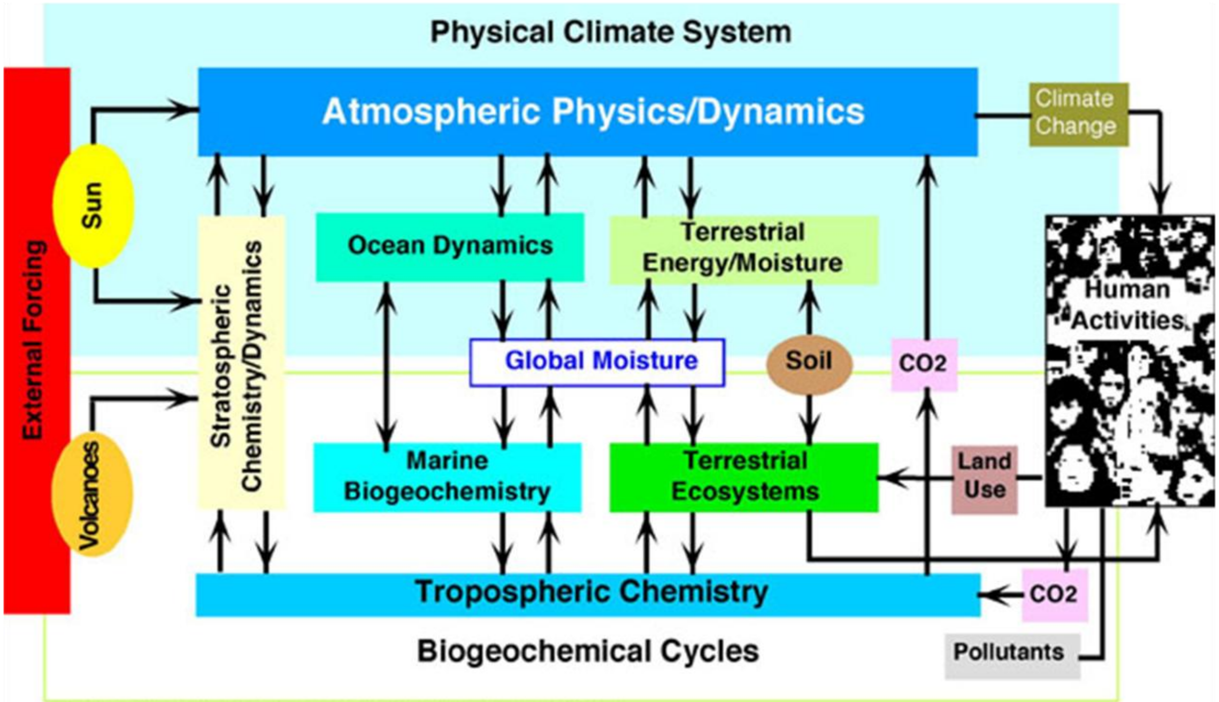
The Bretherton Diagram- Complex



CONCEPTUAL MODEL of Earth System process operating on timescales of decades to centuries

' = on timescale of hours to days * = on timescale of months to seasons ϕ = flux n = concentration

The Bretherton Diagram- Simplified



(from Earth System Science: An Overview, NASA, 1988)