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TAPE RECORDED INTERVIEW PROJECT

Interview with Susan Solomon
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Interviewer: Dale Kellogg

Kellogg: This is an oral history for the American Meteorological Society of Dr. Susan Solomon, atmospheric chemist, senior scientist at NOAA here in Boulder. I am Dale Kellogg, interviewer, from National Center for Atmospheric Research.

Susan, I thought we would begin with some of your biographical background--a little bit about your childhood, where you grew up, when your interest in science first demonstrated itself, and what those influences were in y
our early years. Can you tell us a little bit about your early days?

Solomon: I was born and raised in Chicago. That was in the city of Chicago, not in the suburbs. I grew up in a sort of typical middle-class neighborhood. My interest in science really began when I was less than ten years old, I think. A lot of it had to do with things related to the natural world. I've always had a fascination with animals, with nature, and a particularly big influence was the appearance on American Public Television of Jacques Cousteau. I thought that was not only the most beautiful thing I'd ever seen but also probably the most interesting thing I'd ever seen. So at that point, when I was nine or ten, I decided I wanted to be a marine biologist and go study the whales or something.

Kellogg: You say you were taken by the beauty, as well as kind of an intellectual stimulation. Just the beauty of the undersea world?

Solomon: Yes, the beauty of the undersea world; it's interesting because I think for many years, as I developed my science more in an analytical and direct framework, I kind of forgot about that early fascination with the physical beauty of it, but it really came back to me in an incredible way when I finally did become a certified SCUBA diver and I went on my first really spectacular dive in the Caribbean, and I can still remember being down there with corals and I saw live turtles and all the beautiful reef fish, and I suddenly remembered how, at that very young age, that was exactly what inspired me--a portion of what inspired me to be interested in science in the first place.

Kellogg: But you didn't become a marine biologist. You're dealing with a different fluid--the atmosphere. When did you begin to think that maybe marine biology wasn't what you wanted to do?

Solomon: It's kind of ironic to be working for the National Oceanic and Atmospheric Administration as an atmospheric scientist having thought originally that I would be interested in oceanography or marine biology. I got to high school and began to actually take science classes, so we had to actually get to grips with the real study of a phenomenon. And I found that I really wasn't that crazy about biology, because it just wasn't quantitative enough. It didn't appeal to me because there were too many things that--the entire issue of the influence of the observer on the system. While I understand it, I found it very frustrating, and I found just the difficulty of ever achieving a level of quantitativeness, or closure, very, very frustrating. And then I hit chemistry. One of the most vivid memories there really has to do with the time that we did our first acid-base titration. I thought it was absolutely fantastic that you could calculate exactly how many milliliters of hydrochloric acid you were going to need to make the indicator turn pink or blue or whatever it was, I don't remember what indicator we used. The quantitative nature of it was, I thought, really elegant. So chemistry became a real fascination from that point on.

Kellogg: Now you don't have any scientists in your background, neither your mother nor your father are scientists?

Solomon: No. My mother was a fourth-grade teacher, and my father sold insurance. They were not, either one of them, at all really interested in science.

Kellogg: Do you have any siblings?

Solomon: I have one brother, and he's also a fourth-grade teacher.

Kellogg: That's lovely. So I know that from your résumé, from your curriculum vitae, that you did win a third-place prize in a national science fair while you were in high school. Did that pretty much clinch it for you, did you decide at that point that you would go to college as an undergraduate in science? Did you at that time think you would go on and get a Ph.D.? Were you that sure that early?

Solomon: That's a good question. Actually, it's interesting now that you mention it. I hadn't really thought about it in this way but all through high school I was kind of torn between an interest in art and an interest in science. I was taking classes at the Art Institute of Chicago, and I really enjoyed painting and sculpting and stuff like that. I wasn't quite sure whether I wanted to go to college to study art or go to college to study science.

I'm sure the science fair had an important impact as a confidence-building experience. I also really enjoyed actually doing the project, I enjoyed doing the research. Interestingly enough, my high-school science fair project was called, "Using Light to Determine Percentage of Oxygen," which has something small to do with atmospheric chemistry, I suppose. I think what was particularly fascinating about it for me was again the fact that I was able to build a quantitative framework that actually helped me to understand something about the physical and natural world. I actually won the City of Chicago's Science Fair and went to the International Science Fair as the representative from Chicago, and placed third there. So that was really quite a confidence-building experience all the way around.

Kellogg: So from high school then, where did you go to college?

Solomon: I was a little bit perhaps too young or too inexperienced to feel comfortable going too far away from home, so I went to the Illinois Institute of Technology, which is on the south side of Chicago, where I studied chemistry. I do find it kind of amusing that at that age I thought I didn't want to go too far from home because I went there for two years. Then, in my third year, I went on an exchange program to France and actually spent my third year overseas in an entirely foreign and very challenging and interesting environment. Then I came back and finished off at IIT and got my undergraduate degree there.

Kellogg: Can you tell us a bit about that year in France, what you had planned to do there, what kind of influence you feel that had on your future life course?

Solomon: Study overseas is a tremendous experience, especially for an American, because in this country we are so isolated from the rest of the world, and it is very difficult for us to really appreciate all the influences, the cultural forces, the historical forces that are at play on a global basis, particularly in Europe, which is a major player of course on the international scene. On a personal level, it was a different kind of challenge I had ever faced before, living in a foreign environment and learning to speak a foreign language--all my classes were in French. All my friends were French. There was one other American at the University where I went, but I actually didn't interact with him that much, so it was a total immersion type of experience, and I found it to be a tremendous character-building thing, I guess, in the sense that it was a challenge, and I found that I was able to meet that challenge. By the end of the year, I could actually speak French very well.

Kellogg: So then you went ahead and finished off at the Institute of Technology. Where did you do your graduate studies?

Solomon: I should say by the way that I didn't learn a whole lot about science in the year I was in France. I spent too much time--I think I probably could have because it was a very good school, but I spent a lot of time going around and traveling all over Europe, seeing the world rather than focusing on my studies. I was focusing on other aspects of life, and it was a great experience. In fact, as I rank sort of life's experiences, it's right up there in my top five; having gone overseas to study is something that I would recommend to anyone to do.

I went from IIT to Berkeley, where, again, I was studying chemistry.

Kellogg: Now there's a quote from one of your many interviews in the past that says that you found yourself doing too much test tube chemistry again in graduate school. Can you tell us exactly what test tube chemistry is and a little bit about how that differs from the kind of research that you do now?

Solomon: Actually, it wasn't in graduate school, it was in the period leading up to graduate school. I knew I wanted to do science, I knew I really wanted to be a professional practicing Ph.D.-level scientist. But I also knew I didn't really want to spend the rest of my life studying chemistry that was only important in a test tube, no matter how quantitatively elegant it might be. I was fortunate in my senior year at IIT to stumble on the existence of atmospheric chemistry as a sub-discipline, because it opened my eyes to the fact that you could actually engage in chemistry that took place on a planet, rather than in a test tube. I guess in a sense I was re-discovering the whole idea of biology, and the grand oceans of the world. The analogy I suppose is chemistry and the oceans of air of the world.

Kellogg: That is elegant. It makes me think that your appreciation for beauty is something that has actually helped you in your research--the ability to recognize the elegance of a solution, the elegance of a quantitative experiment that gets you exactly what it is you're trying to measure. We can come back to that.

Solomon: I've never really thought about it in those terms before. I suppose all scientists have that in some measure. I don't think you could enjoy doing science if you didn't feel that way. It is often said that a lot of scientists are musicians. I personally have never had a strong interest in music, but I've had a strong interest in the visual arts.

Kellogg: It was at the University of California at Berkeley that you first met Paul Crutzen?

Solomon: No, I actually worked with Harold Johnston at the University of California at Berkeley. And the fact that he was there was a key part of why I chose that particular university. I wanted to do atmospheric chemistry. I certainly wanted to go to a good school--and Berkeley is a very good graduate school in chemistry. And Harold was there. He was well-known for his work in the early seventies on the influence of supersonic transport on the ozone layer. So it seemed that Berkeley with him was one of the rare places where one could actually do chemistry in the ocean of air instead of a test tube.

Kellogg: When did you meet Paul Crutzen?

Solomon: I was very fortunate in that I applied for a UCAR Graduate Fellowship--I don't think they have that program anymore, in fact I'm sure they don't. I applied for, and got one, in 1977; that system, that program was one where you would come to NCAR for the summer and work with somebody at NCAR, then go to your graduate program--which they would pay all the full freight for two years--coming back every year to NCAR in the summer.

Kellogg: Did you have a research project in mind when you did that?

Solomon: No, I can vaguely remember seeing the notice for that program on the bulletin board at IIT, and thinking that this would be great fun to have the opportunity to actually start doing something in atmospheric chemistry before I went to graduate school. I had never heard of NCAR, and had no idea of what the place was or who Paul Crutzen was. He at the time was the director of the Atmospheric Chemistry Division here, and I worked with him and his postdoc at the time, Jack Fishman, who's now at NASA-Langley, who's also a very well-known and very capable researcher.

So I was really fortunate in being able to connect up with actually two such good people to work with.

Kellogg: What was the research you were contributing to at that time?

Solomon: There is an archive of ozone measurements that is kept by the Canadians, by actually the Atmospheric Environment Service in Canada [which] keeps these records of ozone sonde measurements. Back in those days, nothing was digitized; it was all recorded in these books which they had in the NCAR Library, and I went through those books and tried to evaluate as many different stations that had measurements of the vertical profile of ozone that I could find. In particular, what we were trying to focus on was whether we could see a difference between the polluted Northern Hemisphere and the relatively clean Southern Hemisphere, and also whether we could see anything of interest in the tropics, of course, was also a matter we tried to focus on. And remarkably, we were able to find a fair amount of data, enough to really see the difference between the two hemispheres. That paper--which was my first scientific paper that I participated in--is still actually quite widely cited today, and people's interest in the differences in tropospheric chemistry between the two hemispheres. Of course, I can't really take any credit for it. What I did was to literally act as the human calculator on all this stuff, all these data files. I processed an incredible number of ozone sondes by hand.

Kellogg: Did you have any hint at that time that ozone would play such a large part in your research career?

Solomon: Absolutely not.

Kellogg: We'll get to that part.

So, here you are, you're twenty-three years old, you've just received your Ph.D. from Berkeley, what's next? Do you remember what it was you wanted to do at that point, what your expectations or hopes for the future were?

Solomon: As often happens, you're kind of making me larger than life. I was actually twenty-five when I got my Ph.D. Technically, when I actually received it, I must have been twenty-six. Although I guess I finished it at twenty-five. Anyhow, I wasn't quite as young as you say.

We should backtrack just a bit. I spent two years at Berkeley doing the coursework in chemistry that was required to get a Ph.D. in chemistry which included all the standard stuff of quantum mechanics and statistical mechanics and kinetics, most of which frankly I had very little interest in because I knew it wasn't going to be terribly helpful to me in studying atmospheric chemistry. In fact, I can't count the number of times really that I've had to use statistical mechanics, for example; in doing atmospheric chemistry, I don't believe I ever have used it as such.

So I was not really too interested in staying there, and I was fortunate to be able to come back to NCAR as an NCAR graduate assistant, which was another student program that they still do have now. Because Harold Johnston and Paul Crutzen were good friends, it was pretty easy to work something out that allowed me to actually do my thesis work at NCAR rather than at Berkeley. And Harold was actually extremely supportive of all that, which I very much appreciated at the time and still do.

So I got my graduate degree here. By that time, I knew that Boulder was a very stimulating place to be to do atmospheric chemistry, and it still is one of the remarkable meccas for this field. So I was interested in staying in the Boulder area, and ended up getting a job at the National Oceanic and Atmospheric Administration (NOAA) Aeronomy Lab.

Kellogg: Were you recruited for that position?

Solomon: Yes. Paul Crutzen had actually had at one time a half-time arrangement--half-time between the Aeronomy Lab and half-time at NCAR. I guess he must have been the one who told them that I was someone worth taking a look at. The rest, I can say, just kind of happened.

Kellogg: Now you say that "the rest just kind of happened." Four years later, you are a woman leading the first U.S. expedition to the Antarctic to try to figure out the chemistry behind the ozone hole that had been discovered just a few years before. That must have been a very exciting four years between the granting of your Ph.D. and arrival at NOAA, and four years later, standing at the South Pole. What did you do during that time?

Solomon: Well, a lot of different things. Certainly a major focus was I continued to work with Rolando Garcia at the Atmospheric Chemistry Division (ACD) at NCAR. We've been for about twenty years now developing a two-dimensional stratospheric model which has been--we've been able to use it to look at a broad range of chemical and physical processes that influence ozone, and I think actually it's fair to say that the major credit really goes to Rolando in the sense that the thing that was innovative about what we did was that the way that the circulation was formulated. We were among the first, if not the first, to really take advantage of what's often called in meteorology (and since this is an AMS interview, I'll get into a little detail here) the so-called Residual Eulerian framework for describing stratospheric transport, which gives you a much more direct way of getting at basically how the winds blow ozone around might be the simplest way of stating it. And it's not just ozone, it's a broad range of other species. So when Rolando and I got that model in shape and began to get results with it, it was a tremendously productive time in the early eighties because it gave, right off the bat it's fair to say, a more realistic distribution of ozone than I think anyone had ever gotten before. And it allowed us to begin to look at latitudinal gradients with a much greater degree of confidence than I think one could have had before. Because the transport was so much more realistic. And you could see that. Just looking at ozone profiles or methane or whatever you want, I mean we've come a long way since then in further evolving that model, but even in those early days there were just a bunch of things. We wrote probably half a dozen papers where we were really able to examine things in a way that no one had ever done before in stratospheric chemistry.

Kellogg: Let's talk about that a little bit more, too, because this had to have been a very exciting time. All of a sudden, based on the Canadian observations, there is a realization that there is a huge ozone depletion going on.

Solomon: The British observations.

Kellogg: Sorry, the British observations. This came as a surprise, number one, but number two, it came really as a huge mystery. Nobody understood it. Can you tell us a little bit about just the excitement of being presented with that kind of a real-time problem, as a chemist, as a young chemist, and as a bright young chemist at a place devoted to atmospheric chemistry, to suddenly have this scientific opportunity?

Solomon: It was fantastic. I guess one of the things I'll never forget about it, I'm not going to mention any names, but there were a lot of people who simply said, "Oh, gotta be wrong. Can't possibly be right--it's just nonsense." They, of course, slowly changed their tune and I can understand how people develop that kind of hesitation about things. In fact, I think it's one of the things that a scientist really has to guard against as they get older--the belief that they already know everything. We can perhaps get back to that later, but getting back to the British stuff, it was really tremendously thrilling to suddenly see this ozone depletion. I looked very carefully through the paper, and I concluded pretty quickly that it had to be right. Now, whether it was due to chlorine or some natural process or cosmic rays or who knows what certainly was a big question, and one that took a lot of work to begin to get insight into. Yeah, I found it to be the most exciting paper I had ever seen, and I had it as a preprint. In fact, I was one of the reviewers of the paper, so I saw it in its very early stages and immediately began trying to figure out whether it was real or not, and what could be causing it.

Kellogg: So you believed that the Brits were right, that there was ozone depletion going on. Do you remember what were some of the early theories about what actually was the cause of this?

Solomon: I certainly remember them very well. They'll be indelibly etched on my memory for the rest of my life. There were really three scientifically credible theories: one was the idea that somehow this might be coupled to chlorine. And in fact the British in their paper said they thought that was the case, although they really didn't have a detailed mechanism. They had an idea about how it might work that was pretty quickly shown to be wrong; in fact, I even pointed out in my review of their paper the reasons why that mechanism probably wasn't the right one, although I felt it was a brave attempt, and it was worth pointing out that obviously chlorine was a candidate, certainly for this trend because the key thing about their data is that they started making measurements in the late 1950's, and for twenty years or so, everything was pretty normal. Now every year was pretty much like the last one, with some ups and downs that were related to dynamical variability. But sometime around the late 1970's, the ozone began dropping. And by the time 1984 rolled around, the ozone was about 30% below what it had been in the historical record, way below the natural variability. And that was the point at which the British said, well, this was more than 2Σ outside of anything we've ever seen before, so it's time to publish this. And they wrote their paper.

So they said it might be chlorine, didn't really have a mechanism but certainly had some really pioneering measurements. Shortly after the publication of their paper, there was a guy from NASA-Langley who suggested that the depletion might be due to the oxides of nitrogen produced in association with solar maximum coming down out of the thermosphere into the stratosphere. And this was kind of ironic for me because my Ph.D. thesis actually was on that very topic. In fact, I worked with Ray Roble and Paul Crutzen at NCAR, trying to quantify this whole issue of transport of NOx from the thermosphere to the stratosphere. And I looked at that as a possibility to explain this and pretty quickly convinced myself it couldn't be right for a variety of reasons. But this guy wrote a paper on it and actually got it published in Nature, saying that might be a mechanism, and indeed it's not impossible from a chemical point of view, but it just didn't really add up to fitting the facts in terms of the things like the shape of the vertical profile of the observed ozone changes--not just that you've lost 30% of the ozone. Even back then, we knew that we'd lost most of it between about 15-25 kilometers. In fact, I showed that in a paper that I wrote in 1985, where I once again remarkably (guess I hadn't really thought about this before) made use of that same ozone sonde record that I had started my career off with, working on tropospheric ozone. I went back to that database and found all the Antarctic data that had ever been taken. There's a lot of it from the Japanese station at Syowa. I was able to look at that, and convince myself at least that it looked like the ozone depletion was happening low down and the auroral NOx mechanism would produce an ozone loss much higher up, more like 40 kilometers, so it really didn't have much potential to explain the shape of the profile.

The third idea of course was that what happened was some kind of systematic change in atmospheric dynamics, and there were a number of authors who had papers published trying to propose different ways, different processes that might cause the dynamics of the stratosphere to change in such a way to reduce the ozone dramatically compared to what it had been earlier.

In late 1985, there were basically three different credible theories. The NOx theory, which as I said, I think, as more data became available pretty much got shown to be incorrect. The dynamical idea where it wasn't clear what was changing, but maybe something was changing the dynamics, and the idea of chlorine chemistry being involved. I did play a role in that myself; most particularly, I think it's fair to say I've done more to show the ways in which surface chemistry can matter to ozone than anybody else. By surface chemistry, what I mean is reactions between gas molecules and solid or liquid surfaces. It's a little bit akin to the idea of catalysis where there are processes that can happen in a surface that simply don't happen in the gas phase, or happen much more slowly in the gas phase. And in this particular case, what I picked up on was the fact that Antarctica being the coldest place on Earth happens to be a place where clouds can form in the stratosphere. Because the stratosphere is very dry, we normally don't have clouds at those altitudes. But in Antarctica one does, and they are very pervasive there, particularly in the winter and on into the spring. What I suggested was that the reaction between HCl and chlorine nitrate might take place on the surfaces of polar stratospheric clouds, thereby changing chlorine from a form which is not damaging toward ozone into other chemical forms that make it much more damaging. So you might say, transforming the ozone into a chemical that's just more able to destroy ozone.

So I was the one who suggested that, and that does indeed turn out to be the initiating reaction in producing the ozone loss.

Kellogg: Which was a tremendously important contribution. It provided the mechanism that brought together all this previous knowledge, the observations, and some of the other insights. I have a question here for you. In previous interviews, a couple of people have been quoted as saying that you display an incredible chemist's intuition, or "chemical intuition." I'd like to discuss that just a little bit in terms of the recognition of the possibility that the polar stratospheric clouds could provide suddenly a surface, to turn this whole scientific problem kind of upside down. It wasn't a gas phase, it wasn't a dynamical solution you came up, it was a surface chemical reaction solution.

How would you define a chemist's intuition, Susan? Within that definition, was there any intuition that came to play in suddenly recognizing the solution?

Solomon: That's a very interesting question. I like to think of atmospheric chemistry as being a little bit like organic chemistry. A really good organic chemist has a feel for how--in their case, of course, they're dealing with very complex molecules, with large numbers of atoms and all that. But it's fascinating how organic chemists who are really good at it have sort of a sixth sense of knowing what you can make from other molecules, or which reactions will go under what conditions, and which ones won't. Organic synthesis is full of that kind of stuff. I actually enjoyed organic chemistry when I took it. And I think that atmospheric chemistry is a little bit the same way in the sense that what's going on are literally hundreds of different processes. To some extent, they compete with one another. In some situations, one process will be extremely important; in other situations, totally unimportant. As you begin to develop a detailed understanding of how atmospheric chemistry works, you start to get sort of a chemical intuition if you will, about what tiny changes might actually drive something into a whole new state. I guess it was something like that that led me along this pathway. It is fair to say, I think, that people had never thought about heterogeneous chemistry in the stratosphere before. Not in any significant way. It was always assumed that everything that happened in the stratosphere would involve gas molecules because--well, for one thing, there are so few particles in the stratosphere compared to the troposphere. That's obviously true, but it doesn't mean that they're negligible. And the key thing about the lower stratosphere where the ozone loss happens is that it's a region that's photochemically not very active. So the rest of the chemistry is, if you will, fairly sluggish, and smallish perturbations can begin to be remarkably important. Particularly and--if this is chemist's intuition, it's frankly not very profound. The reason that HCl and chlorine nitrate reacting on a surface is so important is that those reservoirs take up so much of the chlorine at low altitudes. So exactly at the altitudes where the ozone maximum occurs--right in the heart of the ozone layer. Most of the chlorine is sitting in a form that's inert. By "most," I mean we used to think it was well more than 99%, so it's pretty obvious that a small change in one of those guys could give you a big change in the other guys. But then the question becomes, well, chemically how would you make that happen? You really have to begin to have some kind of feel for how all these molecules interact with one another to come to that kind of picture of what might be happening. I don't really have any further explanation for it than that.

Kellogg: I think it's important that the recognition of the tiny changes can have huge impacts. It's maybe as close to intuition as we can get in the form of a definition.

Solomon: It involves a certain amount of non-linear thinking, if you want to think about it that way.

Kellogg: So let's continue. We haven't even gotten you to Antarctica yet. Let's talk about the field program, let's talk about your leadership of the U.S. team that went down there. Can you tell us a little bit about the events leading up to the decision to do the field program, and then your selection as leader?

Solomon: In a way, there's a certain irony behind what happened. A very close colleague of mine at the Aeronomy Lab, whose name was John Noxon, had been doing field measurements for many, many years in which he did visual absorption spectroscopy, taking sunlight or moonlight and looking for their weak absorption due to NO_2 and ozone and other molecules. He was a real pioneer in this field and someone I had a very close personal relationship with as well. He was certainly one of my early mentors at the Aeronomy Lab. Shortly before the British paper on the ozone hole was published, John actually committed suicide, which, when I think back on it, is so incredibly tragic because he would have been so energized, I think, by the ozone hole and all that--his life might have been very, very different. But in any event, he died six months before the British published their work, leaving behind, if you will, a kind of rich Aeronomy Lab tradition of doing that kind of measurement. And also, leaving behind an instrument which he and his close colleague, another colleague of mine, named Art Schmeltekopf had been developing over a period of quite a few years, with the intent of measuring NO_2 and ozone. Actually what happened was we had a meeting in Boulder in March, 1986. The British paper came out in the May, 1985, issue of Nature, and we had a meeting in Boulder that was oriented around kind of the future of ground-based measurements of the stratosphere. It had been planned for over a year. It wasn't stimulated by the British work, but because suddenly everyone was talking about it--"Is there really an ozone hole in Antarctica, my God!"--we decided we would have a short session at that meeting about the different theories and what that might mean for what you could do from the ground and so I actually stood up and talked about my work. That was the first meeting where that idea of HCl nitrate was presented and, of course, lots of people pooh-poohed it, which I think is kind of funny in retrospect. But it didn't bother me then, and doesn't bother me now; that's kind of the nature of science--that people will be skeptical at first and that's good, that's one of the things that keeps us critical in science. So it didn't bother me. And there were also talks about the solar theory, and the dynamical theory. And then we had a little brainstorming session on what we might be able to do to really take the kind of measurements that would help to show what was going on down there. I should also say that in this early day, March, 1986, we weren't even completely convinced that the ozone hole was real. I mean, there were still people saying that maybe the British measurements were just wrong.

So just going down there to measure the ozone itself--if that was even all you did--at that point, was going to be very valuable, because people weren't even completely convinced that we really had a problem.

In March, 1986, we talked about who could go, and it was clear that the group from the University of Wyoming, who had been going down there for many years--they were planning to go anyway in November, but it would make a lot more sense for them to go in August, because we knew there was an ozone hole in October, but there wasn't one in February, from the British data. So something happened between February and October. The ozone hole opened up in that time frame. It was silly to think about going in November when it was already over. You want to go as soon as you could to kind of catch the whole thing in action. And to do that meant going in August, which is the earliest... Well, it either meant wintering over (and it was too late to do that because the station closes in February and we were talking about this in March) or going in August, which is the next opportunity to get into Antarctica when they fly six flights typically in at the end of August and they call that period "win-fly"--"winter-fly-in."

So they were going to go to August and we talked about some other instruments that could go. There was a microwave emission system that in principle could measure things like not only ozone, but also chlorine monoxide. We were very hopeful that they could measure ClO down there, ClO being critical in the ozone depletion cycle, measurement of that molecule (that's kind of the king molecule--if you can measure ClO you can tell how much ozone loss you've got). So that was a very essential instrument, and Bob DeZafra, Phil Solomon and their colleagues were involved with that from Stony Brook.

There was also an infrared absorption instrument by the JPL group, Barney Farmer and Geoff Toon, and they had flown that instrument, I believe, previously on a balloon, and they were available to go down there and try and do that. Then people talked about well, obviously, you'd like to measure NO_2 and ozone and visual absorption spectroscopy is how the Aeronomy Lab has always done it. But John was dead, and Art was committed to an experiment in Australia that he was very heavily involved in so...Here we were at this meeting and everyone was saying, well, it would be great to have the Aeronomy Lab instrument, but we don't have anybody to take it, so--And I just kind of said, "Well, I'll go." Of course, everybody laughed because I was a theoretician--I mean my idea of an instrument was a keyboard up until that time. But there wasn't anybody else, so they figured the worst that could happen would be I would get no data. And that's not how it turned out, fortunately.

So I decided I would go and the team was organized. A few months later, I'm talking about the difference between March and August, which is not a whole lot of time, sixteen of us were on our way to Antarctica. Now, that was a tremendous thing actually for the National Science Foundation to do. It was very, very difficult. Because normally if you want to go to Antarctica, you have to start getting organized about two years ahead so that they logistically can do all the stuff that they have to do, to get you on the airplane, the payload on Win-Fly is very tight--they only fly six flights in, they need to get a whole bunch of stuff down there--normally, they don't take scientists at all in August. We were the first group of scientists that were actually allowed to go in August because NSF put such a high priority on this. John Lynch at the National Science Foundation worked very, very hard to make all of that happen, and so there we were in New Zealand, waiting for a C-130 with skis to take us the rest of the way to Antarctica.

Kellogg: So how did you end up being chosen group leader, though?

Solomon: Somebody had to be the person who would do things like talk to the media, which is frankly hard work, and where you have to be pretty good at expressing things in a way that anybody could understand, and that is willing to put the time and energy into doing that. So I think that was part of it. Also, I think the fact that I was a theoretician was viewed as a little bit of an advantage because I suppose if you assume that my instrument is not going to work anyway--and one of the main jobs of the expedition leader is actually to make decisions and at times I did have to about priority among the other three experiments, you know, when we had logistical needs. You know, what was the first priority? You had to have somebody who was going to manage that in such a way as to maximize the science of their group as a whole rather than their own instrument. And so a theoretician is in some sense a prime candidate for that because they're not personally invested, of course. Actually, I don't think our instrument posed much of a problem because out of the four experiments I should say that ours was the one that required the least logistical support. So I think that was happenstance rather than anything else. But I really didn't have to make decisions about our instrument versus the others. More often, it was among those.

Kellogg: I still remain a little skeptical, Susan, that the only reason they made you the leader was that you could speak well with the media and your instrument probably wasn't in great competition in terms of scheduling or support, but we'll leave it at that.

Solomon: You can't get a swelled head about these things, you have to kind of view them in the least common denominator of how things might have been.

Kellogg: Right.

So there you are, you're down there, the field program was a tremendous success, tremendous amount of data, observations brought back, there is an ozone hole, was it at that point that you began to get these insights into the solution of your problem, or did you go down and confirm that the insights that you had already arrived at...?

Solomon: I already published that paper. I submitted the paper on the idea that the polar stratospheric clouds might mitigate the chemistry and enhance the chlorine by a factor of 100 at 20 kilometers over what it would normally be--I should say enhance the chlorine monoxide by a factor of 20 of what it would normally be, and produce the ozone hole. I submitted that paper in January, I think it came out in June. So it came out before I ever set foot on the ice.

Antarctica was incredible in part for me personally--

END OF TAPE 1, SIDE 1

Interview with Susan Solomon

TAPE 1, SIDE 2

Solomon: --it turned out that we had not only measured NO_2 and ozone, but also OClO, chlorine dioxide, which is a close cousin to chlorine monoxide. And in fact we made those measurements and were convinced that we had actually measured chlorine dioxide several months before the Stony Brook group became convinced that they had measured chlorine monoxide. So we actually made the first measurements of active chlorine showing that--in that case we saw there was about 100 times more of it than there should have been, way more than you could explain from gas-phase chemistry, had to be from heterogeneous chemistry, and was in the ballpark of explaining the ozone hole. So it was a fantastic experience. I mean, I think it's fair to say that I predicted that chlorine would be remarkably enhanced because of polar stratospheric clouds, and I made the first measurements that showed that it was.

I should also say that the ability to make those measurements, that was an instrument that I did not build, did not design, and the person who kept it running was my good friend and colleague, Ryan Sanders, who works with me at the Aeronomy Lab. Anytime we had breakdowns in the system, it was always Ryan who fixed them, so he deserves a lot of credit.

But where I did, I think, make some contribution was in having some insights about how to make the measurements to be able to show that we had chlorine dioxide. And I'll never forget--we knew we wanted to look for chlorine dioxide, of course, because we knew that it absorbed in our wavelength range and ought to be just about detectable if chlorine was causing the ozone hole. It's a molecule that breaks down in sunlight or photolyses pretty rapidly, so we thought we would only see it at night using the moon as a light source. And that was pretty exciting, standing up on the roof in a building in Antarctica with -40° temperatures and 40 mph winds was a tremendous challenge. And we had to hold the mirror up there and do all this. So it was exciting physically to make those measurements, and of course, intellectually as well. But we knew the moon would be our best light source, but like all absorption measurements, you do the measurement relative to another background spectrum. And so what you have to do is take a spectrum under some set of conditions and then divide it by a spectrum under another set of conditions to do what we call "Beer's Law," which allows you to see the absorption. I'll never forget having done some measurements and it was getting on for about the middle of September, and I was walking back to my room and it was pretty cold, probably about -20° or so, but you get used to the temperature down there, and I suddenly looked up and saw the sun. It was probably the first time I had seen the sun since I came down at the end of August, and I [thought], oh, that's great, the sun's coming up, you can actually see it now, it's above the horizon. And I suddenly realized that would make the perfect background. Because if you divide a spectrum that has a lot of OClO taken with the moon by one that has presumably almost no OClO taken with the sun, you're going to maximize the difference between the two and maximize your chance of actually getting a good measurement of OClO. Up until that time what we were doing was taking a spectrum of the moon when it was very low on the horizon and had a big path through the atmosphere by when it was almost overhead or as overhead as it got, which had a smaller path but still a finite amount of OClO, and it's much better to have zero. So I immediately turned around and went back up to the laboratory and took a bunch of solar spectra. And it did indeed turn out to be a way of basically enhancing our signal by about a factor of two, and helped us to be able to see the OClO. It seems like a fairly obvious thing, but I don't believe I would have had the insight to think about diurnal chemistry and what backgrounds you could use and all that stuff--especially not after basically weeks of working 18-hour days, you know, if I were not a theoretician sort of by training. I've had a lot of insights into how to use these instruments that were really due to the blend that I was doing between doing theoretical work and experimental work.

Kellogg: So how long were you in Antarctica at that time?

Solomon: I was there for almost three months.

Kellogg: I want to interject here that this is getting ahead of ourselves a little bit, but I happen to know for a fact that you have written a mystery novel called Death on Ice, which takes place at the South Pole among a bunch of scientists who are wintering over. I hope it's not based on any real-life experience?

Solomon: No, no. But it was a lot of fun to do and it probably comes from the tremendous love that I have for the place, which I think comes out in the writing. But this mystery novel has been in the works since 1987, has failed to find a publisher, so I don't know if it will ever see the light of day. But it's been a lot of fun to do.

Kellogg: So that was in 1986, you came back, and what did you do then, Susan? What did you do then, and then I want to talk about the whole political dimension of the ozone hole problem and get your impressions about the role of science in policy-making. But we'll get into that in a minute. What did you do when you came back?

Solomon: We had a lot of data to analyze. We left the ice in November, being pretty convinced that we had seen OClO with the moon. And obviously that was the most important piece of our work. We'd also seen NO_2; it was easy to show, by the way just as an aside, that the NO_2 was very, very low. Now if NO_2 was going to be the cause of the ozone hole, it had to be high and not low. So I think our measurements very clearly were kind of the last nail in the coffin on that whole issue. And that was an easy measurement for us to make.

But here we were, faced with this blockbuster: My God, we've also measured chlorine dioxide! And of course, we also saw the ozone go away (getting ahead of myself a bit). All four instruments were able to very clearly document that the ozone was normal at the end of August. And it is was kind of a remarkable experience to watch it drop. I mean, it dropped very systematically, so that by the end of September, there was only about two thirds as much as there had been at the end of August. It shouldn't have done that--they all showed it, three different instruments, three completely independent techniques. So we, I think, were able to play a major role in convincing the world of something that it probably forgot that it ever needed convincing of, by this time. But I remember very clearly that our measurements were very important at that time, in that regard.

But we'd also measured this blockbuster molecule. I mean, if there really was 100 times more chlorine dioxide than there should have been, ballpark of a part per billion of ClO involved, that meant the chlorine was causing the ozone hole. I was very careful not to reveal that to the media initially. I was very well aware that I had to be extremely sure that that was the case before I started talking about it, so I kept just telling people, "Well, we're analyzing the data, and don't worry--you'll be the first to know whenever we find out." So I pretty much steeped myself in analysis of data for the next several months, and had a little bit of an epiphany when I found a way to actually pull the chlorine dioxide signal also out of our daytime data. We routinely took data every single day in which we collect scattered light from the sky. We point the instrument straight up and were just collecting the downwelling of scattered radiation. That's a completely different mode of operation from when we look directly at the moon or directly at the sun. And I came up with a way of getting chlorine dioxide out of that data as well by making use of its zenith angle dependence. Because again, as the sun rises and sets, the scattering path through the atmosphere is changing dramatically, and it took a little bit of work to the way we normally analyze the data to actually realize there's a way to handle that data set that would allow me to see chlorine dioxide. And in fact, the signal, as the sun was setting, goes way up, because chlorine dioxide stops photolyzing, because there's a lot less sunlight as the sun is setting. It's getting attenuated. It's not as high as it is when you hit the nighttime and the moon, but nevertheless it's considerably enhanced over what it would be say at noon, and that actually allowed us to it in the daytime data as well. Well, now, we had the full diurnal cycle. We had daytime, we had nighttime, it was 100 times more in both cases than what it should have been. We even were able to pretty well look at how it varied as the sun set, you know, start following it with the sun, have the sun go down, see it grow as the zenith angle increased and then pick it up with the moon and see it grow in the lunar data. So it got to the point where I had done so many different things to convince myself that it was real that I began to be pretty convinced. So that's when I wrote the paper. And if I recall correctly, I think I submitted the paper in January, 1987. So there was a pretty intense period between November and January of analyzing data and writing papers, pretty intense.

Kellogg: Let's talk about a comment that appeared in a couple of your previous interviews, where you talk about just because you're a scientist doesn't mean you have expert opinions on everything, and the fact that you prefer to keep your science "pure." Could you explain that position a little bit more?

Solomon: As soon as I got home from Antarctica, and even before I left Antarctica, the media were after me to find out what I had to say about all this. We did have a press statement which we released from Antarctica. When I go back and read that statement now, over ten years later, frankly I think we played it exactly right. We made careful statements about the things we had done where the technology was very clear--like measuring in NO_2--we had a long history of doing that, it was an easy measurement for us, there was not a lot of NO_2. I had a lot of confidence that we could simply say that and we did. By the same token, the group from the University of Wyoming in measuring ozone with ozone sondes for years, they were some of the world's experts in it--it was pretty easy to show that the ozone was depleted in a remarkable layer between about 15 and 20 kilometers. We had confidence in being able to make that statement. We were careful not to make statements about things that were really going to require, if you will, the standard kind of vetting by the community that involves the peer review process and all that, and I feel very strongly that scientists should always do that. So we didn't say anything about chlorine dioxide, we didn't say anything about chlorine monoxide until those papers had been accepted for publication.

I suppose you could argue that we could have done that differently but I don't have any regrets about having made that choice of the way to do it. I think it's the only way to do it.

What happens as you begin to get involved in these things is that you get a little bit carried away with the need to be quick. I mean, I think society will always pressure a scientist to release their results--you know, "give us something now." In most cases, six or eight more months, or even a year of delay really doesn't change anything. It just makes the work more credible, it makes the work more solid. And it takes a lot of looking at it and thinking about it before you realize that that's true, before you kind of realize that there's no benefit, even to society, in rushing to get stuff out. Because society can't absorb it that quickly anyway. Society's rate of assimilation of information is actually fairly slow. It's gotten a lot faster in recent years, you know, with CNN and all the other media mechanisms that we have to get the news quickly. But even though we're barraged by all this information, our absorption of what it really means remains actually quite slow. And, I don't think the world would have been served by our having made any statements earlier. I don't think the world is well-served, for example, by the very premature statements about cold fusion, for instance. I think that's an example of the kind of thing I'm talking about. I think scientists generally make those kinds of statements for the very best of reasons. I mean, they really want to tell people what they're doing, it's exciting, you know, you believe in it, of course, you wouldn't be doing that if you didn't. I think people in general do it for the very best of reasons, but it really doesn't help the process. And I spent a lot of time thinking about that in those days. I was completely barraged by phone calls. My phone was trying to ring every fifteen minutes after I got home from Antarctica. I got one phone call while I was in Antarctica, before we had our first press conference, from a reporter from a very well-known famous American newspaper, a mainstream newspaper, very well-known reporter whose name will remain nameless, who called--in Antarctica, and in order to do that, he had to get into the emergency system and he must have claimed that he was a member of my family with a personal emergency--that was the message that I got in Antarctica--I mean I thought my brother was dying or something, or God knows, what terrible thing might have happened in my family. And then I found out the name of the person, who was someone I had spoken to actually, had given an interview to before I left for Antarctica. Of course I refused to take the call and I also informed the switchboard--and fortunately, one thing about Antarctica is that when you call you don't dial the number of the room you want to talk to, at least you didn't in those days. There was one phone line in and out. It cost $10.00 a minute, and was only to be used for serious business purposes or personal emergency purposes. And this guy got in by trying to claim that he was a member of my immediate family with a personal emergency. Maybe that was part of the reason why I developed the kind of attitude that I did, maybe it was good in a way that that happened. Because it really made me realize that I had to keep whatever happened scientific, I had to keep it as clean as I could keep it, I had to keep it in my control. I couldn't allow the release and the use of what I was doing to be controlled by the agenda of other people. I had to keep it on my agenda. And I think that was a good lesson to learn and I've tried to do that all these years.

Kellogg: We may come back to this question of pure science--the role of scientists with policymakers--a little bit later on.

Now, between the time you got back from Antarctica within five, six years, you were accorded one of the highest honors that's available to American scientists: you were elected to the National Academy of Sciences. Susan, you were the youngest member ever elected at that time in 1992. (I'd be interested to know who was even younger than you after 1992).

Solomon: Well, actually, the youngest person ever elected to the National Academy was Julian Schwinger, who is, of course, a very famous quantum physicist who got elected at 29, I believe, on the basis of his Ph.D. thesis. So I was quite a bit older than Julian Schwinger. (I was 36).

Kellogg: Tell me what that was like, though; 36 is an extremely young age anyway, but especially as a scientist to have made a mark and to have made such a mark by that age. Tell me what it felt like to you when you were notified of that election.

Solomon: I was of course extremely thrilled. I had no idea that anything was happening. And in fact as I look back on all that entire period, I think that the thing that's good about young was that I was so young and naive that although of course I enjoyed very much the science, and I was incredibly into what I was doing scientifically, I had no clue as to what its ultimate impact on my personal career might be or any of that. I was perhaps just so very young that I didn't even know that such things happened to people. I certainly didn't think about it ever happening to me. I guess as you get older, you probably begin to realize, "Oh, well, you know, I know so-and-so, and they got elected to the Academy and maybe I would be someday." I'm sure that kind of thing must go on in people's minds at times, but it never has in mine because I just never thought that way. Events kind of overtook it, if you want to think about it in that sense.

So, yes, I was incredibly thrilled, but yet, I've never thought of myself as "young" or as a "woman" or any of those kinds of things. I mean, I don't think that way. When I walk into a meeting, for example, I'm not going to notice if I'm the only woman in the room, I'm not going to notice if I'm the only person under 40 in the room. I simply don't notice those things. Now again, it's maybe related to being naive, it's related to being very focused on what I'm doing. I wouldn't have known I was the youngest person if they hadn't told me, and I don't normally think about it. Even now, when I go to an Academy meeting, I suppose you look around, you sort of do--at times, I do notice in the Academy that most of the people are fairly elderly. But not much of the time, actually. I mean, it's not just something I think about.

Kellogg: Susan, you have actually been elected to two academies. Besides the U.S. National Academy of Science, you were elected recently to the French Academy of Sciences. Could you tell us a little bit about how that came about?

Solomon: All of the international Academies of Science have what they call "foreign associates," which is a similar status for foreign scientists to sort of develop connections to their own academies, and it's quite a wonderful honor, actually, to be named to a foreign academy. Of course, for me, becoming a member of the French Academy was not only a tremendous surprise, but has a very special significance because of the time I spent in France and my very strong affection for that country.

Kellogg: Is that election based also on research accomplishments similar to the National Academy election?

Solomon: Research accomplishments are certainly the prime issue. Different academies may place different emphasis on young versus old or that kind of thing. The French Academy is of course one of the oldest in the world, and has a very distinguished scientific history, so it's a tremendous honor really to be part of it.

Kellogg: The other kind of global acknowledgment of achievement is the Nobel Prize. And recently, the first prize ever given in atmospheric chemistry was given to three researchers, one of whom you've been very closely connected with. Drs. Paul Crutzen, Sherry Rowland and Dr. Molina. Could you tell us a little bit about the Nobel Prize and your contributions also to that science?

Solomon: I'm glad we're talking about this because I really wanted to emphasize that I think that the Nobel Committee very wisely chose the three most qualified people for the Prize. One thing that people may not be aware of broadly is that the Prize is limited to no more than three recipients. That's what's stipulated in Alfred Nobel's will, so even if there are five or ten or however many people contribute to a particular scientific discovery for which the Prize is awarded, the conditions of the will require the Swedish Academy to choose the three most deserving recipients. And I think they chose exactly the right three people.

I was very pleased to be able to actually go to the ceremony in Stockholm. Paul Crutzen invited me to come as one of his personal guests. It was a great thing to watch Paul and Mario and Sherry, who are also good friends, receive the Prize, and of course, it's a tremendous thing for atmospheric chemistry to be recognized in this way as a real major scientific achievement. Certainly understanding that ozone depletion would happen is something you have to appropriately attribute to the work of Molina and Rowland. They were the first to point out that gas-phase chemistry could cause ozone depletion, that CFC's have lifetimes in the atmosphere of 50 to 100 to even 500 years depending on which one you're talking about. And that therefore, any atmospheric change would last for a long time. Crutzen also made unprecedented contributions to our understanding, particularly with regard to the NOX chemistry that can lead to ozone loss. Taken together, I think it's very, very clear that the Swedish Academy made very much the right choice in the recipients of the Prize.

If I had to describe my own contributions, they lie in the later work on ozone depletion, particularly heterogeneous chemistry. What I've done the early work on is recognizing that heterogeneous reactions could be an important cause of the ozone hole, then doing the work on volcanoes has also been a major thing for me and the most recent work in explaining mid-latitude ozone depletion has also been the thing that I'm probably the best known for. Of course, I have to say that being able to make the first measurements of chlorine dioxide, which, as we discussed, point towards chlorine as the cause of the ozone hole, is also something I'm particularly pleased at having been able to do.

But those were later contributions and the way the Swedish Academy worded the Prize was very clear and very appropriate that they were giving it for the first indications that catalytic reactions could deplete ozone, for which clearly, Molina, Rowland and Crutzen are the ones who made that contribution.

So I think it's important for the public to understand that there's always questions. The Swedish Academy really does a remarkable job at sorting through all the different contributions that people have made, and coming up with their judgment of what the appropriate thing for the Prize is, and I think they did an outstanding job in this case with a very difficult problem.

Kellogg: And, as you say, it had a tremendous boost to atmospheric chemistry to be recognized this way.

Solomon: I think to atmospheric science in general, not just chemistry. Certainly it has been a boon to what we do. It's interesting--you know, in this country, there have even been people who've said, "Well, you know, the Swedish Academy only did that because they wanted to make a political statement about the ozone layer." I don't think that's true, actually. I talked to members of the committee who were involved in making the choice. It was a very difficult choice and they were looking at a variety of different issues. They chose to describe the reasons for what they did in a very, I think, fair and--I want to use the word "dispassionate"--but in a very pure scientific way. "These are the reasons why we're giving the Nobel Prize in chemistry this year on this topic." It was extremely scientific. I actually had the opportunity to talk to them about the fact that the Prize was controversial because in fact in Stockholm there were people standing on the other side of the street demonstrating against it--there are "anti-ozone" elements even in Sweden, which is in general a very environmentally-conscious country. But there were such people out there, and I was wondering whether these Swedish academicians would begin to worry about whether they had made the right choice, seeing this. I said, "Does this bother you? There's all this demonstration, anti- the 'ozone' Prize." The guy laughed and said, "No, no, no. Half the prizes are controversial for one reason or another." They had recently given a Prize that had do with a nuclear physics type of experiment that was actually conducted during the Bomb era. So there were people demonstrating against--"this work would never have happened without the pain of the Japanese people," which of course I think we all agree is a terrible tragedy, but nevertheless it gets back to this issue of separating the science from the emotional policy-related matters that in this case were considered, were carefully thought about by the Swedes, but were clearly neither unusual nor a deterrent for giving a Prize where they thought it needed to go.

Kellogg: Just in passing, you've mentioned [discrimination], and you've spoken about this quite a bit in previous interviews and I don't want to spend a lot of time on it here, but your approach towards discrimination as a woman in science and your position on that, I think, has been very straightforwardly dealt with. You do not feel that you have been excessively discriminated against, that while there may have been some incidents in discrimination, there have been many more incidents of supportive behavior from scientists, men scientists and women scientists alike. One of the things you've said that I've found particularly interesting was a story about a woman colleague and how you felt that as a woman, she tended to compromise too quickly in discussions. Would you tell us a little bit about that and what you see as being the misinterpretation of that willingness to compromise?

Solomon: I will just repeat it briefly here: I personally don't feel that I've ever been significantly discriminated against as a woman scientist. I have encountered a handful of men in my life where the instant you spoke to them, before you got beyond "hello," you immediately received a message sent in some fairly non-subtle way, usually, that you may as well not bother because that person will never respect you, they will never listen to you, they don't view you as a human being, and it is painful. But, we're talking about a handful, literally a handful of people. On the other side of the ledger, there have been certainly dozens of very supportive colleagues who have taken pains to send the opposite message--that they actually enjoy being around women scientists, they're interested in supporting the careers of women. Certainly there were a lot of people that helped me, that were supportive of me when I was younger and even now. So you've got to look at both sides of the ledger and people too often are so hurt by the first kind of experience that they never really recover from it, and it scars their attitude towards everybody and everything.

Maybe I'm just too indestructible, I just never let those kinds of things bother me. And they really didn't bother me that much. I think you also have to keep a sense of humor about things. A lot of what goes on is actually pretty funny, and if you can continue to keep things light, it helps a lot rather than getting all sort of bogged down and angry about things. I guess one example that I like to tell is that when I was in New Zealand with a group of sixteen guys about ready to go to the ice, a New Zealand reporter asked me, "How does it feel being a woman working with all these men?" And I always hate that question because it's a terrible question. You can either sound like a misogynist or a nymphomaniac or some combination of both, and you don't want to be any of those things. So I gave him what was really almost an honest answer. I looked around and I said, "Wow, they are all men, aren't they?!" As if I had simply not noticed up until that time. Of course, everybody laughed. And that was a much better way to end that conversation than by making a big deal out of it.

The issue of how women interact with men as scientists is really the critical thing. You asked me about this whole matter of compromise. I had a female graduate student who was from a family of four sisters. She was very bright, but had really failed to kind of communicate her talents to people here at the University. I'm an adjunct professor at the University of Colorado so I have students, and she ended up working for me. And I began to see what the problem was was that she was just too quick to compromise. You would have a discussion about something. If there was any slight amount of disagreement, she would try to see things your way. Which is perhaps a natural feminine--

Kellogg: --"social mechanism"--

Solomon: --characteristic. And in some ways that's of course very good. It's one of the ways women are able to communicate, I think, better and at a deeper level with one another than men very often do. And of course all these gross generalizations are very unscientific and don't apply to everyone and maybe don't even apply--well, I think they do apply, there is some validity to many of the stereotypes that we have, and this is one where I think there is some truth to it. So I began trying to kind of teach her, yeah, it's fine to compromise, but you also need to listen to your own inner voice and to know when what you're saying is actually right, and the other person may indeed be wrong. That doesn't mean you have to be nasty, but you have to actually be fairly forceful in sticking up for your own views. This is something that many women find almost impossible. And it's one of the reasons I think that women have such a hard time in science generally.

Kellogg: This leads me to another question I wanted to ask you about the role of mentorship. I know that you have been lucky to have some superior mentors in your scientific career. Can you tell us a little about the impact, the influence that Paul Crutzen, Ray Roble, David Gutman from your undergraduate years, had on you, and more generally, what you think the role of mentors is for helping scientists develop their careers.

Solomon: I think in almost any career, it doesn't have to be science, mentoring can play a very important role. I think in science it plays even a more important role perhaps than many other professions. Because science is an intellectual worldwide enterprise in which what is happening is that people are emerging as players. To emerge as a player, you have to be able to participate, you have to begin to be invited to the table, you have to begin to move in the circles of the people who really are, if you will, the world leaders so a mentor can bring you to that table and start you off. In that sense, it's very, very important.

I also think it's important in science because there are many different styles in which you can do science. Some of them in my view are rather negative and some of them are very positive, and every scientist makes their own personal choice about which pathway they're going to take. And like all social animals, one of the influences on what choice they make is what kind of society they find themselves surrounded in. Whether you become someone who has a very aggressive, very sort of negative difficult personal style with your colleagues which may even in some cases extend to not only being slightly nasty, but maybe very nasty--I mean, we've all seen scientists whose idea of a good time is to basically tear somebody up in pieces just for the hell of it. And even when they agree with that person. I have seen people do that. I understand the problem and I know the issues well enough to know that person "x" actually agrees with person "y" but they enjoy the process of tearing them up and for them it's sort of an intellectual game to the point where they do it anyway. I must say that I don't believe I've ever seen a woman do that. A woman might disagree. You know, Person "X" might disagree with Person "Y," but only if they really, truly disagree. A woman doesn't do it for sport.

Kellogg: Agreed.

Solomon: There may be exceptions to that but I believe that's a general rule. I can certainly be a person who is critical of other people's science. I try--I don't always succeed, but I try not to be critical of them. I try not to be nasty about it, but if I disagree with someone, I will let them know. And if it's something important, I can be very--I guess I'll use the word "assertive" in describing my view. I do try really not to be nasty. At times, I lose my temper--we all do, but I think that whole personal evolution towards a style is one of the main things a mentor does for you. A person who has had a major influence in my life in that way is actually Dan Albritton, the director of our laboratory at NOAA, because he is really a gentleman and has a philosophy that reflects that. And it's actually not just Dan; that's one of the very good things about a number of the senior people at the Aeronomy Lab, all of whom have had a big influence on my thinking in this regard. And that basically is not just Dan, but also Fred Fehsenfeld. and George Reid have been the prime people who've affected me in that way.

I'm not as restrained or as polite as I'd like to be at times, but I aim for that at least, which is a good aim, in my view.

And mentoring in that regard actually starts pretty young. I can recall feeling that same way about David Gutman, who again was a person who had--he was a tremendously bright guy, but also had almost a personal gentleness about him that you could feel. You realized that you were dealing with a person of great humanity, and that was a very good influence. Similarly, with Ray Roble, who's also a very human and very--a gentleman and a scholar-type of scientist, as opposed to what some other types of scientists might be. Paul Crutzen is someone who I believe also is a very caring person. He has also played a big role in my life, through really the way he taught me how to think about science and about atmospheric chemistry. He spent a tremendous amount of time with me when I was a student. I was very lucky to be able to come to work with him at a time when he actually had already decided to leave the United States for Germany, but hadn't gone yet, so he was kind of freeing himself from his obligations here, but he wasn't going to move for another year and a half. He used to spend an hour a day with me, which is phenomenal for a graduate student. And he taught me a lot of stuff. He's got fantastic chemical intuition.

Kellogg: To pick up our chronological story here a little bit: so here it is, 1992, you have just been elected to the National Academy of Science. Your science is going so well. You're happy at NOAA. What's happening in your personal life, Susan?

Solomon: Well, I guess the personal life is great too. I met my husband in the late 1980's--actually in 1987 right before I left for Antarctica in my second year down there--and got married in 1988. We've been married for almost ten years now and my personal life is great.

Kellogg: Something we've talked about--there is kind of a new dimension here. You're suddenly a public figure. You've been well-publicized because of the ozone hole and your work, the election to the National Academy and all of a sudden you're not that post-doctoral researcher, you're suddenly a public figure of a scientist and a very high profile scientist. What kind of impact do you think that's had in the last five years?

Solomon: A lot of it has been fun. At times, it can be very satisfying to participate in something you really think makes a useful contribution to educating the public or to, if you will, providing a role model and I have trouble with that sometimes because I think there's a fine line between a model and a mold, and I don't want to be a mold, but I don't mind being a model, along a range of models. For example, one of the things that was a lot of fun to do was the work I do with the Smithsonian Institution on the "Science in American Life" exhibit in Washington, D.C., where they have a lot of nice stuff in there but one of the things they have is a computer-driven display where kids can learn about different scientists and I'm actually one of the scientists where they get to push buttons and find out stuff about me.

Kellogg: Do you talk?

Solomon: Yes, I talk, you know, all that stuff. That's kind of the fun part of the visibility, especially when you can be involved with that level of operation. I learned at a very early stage in all of this never to do AM radio. I mean, I'm sorry if I'm slighting--there must be some great AM programs out there, but frankly most of it is poorly researched, involves trying to get you to say controversial things, very un-useful in the sense of providing any education for people. It's just "shlock" radio. So I learned early on just never to do anything but Public Radio; I still do Public Radio interviews. So you kind of begin to develop a discrimination between what's worth doing and what's not worth doing. You get burned sometimes. I'm not going to mention any names, but I gave an interview to someone writing a magazine article, a respected writer for a good magazine and I felt that the end product was very poor. So you start to become a little bit cynical and you begin to learn to distance yourself and I won't say "never trust anyone"--that's probably TOO negative)--but to begin to realize that there's nothing wrong with testing them. I mean, they call you up and it gets to be a real drag at times. Sometimes they're doing a piece on something and they're basically auditioning you because they're going to interview half a dozen people and use quotes from two of them. Well, frankly, I don't have any interest in competing for who can produce the best sound bite. So I just do my own thing, and if they like it, fine, and if they don't like it, that's fine too, and I don't care whether I'm one of the two they pick or not. What I do care about is not having my time wasted. So I begin testing them, and if I find that it's someone who hasn't done any research on it on their own, is trying to rush through the whole thing too fast--you know, they've got a 3:00 deadline and they've just started working on it at 2:30 and they called you up and they would like to talk to you for the next twenty minutes so you can fill them in on everything that's happened in the last ten years. I wish I was joking when I say this, but it happens. You know, you begin to learn that your time is better spent not doing that because the article isn't going to be any good anyway, so I don't respond to that kind of call, or if I do, I keep it very short, and I tell them that they really need to take more time with it, which they ignore and go on and do their own thing anyway.

So you get a little cynical. You asked me, what's the impact on a person? It's a tremendous and growing sense of cynicism, actually. The real sad part is you begin to realize that out of let's say a dozen articles by reputable reporters in very reputable magazines or newspapers, there's probably going to be only a very small number that are good. Which is OK. Except then you begin to wonder, "Well, how much of what I'm using as my information sources about things that I don't know anything about, is wrong?" I mean, how often when I read about political developments or other areas of science that I'm not familiar with, how often am I getting the kind of crap that people are getting when they read 80% of these articles? I'm not sure if there's a solution for it but there's such a rush to get everything out the door quickly, rather than get it out with quality that I'm afraid that the level of information that the public is getting, the quality is just increasingly poor.

Kellogg: Let me ask, though, I'm curious because there's been an awful lot of talk in the last few years about the general science illiteracy in this country, and there's been a lot of time, a lot of words, and a lot of money spent on trying to improve science literacy. But what you just said suggests it isn't limited just to science, that the quality of information that most people are given, either through the media or even through well-respected sources, tends to be hurried, incomplete and not comprehensive. Do you see that that is even more dangerous for science because it does require a certain level of understanding or do you think that it really is pervasive, that most of the information about our complex world is inadequate?

Solomon: Yes, I actually think that's pretty pervasive. And I would hesitate to say that it's any worse in science than in anything else. In fact, in most respects, I suspect it's better in science. That's the scary part. You know, in a lot of cases at least you are dealing with science writers who generally are trying--I really don't want to leave such a negative impression about the media. Some of them do a remarkable job and they do a remarkable service to society by writing really good stuff. It's just that it's all so rushed. And no one can get it right when they're in that much of a hurry. I don't care how brilliant they are. My guess is when you're rushing to report on science, you at least kind of get the rudiments. I mean, even the worst story on the ozone, which may call it an "ionized gas" or may describe the ozone hole as being in the Arctic instead of the Antarctic and all that stuff...but at least it's sort of got the broad features in some sense. I suspect that a lot of what we get in more subtle issues isn't even at that level correct, which makes it even more frightening to me.

Kellogg: There's another aspect here. We take our best researchers and as soon as they've made significant research contributions, we use up all their time, making them sit on committees or testify before Congress or respond to media inquiries; we don't let them do research anymore. How have you handled that constant tension between becoming a spokesperson for your research while retaining enough time to actually do research?

Solomon: That is a very big problem. It's one that I came to grips with fairly early in my career, which is probably good. I began to see the thread of this as early as the late 80's. You know, I began to have to turn down a large number of things. I guess the sad part is, as I've said before in connection with something else, is that there's a process of learning to be cynical about these things. When you're first invited to be part of an NRC committee, you think, "Oh, wow! I'm going to be on this Academy panel and I'm sure what we're going to do is really interesting and I'm sure what we're going to have to say is going to be really important." And then of course you begin to realize that most of what Academy panels do is not very interesting and that, even with the best of intentions, very often the results are not very useful, and the world would really have come out just about the same if they had never existed. And of course, I don't want to pick on the Academy--in fact, I think on a scale of one to ten, Academy panels are probably more useful than most other ones but all of these advisory committee functions--I think we have too many advisory committees these days. A lot of it is going through the motions. You know, everybody's got to have one because everyone else has one. So you get cynical. I mean that's unfortunately how it is. You begin to realize that these things are not really that much fun, they are not really even that useful a lot of the time. So you being to develop a discriminator for how useful they might be, and choose the ones that are likely to be more fun for you and be more interesting for you. And that's kind of just a process of experience.

Another really important thing is to keep your calendar under control and a couple of things that are very important in doing that are to set up in your mind what is your ideal state. Is your ideal state to travel no more than one time per month, or two times per month? For me ideal is probably zero, but acceptable is once a month. And unacceptable is more than twice a month. So...

END OF TAPE 1, SIDE 2

Interview with Susan Solomon

TAPE 2, SIDE 1

Solomon: You begin to realize that you need to look at your calendar and say, "No, I can't do that thing the week of the 20th because I'm doing something else the week of the 10th." And even though there's nothing explicitly in conflict the week of the 20th, it's in conflict with what I need to make my life what my life to be, which is not traveling more than once a month. And that's the trap that I think it's too easy to get into to not doing is to simply say, "Well, I'll just squeeze in as much as I can." As you become more and more visible in any field, you can get to the point where you could be traveling every day if you wanted to, easily. Some people enjoy that, and more power to them. But I don't. So what I do is, I never say yes to anything on the telephone. If anyone calls me up to ask me to do something, they will invariably get the answer that I need to check my calendar and get back to them, but that I can't make a decision right at the moment. Because I want to sit down with my calendar and think about how much time I really have, and how much time I need for me. And it's really for my research that I'm talking about because I don't want to get--I see the value in being a spokesperson for the field and a role model particularly, frankly, for women, women students. And I see the value in communicating to the public, but I enjoy doing my own science too much to have those things become any bigger in my life than they are right now. And right now they're sitting at a level of probably on an average of somewhere around 20% or so of the time.

Kellogg: You did, though, just within the last two years, take on a particular appointment away from NOAA, at the National Center for Atmospheric Research, that gave you the opportunity--or should I say put you in a situation where you became a science program manager for a year, as Acting Director for the Atmospheric Chemistry Division at NCAR. Can you tell us a little bit about what that year was like for you--high points, low points, and what you took away from that experience?

Solomon: I guess it's probably true at some very deep level that the fact that I was going back to NCAR twenty years later in the position that Paul Crutzen had had at the time that I was a student, had to be, at least on a subconscious level, a reason why I thought it would be an honor to do the job. And it was an honor to do the job. I mean, NCAR is a great place, and I had the opportunity to work with some fantastic people there. But I went to it knowing that I would very likely come away from the experience feeling that management was really not a role that I wanted to participate in on any kind of longer term basis. I took the job partly because I do get a lot of inquiries these days. Let's face it--I'm 41 years old, I've got a few hopefully good years left in my career; I'm still young enough to be energetic and I'm known and all of that. I get inquiries from people about various management-level jobs because it's perceived that that is the promotion ladder, if you will, in our field. And I've thought a lot about this. I don't think there's anything to be ashamed of in doing management. I think it's a wonderful service. What it does is to foster the careers of other people in science and certainly people who are good at it and do a good job of it do a tremendous service for their colleagues. But is it really a ladder towards bigger and better things? For me, I knew pretty well before I went to do this that it wouldn't be, but having the opportunity to really experience it firsthand was very good for me. And I confirmed my belief that for me, that's really not my ladder. I may someday change that view, you know, as my own life evolves and different things happen, but for right now certainly, that's not the ladder I want to climb. I find the scientific ladder much more rewarding and in some sense, I'm almost addicted to it. You know, I just can't be happy not spending a lot of my time doing science. I love doing programming. I can spend all day finding a bug in a computer program, or analyzing my Antarctic data or whatever. I really enjoy that, and I much less enjoy the management aspects. I like people, but I love science, and I can't give up the personal hands-on aspect. Maybe someday I'll change my mind about that. A lot will have to do with how much longer I can continue to feel that I'm able to do work that's on the cutting edge. And you know even now there's times when I see some of the young hotshots you know come along, and they impress me. There's some things that some of the young people can do, certainly with computers, for example, that I'm having a hard time keeping up with. I can still make my own graphics and things like that but they're not as nice as what some of these people can do. And they do it so casually. They're so good at it. I'm trying hard to keep up, but it gets harder as you get older.

Kellogg: Would you ever consider a stint in academia, either as a department head or chair?

Solomon: Those opportunities are also ones that I've had a chance to think about and look at at times. I enjoy teaching, but again I feel I enjoy my own personal research too much and the problem with that kind of job is again the amount of time you have for doing your own personal science is reduced. It's going to depend on what kinds of things I find that I'm able to continue to do. There are some people--and I'm privileged to know some people like George Reid and Fred Fehsenfeld who have spent their entire careers and are now in their sixties doing, in my opinion, fantastic work. I'm going to have to see if I can continue to feel if I can be one of those people. But I think, again for me there are a lot of different paths that I feel that I could take at some point, any one of which would give me satisfaction. I just have to know which path is the right one at the right time. So I'm not ruling anything out for the long-term future, but I think the main thing a scientist--we are very, very fortunate as scientists: it's a field in which you can grow old gracefully. And it's actually hard to grow old gracefully in a lot of other fields. What I see of the business world is that it's a very tough dog-eat-dog place where, as you begin to lose your edge, the other dogs really begin to eat you alive. In science, you know, there are so many you can contribute. You can be a hotshot researcher. You can be a senior statesman. You can be on Academy committees, you can chair Academy committees, and you can help the field. You can be a professor, you can mentor students. You have dozens of different choices to make at different times in your career where the changing nature of your talents and skills can all come in and out as appropriate at the various times. As you begin to develop the insight and experience of the history of science, or the national scene or whatever--you know, you can contribute at all these different levels. So I think it's going to be a very exciting next twenty years. I don't have a set idea about what it's going to look like, but right now I'm enjoying still doing science.

Kellogg: Well, let me ask you to speculate a little bit on the future. Both the future of science as a whole and also for you personally, Susan. But specifically in terms of the future of science as a whole, there's been a lot of talk recently about--to coin a phrase from Jerry McGuire [1996 movie about a sports agent--ed.]--a "show-me-the-money" attitude about science. The expectation is that science shall deliver. Science shall deliver technology that makes our lives better, solutions to these deep policy problems that the nation confronts. Can you tell me a little bit about--what is your position on the expectations that are currently being put on science? What are the limits of science for solving those kinds of societal problems, and yet what do you also see as kind of the true benefit of science and why it should be supported by our society?

Solomon: That is a very tough question. Let me be specific first, and then I'll be general. When it comes to atmospheric chemistry, I think we've got really a very important mission to do. I use the word "mission" which is the way that NOAA describes itself with a certain amount of--I'm very cognizant of the fact that I work for a mission-oriented agency whose goal is to try to serve the public in the areas of the ocean and the atmosphere. And I'm privileged I think in that in the research arm of NOAA, we have a fair amount of flexibility in choosing the problems that we work on, but we try very hard to serve society. I enjoy that. I have never felt that that infringed on my scientific freedom. I don't feel that I have to be able to go into work and say to myself, "Well, maybe I'll work on black holes today." As a NOAA employee, I can't do that, but I don't mind. The only thing I want to work on is atmospheric chemistry and I'm a very narrow thinker. You know, I'm deep but narrow. And I love it, so it's fine with me to do things--I get a certain amount of satisfaction out of feeling that I can do things that are relevant. It gets back to why I chose atmospheric chemistry instead of test tube chemistry in the first place. So for me as a scientist personally, the idea of having to do things that have a goal, that are oriented around public service is absolutely consistent with the way I think about what I like about what I do. That's probably not true for everyone. And it's probably not true for all fields. Getting back to atmospheric chemistry, we've got a lot of people on this planet and we're going to have more. And they're going to be using all kinds of chemicals and putting all kinds of things in the atmosphere and trying to live in a smaller and smaller world. So I think the 21st century has got enormous opportunities for people to do atmospheric chemistry.

For people who don't think that way, you know if you want to do nuclear physics, you want to do it in the way that people do art, you know where the end product is the intellectual exercise whether or not it's of relevance. I'm sure they wouldn't feel the way I do about these sort of tendencies in our country to make things mission-oriented. As a taxpayer, personally I see tremendous value in basic research and I believe we should continue to support it. I think the level at which we've supported it in the past is very small relative to the benefits. I realize that's a personal view, and that's just my own private view, it's not meant to be any kind of bigger statement or criticism of the way other people think about it.

Kellogg: Let me just go back to another piece of it, which has to do with the policy implications of science. I think you've stated earlier that you see science as being limited in determining policy questions. That even when we know all the facts it still doesn't necessarily lead us to a simple or a single solution. Do you see science as having limits that way?

Solomon: Yes. There's very few things about which you can make a black-and-white decision based on facts, and even fewer when they involve an organization as large as our society. A lot of things come down to choices and values along with scientific fact, and you take a sort of personal example. You know, I think it's a scientific fact, a medically-proven fact that smoking will damage your health. Nevertheless, it's a personal choice whether you want to do it or not in our society and I personally believe that's appropriate. But science stops pretty early in that process, and personal values kick in, societal values kick in, economic issues kick in, morality even kicks in--is it moral to tell someone else what they can and can't do with their own body? I mean, there are a lot of parallels with that when you start talking about the applications of atmospheric chemistry. Of course, when you talk about the ozone hole, you know I can say I've been to Antarctica, I've measured the ozone go away, there is an ozone hole in Antarctica, and there is more measurable ozone depletion at mid-latitudes, a significant fraction if not all of which is related to chlorofluorocarbons. I think that's a clean and clearly justified scientific statement. Well, now, what are you going to do about it? It depends on your world view. Some people would say you should ban all chlorofluorocarbons, and for that matter, anything containing chlorine that might have even a 1% chance of being bad, even an HCFC, which is a sort of mixed blessing, because it's what helps us to get out of using the CFC's. So you can think of that as the methadone of the problem if you want to. That's one view. Another view is: well, have you clearly established that it's going to cause an ecological catastrophe? Is everyone going to die? Is there going to be widespread famine? Are there clear ecological consequences? And you can say, well, we have scientific reasons to believe that for every 1% decrease in the ozone layer, there's a 2-3% increase in skin cancer. But that's not ecological catastrophe, I suppose. Especially if it doesn't happen to you...

You begin to get into a whole range of sort of world view kinds of issues. And I've tried very hard to avoid those. You asked me earlier about this whole issue of "you become an expert and people expect you to be an expert on everything." I think it was Einstein who complained bitterly about the fact that because he was a genius, people would ask him his opinion about things he knew nothing about. And he would simply say, "I can't comment on that." And they would be shocked and disappointed. And he also wrote about the tremendous temptation to yield to the belief that these people had that he was a genius, and therefore had something to say about anything, everything. Of course, I'm not really trying to compare myself to Albert Einstein, but the issue that he grappled with is one that faces anyone who's a scientific expert on any topic. There begins to be the belief that because you know a lot about how ozone gets depleted, you should be an expert on how we deal with it. I'm not sure there are any experts on how we deal with it. It's a collective process of our society in figuring what we want to do about environmental problems. I think you do yourself a little bit of a scientific disservice when you reach beyond what you really know, to begin making expert type statements about things you're not an expert on. And I'm not saying that--well the temptation to do that can be very strong.

Kellogg: Well, let's return you to your area of expertise then, Susan. What are you looking forward to in the future? Can you tell us a little about what your current scientific pursuits are at the moment without giving anything away outside of peer-review journals?

Solomon: We have to be very careful about that, as we've discussed. I'm continuing to be fascinated with ozone depletion, and in particular, the nature of mid-latitude ozone depletion. Ozone is clearly changing at mid-latitudes as well. We know that. There are a lot of aspects to it that we don't understand nearly as well as we do the Antarctic problem, [but] I think we're getting close, though, to getting a handle on some of those. I've done some work recently on volcanoes; together with my colleague Dave Hoffman, in 1989 I wrote a paper in which I said that it's pretty clear that the chemistry we're understanding on liquid sulfate aerosols now--not the ice clouds that polar stratospheric clouds are, but liquid aerosols can in some ways be similar to what happens in the Antarctic, and if we had a major volcanic eruption, we might indeed see some major ozone loss at mid-latitudes via a process somewhat analogous to what happens in the Antarctic. I think it's fair to say that prediction was confirmed with what happened after Pinatubo, where we saw record low ozone over mid-latitudes. I've played a continuing role in explaining the details of that chemistry and looking to see how well we can simulate that quantitatively. I'm continuing to work on that, and I'm also doing some work now that has to do with understanding, again, the vertical profile of the ozone loss. I mean, you know that it's missing and you know where it's missing, and the shape of how it's changed is a very critical clue in figuring out what's causing the change. Just as it was so in Antarctica it's also that way at mid-latitudes. What we see at mid-latitudes is very interesting. The ozone depletion seems to go all the way down to the tropopause--very hard to explain with conventional chemistry. For a lot of the same reasons that the Antarctic ozone loss was hard to explain. The lower down you go, the fewer reactive species you have, and the more the chlorine should be getting tied up in the reservoirs. I've done some work recently looking at the fact that you can get cirrus clouds near the tropopause; I believe they can be liquid as well as solid at times. But those surfaces catalyze a lot of the same chemistry that you can have on Antarctic polar stratospheric clouds, and I think they are the reason why we have ozone loss going all the way down to the tropopause at mid-latitudes. So I'm continuing to work on those kinds of things. I'm also beginning to work on the way that chemistry enters into climate change. I've sort of returned to my roots in looking at ozone sonde profiles to try to understand what the tropospheric ozone distribution looks like and how tropospheric ozone is influencing the climate system. I'm starting to do more work on various aspects of climate chemistry. So those are kind of the directions I'm moving in right now. One of the things we did this past year that I'm pretty excited about is to use some seven year-old data that we took in Antarctica to look at the issue of anomalous absorption in clouds. We haven't come up with the answer, but I think we've helped to address some of the sources of missing absorption, in particular from the complexes of oxygen that I've had a lot of fun measuring for a long time and never really thought too much about their relevance to the climate system. Science is kind of like a tree. You have your major branches; as you build on those branches and work your way out, you always find connections to the work you did before. There's a trunk, which is probably ozone, and the left branch is ozone depletion, the right branch is tropospheric ozone and climate--those are the kinds of things that I enjoy working on.

Kellogg: That doesn't sound like you'll ever become one of those people who's convinced she knows it all, and refuses to listen to a good argument from one of the new young hotshots.

Solomon: Well, if I ever do, I want somebody to come up to me and slap me on the hand or something and tell me that's what I'm doing.

I think that's the key to remaining productive and active. What happens to scientists as they get older is that--it's OK, I think it's OK anyway to get more cynical about the media and about the value of serving on committees and things like that, but it's not OK to get more cynical about new ideas, and to begin to believe that everything that ever could be done has been done and that you know it all because you've been studying it for twenty years. I think there's a very real temptation to do that. Another thing that can happen is that you just get blinded by the belief that you understand something, so even in the most well-meaning way, you can believe that "Oh, you know, that was looked at ten years ago and we have that down pat, so that's not an issue anymore." That's where you miss opportunities to do interesting new things. You always have to be open to re-examining what you think you know, what you think you already know for sure. And even more importantly, being completely open to the idea of something totally different coming along, like the Antarctic ozone hole.

Kellogg: I'd like to return, though, to the subject of Antarctica. You told us everything about the field program and a lot about the science, Susan. But I'd like to know what you personally felt about that experience, at such a young age going to such an extreme part of the world.

Solomon: I think the only time you can go to Antarctica is when you're young. Because it is certainly one of the most physically challenging experiences of your life. I mean, there are people who do go down there, that have been going down there for twenty and thirty and forty years even, but most people go down there when they're relatively young. It's an uncomfortable trip, it's nine hours in a very noisy military airplane without really having seats--I mean, they have these webbed structures that you can kind of sit on. But I didn't mind any of that. I didn't even notice it because it was the most exciting, challenging, fantastic experience of my life. And I guess it gets back again to this sense of the world as physical beauty--I mean, I have never seen anything that was to me more beautiful than Antarctica. And I guess the reason for that is the untouched nature of it. It is really truly the last place on earth. It's absolutely staggeringly beautiful. The colors of twilight down there are incredible, intensely purple and blue. The polar stratospheric clouds, those same polar stratospheric clouds that deplete the ozone, are wonderful to see. They look like tiny suspended rainbows or pieces of rainbows; the particle sizes are pretty mono-dispersed so they're all the same size, that's the reason, as I understand it, that they're so pretty to look at. It's a remarkable place, and the challenge of doing the work down there was for me just tremendous. I love a challenge, and the opportunity to go down there and really challenge myself, not just in terms of what I was trying to do with the science, but also just the environment. There were times that getting to the laboratory was challenging. I've driven around in major snowstorms where I could barely see to the next flag (the way you get around in Antarctica at times is when it gets bad, the roads are all flagged and so there's a red flag on the right which means you're returning and green is away, so you can get to the point in a bad storm where you're driving from one flag to another. And they're spaced out about twenty or thirty feet apart except that of course it's very windy down there, so sometimes they fall down and--it's a pretty harrowing experience. But I enjoyed that. It was scary, but it was also fantastic.

And there was a tremendous sense of history. This is a place that we've only explored in the past century. People died in 1911 trying to get to the South Pole, walking by foot without radios. It's remarkable how far we've come so fast. You can go around and see Scott's hut and Shackleton's hut and reading the tales of adventure of the early explorers is something that I really enjoy and so I've read a lot of the biographies of people like Scott and Shackleton and Amundsen and others. You sort of begin to join a society of people who are adventurers and explorers. That kind of reminds me--there's a wonderful book by P.B. Medawar called **Advice to a Young Scientist**, and he talks about scientists as being four different kinds of people: Artists, Artisans and Mystics. It's really very well worth reading, but certainly there is an element of the explorer in my experience in Antarctica.

Kellogg: You talked about the history of Antarctica. You are now part of that history, are you not? There are a couple of topographic features that carry the name "Solomon." Could you tell us about them?

Solomon: That's probably the greatest honor actually that I think I've ever received is there's a glacier called "Solomon Glacier" and a saddle called "Solomon's Saddle" and they are both located fairly close to McMurdo Station, which is where I've done all my work down there. It's fantastic to have that.

Kellogg: So when we look at the map of Antarctica, we see the names Shackleton, Amundsen--

Solomon: Only if you get the one with the little tiny print are you going to see the name "Solomon."

Kellogg: Anything else?

Solomon: That just about covers it, I think.

Kellogg: I want to thank you, Susan. This has been extremely entertaining and informative for me, and I appreciate the opportunity to be able to do this oral history with you. Thank you.

Solomon: Thank you for asking the questions more eloquently than I could give you answers.

-END OF INTERVIEW-

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